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THE

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON,

FROM THEIR COMMENCEMENT, IN 1665, *TO THE YEAR* 1800;

Abridged,

WITH NOTES AND BIOGRAPHIC ILLUSTRATIONS,

BY

CHARLES HUTTON, LL.D. F.R.S.
GEORGE SHAW, M.D. F.R.S. F.L.S.
RICHARD PEARSON, M.D. F.S.A.

VOL. I.

FROM 1665 TO 1672.

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1809.

TO

THE RIGHT HONOURABLE

THE PRESIDENT

THE VICE PRESIDENTS

THE COUNCIL

AND THE REST OF

THE FELLOWS

OF THE

ROYAL SOCIETY OF LONDON

THIS

ABRIDGMENT

OF THE

Philosophical Transactions

IS

(With Permission)

MOST RESPECTFULLY

DEDICATED.

PREFACE.

AT this period, when science is so generally cultivated, it can scarcely be necessary to enter into a minute detail of the various instances in which the Royal Society of London has contributed to the advancement of natural and experimental knowledge, by the publication of its Memoirs, under the title of Philosophical Transactions. These volumes consist of an invaluable collection of observations and discoveries made by the most eminent philosophers of the seventeenth and eighteenth centuries; among whom may be mentioned a Boyle, a Newton, a Halley, and a Hales; to say nothing of other celebrated philosophers, who have contributed to this Collection in later days and in our own time.

It cannot then excite surprise that a work, enriched by communications from men so distinguished in the different departments of science, and which, from the variety of topics it embraces, may be consulted with equal advantage by the astronomer, the geometrician, the natural historian, the anatomist, the physiologist, and the chemist,—should be held in the highest estimation, not only in this country, but in every enlightened part of the globe. Indeed the Transactions of the Royal Society of London are justly regarded as a lasting and most honourable testimony to the genius and philosophical spirit of the British nation. No scientific library is complete without them.

Every person, therefore, who entertains the least ardour for philosophical pursuits, cannot but be desirous of possessing so valuable a work. But, at the present period, few can satisfy their wishes in this respect, in consequence of the high price and extreme scarcity of the Original Collection; a complete set of which amounts to nearly 100 volumes. Indeed the early volumes, indispensable to those who wish to trace the progress of science, are only to be *casually* procured.

To supply this want of the Original, an Abridgment was long since undertaken by Mr. Lowthorp, and after his decease it was brought down by various continuators to the middle of the last century. But in that Abridgment, made at different times by different persons, not only was the order of the Original, in the printing of the Memoirs, departed from, but a great number of papers not reprinted in other works were omitted. However, notwithstanding these objections, even that Abridgment is now scarcely to be procured.

Either, therefore, an entire re-impression, or a new Abridgment of the Philosophical Transactions, was called for. But the expences attending an entire re-impression of a work so voluminous, and containing such numerous plates, must deter every individual or association from attempting such an undertaking, as from the necessarily limited number of purchasers not even an indemnity, much less any emolument, could be obtained. Besides, many of the papers in the earlier volumes are not sufficiently important for republication. Instead, therefore, of a re-impression of the Philosophical Transactions at large, the present New Abridgment in Eighteen Volumes is offered to the public. It comprises whatever is most valuable in the Original, from its commencement to the close of the eighteenth century, together with Dr. Hooke's volume of Philosophical Collections. All the articles are presented in the same order in which they appear in the Original. The most important communications are reprinted entire, in the words of the respective authors. The less important papers are given in an abridged state, so however as to retain, it is hoped, whatever is especially curious or useful in them. Those papers which, in the Original, are printed in foreign languages, are here translated; one or two instances excepted, where, from the peculiarly delicate nature of the subject, there would have been a manifest impropriety in giving them in English. The omitted papers, of which however the titles have always been retained, are chiefly such as have been re-printed by the respective authors in separate works, still extant; the Proprietors being particularly desirous that their undertaking should not prevent those authors from deriving every possible advantage from their own publications. This circumstance has enabled the Editors to insert, at length, many valuable papers in some of the later volumes, which are not elsewhere preserved, and which would not easily admit of being abridged.

In every instance where they appeared necessary to the elucidation of the subject discussed, the figures in the Original Transactions have been re-engraved, but for the most part on a reduced scale.

Short Biographical Notices of deceased contributors to the original work, with Occasional Annotations, pointing out where the subject under consideration has been more fully investigated in some of the subsequent volumes of the Philosophical Transactions, or in other works—and, when the subject relates to Natural History, the insertion of the Linnæan generic and specific names,—constitute the accessory matter in this New Abridgment. And these additions, it is hoped, will give it some advantages over other Abridgments which have preceded it.

To give further utility to the present Abridgment, a General Index has been subjoined, by way of Appendix to the concluding volume. And that this Index may be serviceable in referring to the Original, whenever the reader may think a paper has been too much curtailed, the following plan has been adopted in the printing—viz. at the head of every leaf are affixed the number and date of the corresponding volume of the Original, and at the title of every paper the corresponding page.

On the whole, it is hoped, that this New Abridgment, on which the Proprietors have spared neither pains nor expence, will be found to contain whatever is most valuable in the Original Work;* and that to those who are necessarily engaged in philosophical pursuits in the way of their profession, as well as to those who in their hours of leisure cultivate the sciences from taste, it will, up to the period which it embraces, be found to answer all the purposes of the more costly and scarcely procurable Transactions at large.

* With the exception of certain papers before-mentioned, which have been reprinted in other works of easy access, and to which, in this Abridgment, reference has always been made.

London, April 26, 1809.

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THE CONTENTS CLASSED UNDER GENERAL HEADS.

Natural Philosophy—Acoustics, Astronomy, Hydraulics, Hydrostatics, Hydrology, Magnetics, Meteorology, Optics, Pneumatics.

Miscellanies—Agriculture, Antiquities, Architecture, Grammar, History, Music, Painting, Perspective, Sculpture, Travels, Voyages.

Anatomy, Physiology, Surgery, Medicine, Pharmacy, Chemistry, &c.

Natural History—Mineralogy, Botany, Zoology, &c.

Chronology, Geography, Mathematics, Mechanics, Navigation, &c.

Books, of which an Account is given in this Volume.

Biographical Notices of the following Authors in this Volume.

THE

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON;

ABRIDGED.

INTRODUCTION.

AFTER a short address to the Royal Society, Mr. Henry Oldenburg,* their second or under secretary, and the collector and publisher of these Transactions, adds a few lines, by way of introduction, stating the intentions, the motives, and the occasion for publishing them; viz. that it was intended, for the advancement of science and the benefit of mankind, to make known to the world, through this channel, the results of the labours, not only of those persons who were members of this Society, but also of other learned men, in this and other countries; that by the communication of such discoveries others might be stimulated and encouraged to similar exertions, in promoting and extending the various branches of natural knowledge. Mr. Oldenburg then enters immediately on the memoirs or papers themselves; which are presented in the following order:

* Mr. Oldenburg (who sometimes wrote his name Grubendol, transposing the letters) was a native of Germany, born in Bremen about the year 1626. He came to England, as consul for his countrymen, in the time of Charles the First, in which capacity he remained at London during Cromwell's usurpation. He afterwards attended some young noblemen, as tutor, to Oxford, where he became acquainted with the gentlemen who commenced the Royal Society, to which he was appointed the assistant under Dr. Wilkins, the first secretary, in which capacity he was very useful, by the extensive correspondence he held with the learned men of other countries, as well as by the arranging and publishing of the Philosophical Transactions, which he continued to do till the 136th Number inclusive, June 25, 1677; when he was succeeded in his office of secretary, as well as in publishing the Transactions, by Mr. Hook. Mr. Oldenburg was a man of considerable abilities, and was very active in promoting the views of the Society. He died at Charlton, near Blackheath, in August 1678, being only in the 52d year of his age.

An Account of CAMPANI'S *Improvement of Optic Glasses,* *N° 1, p. 2.*

The improvement of optic glasses, not long since attempted at Rome by Signor Giuseppe Campani,* is described in a book, entitled, Ragguaglio di nuove Osservationi, lately printed in the said city, in which the following particulars are contained:—

The first regards the excellence of the long telescopes, made by the said Campani, who pretends to have found a way to work great optic glasses with a turn-tool, without any mould. And it having hitherto been found by experience, that small glasses are in proportion better to see with, on the earth, than large ones; that author affirms, that his are equally good for the earth, and for making observations in the heavens. Besides, he uses three eye-glasses for his great telescopes, without finding any Iris, or such rain-bow colours, as usually appear in ordinary glasses, and prove an impediment to observations.

The second concerns the circle of Saturn, in which he has observed nothing, but what confirms M. Huygens's Zulichem system of that planet, published by that worthy gentleman in the year 1659.

The third respects Jupiter, in which Campani affirms he hath observed, by the goodness of his glasses, certain protuberances and inequalities, much greater than those that have been seen there hitherto. He adds, that he is now observing, whether those sallies in the said planet do not change their situation; which if they should be found to do, he judges that Jupiter might then be said to turn upon his axis; which, in his opinion, would serve much to confirm the opinion of Copernicus. Besides this, he affirms, he has remarked in the belts of Jupiter, the shadows of his satellites, and followed them, and at length seen them emerge out of his disk.

* There were two brothers (Matthew and Joseph) of this name, Campani, residing in Rome in those times, both very eminent for their mechanical contrivances of optical and philosophical instruments, and for several treatises on such subjects; particularly relating to pendulums, or time pieces, for the longitude; also to spectacles, and to telescopes, of which it is said they made the very best and largest in their time; being then as famous for their refracting telescopes, as Herschel is now for his large reflectors. It is said it was by their telescopes that the first Cassini made his best observations and chief discoveries in the heavens; and they still show at Paris, among the astronomical antiquities, several of Campani's object glasses, of eight, ten, and twelve inches diameter.—Other circumstances relating to their telescopes may be seen in several parts of these Transactions; as in N^os 4, 8, 12, &c.

A Spot in one of the Belts of Jupiter. By Mr. Hook. *N°* 1, *p.* 3.

The ingenious Mr. Hook * did, some months since, intimate to a friend of his, that he had, with an excellent twelve-foot telescope, observed, some days before he then spoke of it, (viz. on the 9th of May 1664, about nine o'clock at night) a small spot in the largest of the three obscurer belts of Jupiter; and that, observing it from time to time, he found, that within two hours after, the said spot had moved from east to west, about half the length of the diameter of Jupiter.

The Motion of the Comet Anno 1664, *predicted by M.* Auzout.† *N°* 1, *p.* 3.

The motion of comets was hitherto thought so irregular, as not to be reducible to any laws, and astronomers had always contented themselves with observing exactly the places through which they passed, and where they ceased to appear;

* Mr. Robert Hook who became celebrated as a philosopher and mathematician, was born in 1635 in the Isle of Wight. Having a natural taste for painting, he became the pupil of Sir Peter Lely, but soon quitted this pursuit, the oil-colours being injurious to his health. He was then placed under the tuition of Dr. Busby, from whom he acquired a knowledge of the languages. About the year 1653 he went to Christ-church, Oxford, and became a member of the philosophical society of learned men then associating in that university. He there assisted Dr. Willis in his chemical operations; and afterwards became assistant to Mr. Boyle. In 1662 he was appointed curator of experiments to the Royal Society, and in 1664 professor of mechanics to that learned body; at the same time he was also elected professor of geometry in Gresham college. After the fire of London he produced a plan of his own for rebuilding the city, which procured him the appointment of one of the city surveyors, though his plan was not carried into effect. In 1668 he had a dispute with Hevelius respecting telescopic sights, which he supported with such warmth as to give great offence to his scientific friends. In 1671 he attacked Sir Isaac Newton's Theory of Light and Colours; and when that philosopher's Principia came out, Hook pretended that the discovery concerning the force and action of gravity was his own, which occasioned that patient man to feel some just resentment against him. In 1677 he succeeded Mr. Oldenburg as secretary to the Royal Society. In 1691 archbishop Tillotson created him M. D. by warrant. He died in 1702. Mr. Hook was author of several valuable works, besides many curious papers in the Phil. Trans. A posthumous vol. of his writings appeared in folio, 1705. He was a man of great mechanical genius, and the sciences are highly indebted to him for several valuable instruments and improvements. It is said he was rather deformed in his person, of a penurious disposition, and extremely jealous of his reputation as an original discoverer.

† M. Adrian Auzout, a French mathematician and astronomer, of some reputation, was born at Rouen, and died in 1691. It is said he invented the micrometer; and wrote a treatise on that instrument, published in 1693. It is also said he was the first person who applied the telescope to astronomical quadrants.

till M. Auzout first attempted to foretel the line of motion of the comet Anno 1664; exhibiting an ephemeris, in which he determines for every day its place in the heavens, the hour of its coming to the meridian, and that of its setting, until its too great distance, or its approach to the sun, should hide it from our eyes. This ephemeris is founded on the supposition of its moving justly enough in the plane of a great circle, inclined to the equinoctial about 30°, and to the ecliptic about 49° or $49\frac{1}{2}°$, cutting the equator at about $45\frac{1}{2}°$, and the ecliptic at 28° of Aries, or a little more. Then M. Auzout proceeds to show how the motion of this comet is to be traced on the globe, and to calculate the several places of its appearance in the heavens; and in particular, he finds by his calculations what the least distance of the comet from the earth should be, when it is in opposition to the sun; a circumstance that may serve, as he thinks, to decide the grand question concerning the motion of the earth.

An Experimental History of Cold. By the Hon. ROBERT BOYLE,* *N° 1, p. 8.*

The chief heads of this work are,

1. Experiments touching bodies capable of freezing others.
2. Experiments and observations touching bodies disposed to be frozen.
3. Experiments touching bodies indisposed to be frozen.
4. Experiments and observations touching the degrees of cold in several bodies.
5. Experiments touching the tendency of cold upwards or downwards.
6. Experiments and observations touching the preservation and destruction of eggs, apples, and other bodies by cold.
7. Experiments touching the expansion of water and aqueous liquors by freezing.
8. Experiments touching the contraction of liquors by cold.
9. Experiments in consort, touching the bubbles, from which the levity of ice is supposed to proceed.
10. Experiments about the measure of the expansion and the contraction of liquors by cold.

* The Honourable Robert Boyle, son of Richard earl of Cork, was born Jan. 1627, and died Dec. 1691. He was one of the first founders of the Philosophical Society, and the Royal Society, to which he continued, through the whole course of a long and active life, one of the most useful of its members, by his numerous and valuable communications, and other services. A more ample account of the life and labours of this very celebrated man will be given hereafter in the detached volume of memoirs.

11. Experiments touching the expansive force of freezing water.

12. Experiments touching a new way of estimating the expansive force of congelation, and of highly compressing air without engines.

13. Experiments and observations touching the sphere of activity of cold.

14. Experiments touching differing mediums through which cold may be diffused.

15. Experiments and observations touching ice.

16. Experiments and observations touching the duration of ice and snow, and the destroying of them by air, and several liquors.

17. Considerations and experiments touching the *primum frigidum.*

18. Experiments and observations touching the coldness and temperature of the air.

19. Of the strange effects of cold.

20. Experiments touching the weight of bodies frozen and unfrozen.

21. Promiscuous experiments and observations concerning cold.

An Account of a monstrous Calf. By the Hon. ROBERT BOYLE. *N*[os] *1 and 2, pp. 10 and 20.*

This monstrous production was found in the uterus of a cow, killed by a butcher at Limington, in Hampshire. Its hinder legs had no joints: its feet were parted, so as to resemble the claws of a dog, [No. 2, p. 20]: and its tongue was triple, one to each side of the mouth, and one in the middle. Between the fore legs and hind legs was a great stone,* on which the calf rode, weighing 20 lbs. The outside of the stone was of a greenish colour, but, some small parts being broken off, it appeared a perfect free-stone.

In the further account, inserted in the second number, it is added, that the surface of the stone was full of little cavities, and that when broken, it exhibited a great number of small pebble stones of an oval figure. Its colour (internally) was grayish, like free-stone, but intermixed with veins of yellow and black.

* As this remarkable concretion was not subjected to chemical analysis, its real nature must remain unknown. It might be a deposition of osseous matter, (phosphate of lime); but it is more probable that it was similar in its composition to the urinary calculi of herbivorous quadrupeds, and that it was formed either from the water of the allantois or of the amnios.

Of a peculiar Lead Ore of Germany, and the Use of it. N° 1, p. 10.*

Among several minerals lately sent from Germany was a kind of lead-ore, more considerable than all the rest, because of its singular use for essays upon the cuppel, seeing that there is not any other metal mixed with it. It is found in the Upper Palatinate, at a place called Freyung, and there are two sorts of it, one of which is a kind of crystalline stone, and almost all good lead; the other not so rich, and more farinaceous. By the information, coming along with it, they are fetched, not from under the ground, but, the mines of that place having lain long neglected, by reason of the wars of Germany and the increase of waters, the people, living thereabout, take it from what their forefathers had thrown away, and had lain long in the open air.

Of an Hungarian Bolus, of the same [medicinal] Effect with the Bolus Armenus. Anonymous. N° 1, p. 11.

The bolar earths were formerly in high repute as medicines, but they are rarely prescribed by modern physicians. In whatever part of the globe they are found, their effects upon the human body are the same.

Of the New American Whale-Fishing about Bermudas. By a Seaman. N° 1, p. 11.

Though many attempts at mastering the whales of these seas had been unsuccessful, by reason of their extraordinary fierceness and swiftness, yet it had been lately undertaken; and fit persons being out at sea seventeen times, and fastening their weapons a dozen times, killed two old female whales, and three cubs. The length of one of the old ones from the head to the extremity of the tail was 88 feet; its tail 23 feet broad, the swimming fin 26 feet long, and the gills three feet long, with large bends underneath from the nose to the navel; on her after-part, a fin on the back, and the inside inlaid with fat like the caul of a hog. The other old one was about 60 feet long. Of the cubs, one was 33, the other two were about 25 or 26 feet long. Their shape was very sharp behind, like the ridge of a house; the head pretty bluff, and full of bumps on both sides; the back perfectly black, and the belly white. Their swiftness and force are surprising: one of them, that had been struck, towed the boat after him for six or seven leagues in three quarters of an hour. When wounded they make a hideous roaring, at which all the whales within hearing flock to the

* The spathose lead ores are common in many parts of Germany, and are, in general, remarkably rich.

place, yet without striking or doing any harm. These whales are supposed to resemble that species called jubartes; they are without teeth, and longer than the Greenland whales, but not so thick. Their feeding on grass, growing at the bottom of the sea, appeared by cutting up the great bag or maw, in which was found about two or three hogsheads of a greenish grassy matter. The largest sort of these whales might afford seven or eight tuns of oil. The cubs yield little, and that a kind of jelly only. The oil of the old ones congeals like hog's lard, yet burns well. The oil of the blubber is as clear and fair as whey; that boiled out of the lean interlarded, hardens like tallow, sputtering in the burning; and that made of the caul resembles hog's lard.

A Narrative concerning the Success of Pendulum Watches at Sea for the Longitude. By Major HOLMES. *N° 1, p.* 13.

The relation lately made by Major Holmes concerning the success of the pendulum watches at sea, two of which were committed to his care and observation in his last voyage to Guinea, by some of our eminent virtuosi and grand promoters of navigation, is as follows:—

The Major having left that coast, and being come to the isle of St. Thomas under the Line, he adjusted his watches, put to sea, and sailed westward, seven or eight hundred leagues, without changing his course; after which, finding the wind favourable, he steered towards the coast of Africa, N. N. E. but having sailed upon that line about two or three hundred leagues, the masters of the other ships, under his conduct, apprehending that they should want water, before they could reach that coast, did propose to him to steer their course to the Barbadoes, to supply themselves with water there. The Major having called the master and pilots together, and caused them to produce their journals and calculations, it was found that those pilots differed from the Major in their reckonings, one of them eighty leagues, another about a hundred, and the third more; but the Major judging by his pendulum watches, that they were only some thirty leagues distant from the isle of Fuego, which is one of the isles of Cape Verde, and that they might reach it next day, and having a great confidence in the said watches, resolved to steer their course thither; and having given order so to do, they got the very next day about noon a sight of the said isle of Fuego, finding themselves to sail directly upon it, and so arrived at it that afternoon, as he had said.

These watches having been first invented by the excellent M. Huygens, and fitted to go at sea by the Right Honourable the Earl of Kincardin, both fellows of the Royal Society, are now brought to a wonderful perfection. The said M. Huygens, having been informed of the success of the experiment made by Major Holmes, wrote to a friend at Paris a letter to this effect:—

"Major Holmes at his return has made a relation concerning the usefulness of pendulums, which surpasses my expectation: I did not imagine that the watches of this first structure would succeed so well, and I had reserved my main hopes for the new ones. But, seeing that those have already served so successfully, and that the others are yet more just and exact, I have the more reason to believe that the finding of the longitude will be brought to perfection. In the mean time I shall tell you, that the States did receive my proposition, when I desired of them a patent for these new watches, and the recompense set apart for the invention in case of success; and that without any difficulty they have granted my request, commanding me to bring one of these watches into their assembly, to explicate unto them the invention, and the application thereof to the longitude; which I have done to their contentment."

The Character of M. DE FERMAT, Counsellor of Parliament at Tholouse, lately deceased. N° 1, p. 15.

This excellent person died in 1663. He was a general scholar and a man of universal genius; cultivating jurisprudence, poetry, and mathematics, but especially the latter, for his amusement. Fermat was author of several learned works, on subjects relative to which he maintained a correspondence with the most learned men of his time, with whom he was justly placed among those of the first rank, both for genius and acquirements.

Many particulars of his life and writings may be seen more at large, in several books published on biography, &c.

Extract of a Letter, lately written from Rome, touching the late Comet, and a New one. By Signior JOHN DOMINICI CASSINI. N° 2, p. 17.*

I cannot enough wonder at the strange agreement of the thoughts of that acute French gentlemen, Monsieur Auzout, in the Hypothesis of

* John Dominic Cassini, a celebrated astronomer, was born in Piedmont 1625, and educated among the jesuits at Genoa. He had such a turn for Latin poetry, that some of his compositions were printed when he was no more than eleven years old. He afterwards devoted himself wholly to mathematics, particularly astronomy, and in 1650 he was appointed professor of mathematics at Bologna. In 1652 he made an accurate observation of a comet which then appeared; and he determined geometrically the apogee and eccentricity of a planet, from its true and mean place; a problem which Kepler

the comet's motion, with mine; and particularly at that of the tables. I have, with the same method by which I find the motion of this comet, easily found the principle of that author's Ephemerides, which he then thought not fit to declare; and it is this, that this comet moves about the Great Dog, in so large a circle, that that portion, which is described, is exceedingly small in respect of the whole circumference of it, and hardly distinguishable by us from a straight line.

Concerning the new comet you mention, I saw it on the 11th of February, about the 24th degree of Aries, with a northern latitude of 24° 40′. The cloudy weather has not yet permitted me to see it in Andromeda, as others affirm to have done.

Extract of a Letter, written from Paris, containing some Reflections on part of the preceding Letter. By M. Auzout. N° 2, p. 18.

As to the hypothesis of Geov. Dominic Cassini, touching the motion of the comet about the Great Dog in a circle, whose centre is in a straight line drawn from the earth through the said star, I believe it will shortly be published in print, as a thought that occurred to me in discoursing with one of my friends, who maintained that it turned about a centre, because its perigee had been over against the Great Dog, as I had noted in my Ephemerides. This particular I long since declared to many of my acquaintance. I have added an observation, which I find that Signior Cassini has not made, viz. that there was ground to think, that the comet of 1652 was the same with the present, seeing that, besides the parity of the swiftness of its motion, the perigee of it was also over against the Great Dog. But, to state what grounds I had for these thoughts, I said, that if they were true, the comet must needs accomplish its revolution from ten to twelve years, or thereabouts. But, seeing it appears not by history that a comet has been seen at those determinate distances of time,

had pronounced was impossible to be solved. In 1663 he was appointed inspector-general of the fortifications of the castle of Urbino, and superintendant of the rivers in the ecclesiastical state. His astronomical pursuits, however, were still continued with the greatest diligence, and many important discoveries were the reward of his industry. In 1666 he printed at Rome a collection of astronomical pieces, and among others a Theory of Jupiter's Satellites. Lewis XIV. of France desired leave of the pope for Cassini to come to Paris, which was granted in 1669; but the time of his stay was limited to six years. At the expiration of the term he was commanded to return; and, on his refusal, his places were taken from him. Cassini was the first professor of the royal observatory at Paris, which was finished in 1670. Here he made numerous observations, and in 1684 he discovered four satellites of Saturn. In 1695 he went to Italy to examine the meridian line which he had settled in 1655. In 1700 he continued, through France, that meridian line which had been begun by Picard. He died in 1712, in the 87th year of his age.

nor that, over against the perigee of all the other comets of which particular observations are recorded, are always found stars of the first magnitude, or such others as are very notable, besides other reasons that might be alleged, I shall not pursue this speculation; but rather suggest what I have taken notice of in my reflections upon former comets, which is, that more of them enter into our system by the sign of Libra and about Spica virginis, than by all the other parts of the heavens. For both the present comet, and many others registered in history, have entered that way, and consequently passed out of it by the sign Aries; by which also many have entered.

I have observed the position of the comet since January 28, every day, when the weather permitted, viz. January 29, February 3, 6, 10, 17, 19, 24, 26, 27, and March 6, 7, 8. I left it on March 8, at the 18° of the horn of Aries, almost in the same latitude; and I am apt to believe, it will be eclipsed; which I wish I may be able to observe this evening, if it be not already passed.

On the Mines of Mercury in Friuli; and a Way of producing Wind by the Fall of Water. By Dr. WALTER POPE. *N° 2, p. 21.*

The mines of mercury in Friuli, a territory belonging to the Venetians, are about a day's journey and a half distant from Goritia northwards, at a place called Idria, situated in a valley of the Julian Alps. They have been, as I am informed, these 160 years in the possession of the emperor, and all the inhabitants speak the Sclavonian tongue. In going thither, we travelled several hours in the best wood I ever saw before or since, being very full of firs, oaks, and beeches, of an extraordinary thickness, straightness, and height. The town is built, as usually towns in the Alps are, all of wood, the church only excepted, and another house wherein the overseer lives. When I was there, in August last, the valley, and the mountains too, out of which the mercury was dug, were of as pleasant a verdure as if it had been in the midst of spring, which they there attribute to the moistness of the mercury; how truly, I dispute not. That mine which we went into, the best and largest of them all, was dedicated to Saint Barbara, as the other mines are to other saints: the depth of it was 125 paces, every pace of that country being, as they informed us, more than five of our feet. There are two ways down to it; the shortest perpendicular way is that whereby they bring up the mineral in great buckets, and by which oftentimes some of the workmen come up and down. The other, which is the usual way, is at the beginning not difficult, the descent not being much; the greatest trouble is, that in several places you cannot stand upright: but this holds not long, before you come to descend in earnest by perpendicular ladders, where the weight of one's body is found very sensibly. At the end of each ladder there are boards across, where we may breathe a little. The ladders, as we said, are perpendicular, but, being imagined produced, do not make one ladder, but several parallel ones. Being at the bottom, we saw no more than what we saw before, only the place whence the mineral came. All the way down, and the bottom, where there are several lanes cut out in the mountain, is lined and propt with great pieces of fir-trees, as thick as they can be set. They dig the mineral with pickaxes, following the veins. It is for the most part hard as a stone, but more weighty; of a liver-colour, or that of crocus metallorum. I hope shortly to show you some of it. There is also some soft earth, in which you plainly see the mercury in little particles. Besides this, there are oftentimes found in the mines round stones like flints, of several sizes, very like those globes of hair, which I have often seen in England, taken out of the bellies of oxen. There are also several marcasites and stones which seem to have specks of gold in them; but upon trial, they say, they find none in them. These round stones are some of them very ponderous, and well impregnated with mercury; others light, having little or none in them. The manner of getting the mercury is this: They take of the earth, brought up in buckets, and put into a sieve, whose bottom is made of wires at so great a distance that you may put your finger betwixt them: it is carried to a stream of running water, and washed as long as any thing will pass through the sieve. That earth which passeth not, is laid aside upon another heap: that which passeth, reserved in the hole, and taken up again by the second man, and so on, to about ten or twelve sieves gradually less. It often happens in the first hole, where the second man takes up his earth, that there is mercury at the bottom; but towards the farther end, where the intervals of the wires are less, it is found in very great proportion. The earth laid aside is pounded, and the same operation repeated. The fine small earth, that remains after this, and out of which they can wash no more mercury, is put into iron retorts and stopped, because it should not fall into the receivers, to which they are luted. The fire forces the mercury into the receivers: the officer unluted several of them to show us; I observed in all of them, that he first poured out perfect mercury, and after that came a black dust, which being wetted with water, discovered itself to be mercury, as the other was. They take the caput mortuum and pound it, and renew the operation as long as they can get any mercury out of it.

We saw there a man, who had not been in the mines for above half a year before, so full of mercury, that putting a piece of brass in his mouth, or rubbing it in his fingers, it immediately became white like silver; I mean he produced the same effect as if he had rubbed mercury upon it; and was so paralytic, that he could not, with both his hands, carry a glass half full of wine to his mouth without spilling it, though he loved it too well to throw it away.

The Blowing of Fire by the Fall of Water. By the same. N° 2, p. 25.

In the brass works of Tivoli near Rome, the water blows the fire, not by moving bellows, but by affording the wind. See fig. 1, pl. 1; where A is the stream; B the fall of it; C the trunk into which it falls; L G a pipe; G the orifice of the pipe, or nose of the bellows; G K the hearth; E a hole in the pipe; F a stopper to the hole; D a place under ground, by which the water runs away. Upon stopping the hole E, there is a perpetual strong wind issuing forth at G. But G being stopped, the wind rushes out so vehemently at E, that it will support a ball playing above the hole there.

An Extract of a Letter, containing some Observations, made in the Ordering of Silk-Worms, communicated by Mr. DUDLEY PALMER, from Mr. EDWARD DIGGES. N° 2, p. 26.

I herewith offer to your Society a small parcel of my Virginian silk. What I have observed in the ordering of silk-worms, contrary to the received opinion, is:

1. That I have kept leaves 24 hours after they are gathered, and flung water upon them to keep them from withering; yet when (without wiping the leaves) I fed the worms, I observed, they did as well as those fresh gathered.

2. I never observed that the smell of tobacco, or smells that are rank, did any ways annoy the worms.

3. Our country of Virginia is very much subject to thunders: and it has thundered exceedingly when I have had worms of all sorts, some newly hatched, some half way in their feeding; others spinning their silk; yet I found none of them concerned in the thunder, but kept to their business, as if there had been no such thing.

4. I have made many bottoms of the brooms (in which hundreds of worms spun) of holly; and the prickles were so far from hurting them, that even from those prickles they first began to make their bottoms.

[See further on the ordering of Silk-worms in No. 5 hereafter.]

An Account of Micrographia, or the Physiological Descriptions of Minute Bodies, made by magnifying Glasses. By Mr. ROBERT HOOK. *N° 2, p. 27.*

Hook's *Micrographia, or Physiological Descriptions of Minute Bodies,* still maintains a deserved reputation; many of the figures are a kind of standard representations, from which most succeeding authors on similar subjects have copied. Among the most excellent, though not altogether free from minute faults, are those of the common mite, the flea, the louse, the gnat, and the ant. The figures were all drawn by his own hand. A new edition of this work, with abbreviated descriptions, was published in the year 1745. In this edition the part relative to the *Baroscope* the *Hygroscope, the engine for grinding optic glasses,* &c. is omitted.

Some Observations and Experiments upon May-Dew. By Mr.* THOMAS HENSHAW. *N° 3, p. 33.*

That ingenious gentleman, Mr. Thomas Henshaw, having had occasion to make use of a great quantity of May-dew, did, by several casual essays on that subject, make the following observations and trials, and present them to the Royal Society.

It appears from this paper that dew, far from being a pure or unadulterated water, is in reality of a more mixed nature than most others.

Dew newly gathered and filtered through a clean linen cloth, though it be not very clear, is of a yellowish colour, somewhat approaching to that of urine. In moderate quantities, it does not easily putrify, though kept for a long time; but in large quantities, as of four or five gallons, it putrifies, and deposits a black sediment.

Mr. Henshaw having several tubs with a good quantity of dew in them set to putrify, and being about to pour out of one of them to make use of it, found in the water a great bunch, larger than his fist, of those insects commonly called hog-lice or millepedes,† tangled together by their long tails, one

* What is commonly called May-dew is a sweet but excrementitious liquor discharged from the bodies of small insects infesting the leaves of plants, and known to naturalists by the name of Aphides. They are called by the French pucerons.

† The insects here improperly called millepedes appear rather to have been the larvæ or maggots either of the musca pendula or tenax. The other insects which Mr. H. observed, were the larvæ or maggots of the common gnat, &c. The most important part of the paper is the concluding paragraph, demonstrating that a great quantity of saline matter is contained in the dew. The precise nature of this saline matter remains yet to be ascertained, and is unquestionably a subject which well deserves to be investigated by modern chemists.

of which came out of every one of their bodies, about the thickness of a horse-hair: the insects all lived and moved after they were taken out.

Evaporating away great quantities of his putrified dew in glass basons, and other earthen glazed vessels, he at last obtained, as he remembers, above two pound of grayish earth, which when he had washed with more of the same dew out of all his basons into one, and evaporated to dryness, lay in leaves one above another, not unlike to some kind of brown paper, but very friable.

That taking this earth out, and after he had well ground it on a marble, and given it a smart fire, in a coated retort of glass, it soon melted, and became a cake in the bottom when it was cold, and looked as if it had been salt and brimstone in a certain proportion melted together; but, as he remembers, was not at all inflammable. This ground again on a marble, he says, turned spring water of a reddish purple colour.

That by often calcining and filtering this earth, he at last extracted about two ounces of a fine small white salt, which, examined through a good microscope, seemed to have sides and angles in the same number and figure as rochpetre, (rock-salt.)

The Motion of the second Comet predicted, by the same Gentleman (M. Auzout) who predicted that of the former. N° 3, p. 36.

M. Auzout here observes, first in general, that the circumstances of this second comet are contrary to those of the former, in almost every particular: such as, that the *former* moved very swift, but this *latter* rather slow: *that* contrary to the order of the signs, from east to west; but *this* following them, from west to east: *that,* from south to north; *this,* from north to south, so far as observed: *that,* on the side opposite to the sun; *this,* on the same side: *that,* in its perigee at the time of its opposition; *this,* out of the time of its conjunction: that both the body and train of this latter were much more bright and vivid than the former.

After this he descends to particulars, stating that he began to observe this comet April 2, 1665, which he continued but a few days following; from which he infers, that the line hitherto described by it resembles a great circle, as it is found in all other comets in the middle of their course. He finds the said circle inclined to the ecliptic about 36° 30′; and the nodes, where it cuts it, near the beginning of Gemini and Sagittary: that it declines from the equator about 26°, and cuts it towards the 11°; and consequently that its greatest latitude must have been near Pisces, about March 24; and its greatest declination, towards the 25° of the equator, which will fall about April 11. He places its perigee, March 27, in about 15° of Pisces, a little more westerly than Marchab, or the wing of Pegasus; and that it would be in conjunction with the sun April 9. But he concludes by recommending farther observations, to settle these particulars more accurately.

The Advice given by Monsieur Petit,* *Intendant of the Fortifications of Normandy, touching the Conjunction of the Ocean and Mediterranean. N° 3, p. 41.*

This intelligent gentleman, Monsieur Petit, having been consulted, touching the conjunction of the Ocean and Mediterranean, delivers first the proposition, and then gives his thoughts upon it.

Having stated the proposition, or the business to be effected, the artist remarks that the data, or circumstances given him, are not sufficient for determining the question, which therefore he declines for the present. The paper then concludes as follows:—

This artist having thus prudently waved this proposition, diverts himself with reflecting upon several others of the like nature; among which he insists chiefly upon two, whereof one is that so much celebrated in Egypt; the other, of Germany. And he is of opinion, that the most important of all is that of conjoining the Red Sea by the Nile with the Mediterranean, which he looks upon as the most excellent conveniency to go into the East Indies, without doubling the Cape of Good Hope; and yet it could not be executed by those great kings of Egypt, who raised so many stupendous pyramids; although in his opinion the reasons alleged by historians to justify them, for having abandoned that undertaking, are of no validity; and that the Red Sea cannot be, as they feared, higher than the Nile, and therefore cannot endanger the inundation of Egypt.

The other proposition was made to Charlemagne, anno 793, for joining the Euxine Sea and the Ocean together, by a channel, which was begun for that end, and designed to be 2000 paces long, and 100 paces broad, between the river Altmull, falling into the Danube above Ratisbon, and the river Rott, passing at Nuremberg, and thence running into the Maine, and so into the Rhine. But yet this also proved abortive, though at first there was great appearance of success.

* Peter Petit, a French mathematician, was born at Montluçon, about 1598. By the favour of Cardinal Richelieu he was made engineer to the king, upon whose concerns he was sent into Italy.—He was the author of several works on physical and astronomical subjects, and died in 1677.

Of the Way of killing Rattle-snakes. By Capt. SILAS TAYLOR. N° 3, p. 43.

There being not long since occasion given at a meeting of the Royal Society to discourse on rattle-snakes, that worthy and inquisitive gentleman, Captain Silas Taylor, related the manner how they were killed in Virginia, which he afterwards was pleased to give in writing, attested by two credible persons in whose presence it was done; which is as follows:—

The wild penny-royal* or dittany of Virginia, grows straight up about one foot high, with the leaves like penny-royal, with little blue tufts at the joining of the branches to the plant, the colour of the leaves being a reddish green, but the water distilled, of the colour of brandy, of a fair yellow: the leaves of it bruised are very hot and biting upon the tongue: and of these, so bruised, they took some, and having tied them in the cleft of a long stick, they held them to the nose of the rattle-snake, who by turning and wriggling, laboured as much as she could to avoid it: but she was killed with it in less than half an hour, and, as was supposed, by the scent thereof; which was done in the year 1657, in the month of July, at which season they repute those creatures to be in the greatest vigour for their poison.

The same gentleman afterwards affirmed, that in those places where the wild penny-royal or dittany grows, no rattle-snakes are observed to come.

A Relation of Persons killed by subterraneous Damps.† By Sir ROBERT MORAY. N° 3, p. 44.

This relation sets forth that seven or eight men and one woman were instantaneously suffocated by going into the waste in an old coal-pit full of the damp.

* The plant here mentioned is, perhaps, some species of tencrium or satureia, or, as some have supposed, of cunila. Many other plants have been extolled for their supposed powers, as antidotes to the poison of the rattle-snake. The most celebrated is the *polygala senega*, which, however, is only understood to act as an antidote when taken internally, as well as applied externally. The relation in the present paper certainly borders on the marvellous, and has accordingly been fixed upon as a proper subject of ridicule by Sir John Hill in his "*Review of the Royal Society*."

† What is called the *damp*, or *choak damp*, in these and other mines, is fixed air or carbonic acid gas. It is of the same nature with the deleterious gas of the dogs grotto (grotta de' Cani) near Naples. Being heavier than common air, it occupies the *lower part* of these subterraneous places. If a lighted candle becomes quickly and wholly extinguished, when let down into these excavations, experienced miners will not venture to work in them. In the *upper parts* of coal pits, and other mines, there is often another kind of gas, called the *fire damp*, which is inflammable air, or hydrogen gas; the sudden explosion of which has sometimes destroyed those who were employed about these pits, as will be seen by the narratives that will be inserted in the subsequent volumes of this Abridgement.

Of the Mineral of Liege, yielding both Brimstone and Vitriol, and the Method of extracting them out of it, as used at Liege. By Sir* Robert Moray. *N° 3, p. 45.*

The mineral, out of which both brimstone and vitriol are extracted, is not much unlike lead ore, and has often much lead mingled with it, which is separated by picking it from the rest. The mines resemble our English coal-mines, dug, according to the depth of the mineral, 15, 20, or more fathoms. To make brimstone, they break the ore into small pieces, and put it into earthen crucibles five feet long, square and pyramid-wise. The entry is near a foot square. These crucibles are laid sloping, eight undermost and seven above them, (as it were between them) that the fire may come at them all, each having its particular furnace. The melted brimstone drops out at the small end of the crucible, and falls into a leaden trough (common to all the said crucibles), through which there runs a continual stream of cold water, conveyed thither by pipes, for the purpose of cooling the fused sulphur, which is usually four hours in melting. After this distillation of the sulphur, the ashes (or residuum), being scraped out of the crucibles, are employed (mixed with other lixiviated ashes) to make copperas or vitriol; for which purpose they are thrown into a square planked pit in the earth, about four feet deep and eight feet square, and are covered with water. In this they remain twenty-four hours, or until an egg will swim upon the liquor, which is a sign that it is strong enough. When this liquor is to be boiled, they let it run through pipes into the kettles, adding to it half as much mother-water, *i. e.* the water which was left in previous crystallizations of the vitriol. The kettles are made of lead, four feet and a half high, six feet long, and three feet broad, and are placed upon iron grates. In these the liquor is boiled with a strong coal fire twenty-four hours or more, till it acquires a proper consistence; when the fire is taken away, and the liquor, after being suffered to cool a little, is let out through holes made in the sides of the kettles, and conveyed through wooden conduits into several reservoirs three feet deep.

Account of Mr. Boyle's *Experimental History of Cold. N° 3, p. 46.*

He shews,

1. That not only all sorts of acid and alcalizate salts, and spirits, even spirit of wine, but also sugar, and sugar of lead mixed with snow, are capable of freezing other bodies, and upon what account they are so.

* This mineral is a pyrites, and the process here described for extracting sulphur and vitriol (sulphate of iron) from it, is used to this day.

2. That among the substances capable of being frozen, there are not only all gross sorts of saline bodies, but such also as are freed from their grosser parts, not excepting spirit of urine, the lixivium of pot-ashes, nor oil of tartar, *per deliquium*, itself.

3. That many very spirituous liquors, freed from their aqueous parts, cannot be brought to freeze, neither naturally, nor artificially: And here is occasionally mentioned a way of keeping moats unpassable in very cold countries, recorded by Olaus Magnus.

4. What are the ways proper to estimate the greater or less coldness of bodies; and by what means we can measure the intenseness of cold produced by art, beyond that which nature needs to employ for the freezing of water; as also, in what proportion water of a moderate degree of coldness will be made to shrink by snow and salt, before it begin by congelation to expand itself; and then, how to measure, by the differing weight and density of the same portion of water, what change was produced in it, betwixt the hottest time of summer, and first glaciating degree of cold; and then the highest which our author could produce by art: Where an inquiry is annexed, whether the making of these kinds of trials, with the waters of the particular rivers and seas men are to sail on, may afford any useful estimate whether or not, and how much, ships may on those waters be safely loaden more in winter than in summer? To which is added the way of making exact discoveries of the different degrees of coldness in different regions, by such thermometers as are not subject to the alterations of the atmosphere's gravitation, nor to be frozen.

5. Whether in cold, the diffusion from cold bodies be made more strongly downwards, contrary to that of hot bodies: Where is delivered a way of freezing liquors without danger of breaking the vessel, by making them begin to freeze at the bottom, not the top.

6. Whether that tradition be true, that if frozen apples or eggs be thawed near the fire, they will be thereby spoiled, but if immersed in cold water, the internal cold will be drawn out, as is supposed, by the external cold; and the frozen bodies will be harmlessly thawed. Item, Whether iron, or other metals, glass, stone, cheese, &c. exposed to the freezing air, or kept in snow, or salt, upon the immersing them in water will produce any ice. Item, What use may be made of what happens in the different ways of thawing eggs and apples, by applying the observation to other bodies, and even to men, dangerously nipped by excessive cold. Where is added not only a memorable relation, how the whole body of a man was successfully thawed and cased all over with ice, by being treated as frozen eggs and apples are; but also the luciferousness of such experiments, as these: and likewise, what the effects of cold may be, as to the

conservation or destruction of the textures of bodies: and in particular, how meat and drink may be kept good, in very cold countries, by keeping it under water, without glaciation. As also, how in extreme cold countries, the bodies of dead men and other animals may be preserved many years entire and unputrified: And yet, how such bodies, when unfrozen, will appear quite vitiated by the excessive cold. Where it is further inquired into, whether some plants, and other medicinal things, that have specific virtues, will lose them by being thoroughly congealed and (several ways) thawed? And also, whether frozen and thawed hartshorn will yield the same quantity and strength of salt and saline spirit, as when unfrozen? Item, Whether the electrical faculty of amber, and the attractive or directive virtue of loadstones, will be either impaired, or any ways altered by intense cold? This head is concluded by some considerable remarks touching the operation of cold upon bones, steel, brass, wood, bricks.

7. What bodies are expanded by being frozen, and how that expansion is evinced: And whether it is caused by the intrusion of air: As also, whether what is contained in icy bubbles is true and elastic air, or not.

8. What bodies they are, that are contracted by cold; and how that contraction is evinced. Where it is inquired, whether chymical oils will, by congelation be like expressed oils, contracted; or, like aqueous liquors, expanded?

9. What are the ways of measuring the quantity of the expansion and contraction of liquors by cold. And how the author's account of this matter agrees with what navigators into cold climates mention from experience, touching pieces of ice as high as the masts of their ships, and yet the depth of these pieces seems not at all answerable to what it may be supposed.

10. How strong the expansion of freezing water is. Where are enumerated the several sorts of vessels which, being filled with water and exposed to the cold air, do burst; and where also the weight is expressed, that will be removed by the expansive force of freezing. Whereunto an inquiry is subjoined, whence the prodigious force observed in water, expanded by glaciation, should proceed? And whether this phenomenon may be solved, either by the Cartesian, or Epicurean hypothesis?

11. What is the sphere of activity of cold, or the space to whose extremities every way the action of a cold body is able to reach: where the difficulty of determining these limits, together with the causes thereof, being with much circumspection mentioned, it is observed, that the sphere of activity of cold is exceeding narrow, not only in comparison of that of heat in fire, but in comparison of, as it were, the atmosphere of many odorous bodies; and even in comparison of the sphere of activity of the more vigorous loadstones, insomuch,

that the author has doubted, whether the sense could discern a cold body, otherwise then by immediate contact. Where several experiments are delivered for the examining of this matter, together with a curious relation of the way used in Persia, though a very hot climate, to furnish their conservatories with solid pieces of ice of a considerable thickness: To which is added an observation, how far in earth and water the frost will pierce downwards, and upon what accounts the depth of the frost may vary. After which, the care is inculcated that must be had in examining, whether cold may be diffused through all mediums indefinitely, not to make the trials with mediums of too great thickness: where it is made to appear, that cold is able to operate through metalline vessels, which is confirmed by a very pretty experiment of making icy cups to drink in; the way of which is accurately set down. Then are related the trials, whether, or how, cold will be diffused through a medium that some would think a vacuum, and which to others would seem much less disposed to assist the diffusion of cold than common air itself. After which follows a curious experiment, shewing whether a cold body can operate through a medium actually hot, and having its heat continually renewed by a fountain of heat.

12. How to estimate the solidity of the body of ice, or how strong is the mutual adhesion of its parts; and whether differing degrees of cold may not vary the degree of the compactness of ice. And our author having proceeded as far as he was able towards the bringing the strength of ice to some estimate by several experiments, he communicates the information he could get about this matter, among the descriptions that are given us of cold regions: and then he relates, out of seamen's journals, their observations touching the insipidness of resolved ice made of sea-water; and the prodigious magnitude of it, extending even to the height of two hundred and forty feet above water, and the length of above eight leagues. To which he adds some promiscuous, but very notable observations, concerning ice, not so readily reducible to the foregoing heads: viz. Of the blue colour of rocky pieces of ice; and the horrid noise made by the breaking of ice, like that of thunder and earthquakes, together with a consideration of the cause whence those loud ruptures may proceed.

13. How ice and snow may be made to last long; and what liquor dissolves ice sooner than others, and in what proportion of quickness the solutions in the several liquors are made: where occasion is offered to the author to examine, whether motion will impart a heat to ice? After which, he relates an experiment of heating a cold liquor with ice, made by himself in the presence of a great and learned nobleman, and his lady, who found the glass, wherein the liquor was, so hot that they could not endure to hold it in their hands. Next it is examined, whether the effects of cold do continually depend upon the actual presence and

influence of the manifest efficient causes, as the light of the air depends upon the sun or fire, or other luminous bodies? To this is annexed an account of the Italian way of making conservatories of ice and snow, as the author had received it from that ingenious and polite gentleman, Mr. J. Evelyn.

It shall only be intimated for a conclusion, that the author has annexed to this treatise, an examen of M. Hobbs's doctrine touching cold; wherein the grand cause of cold and its effects is assigned to wind, insomuch that it is affirmed, that almost any ventilation and stirring of the air doth refrigerate.

Extraordinary Tides in the West-Isles of Scotland. By Sir ROBERT MORAY, *N° 4, p. 53.*

In that tract of isles, on the west of Scotland, called by the inhabitants the Long Island, (from being about 100 miles long from north to south) there is a multitude of small islands, situated in a fretum, or frith, amongst which there is one called Berneray, three miles long, and more than a mile broad, the length running from east to west, as the Frith lies. At the east end of this island I observed a very strange reciprocation of the flux and reflux of the sea; and I was told of another no less remarkable.

Upon the west side of the Long Island, the tides which came from the south-west run along the coast northward; so that during the ordinary course of the tides, the flood runs east in the Frith where Berneray lies, and the ebb west. And thus the sea ebbs and flows orderly about four days before the full moon and change, and as long after. But for four days before the quarter moons, and as long after, there is constantly a great and singular variation. For then (a southerly moon making there the full sea) the course of the tide being eastward, when it begins to flow, which is about 9½ of the clock, it not only continues so till about 3½ in the afternoon, when it is high water, but after it begins to ebb, the current runs on still eastward, during the whole ebb; so that it runs eastward 12 hours together, that is, all day long, from about 9½ in the morning till about 9½ at night. But then, when the night-tide begins to flow, the current turns, and runs westward all night, during both flood and ebb, for about 12 hours more, as it did eastward the day before. And thus the reciprocations continue, one flood and ebb running 12 hours eastward, and another 12 hours westward, till four days before the new and full moon; and then they resume their ordinary regular course as before, running east during the six hours of flood, and west during the six of ebb.

I was also informed that there is yet another irregularity in the tides, which never fails, and is no less extraordinary than what I have been mentioning; which is, That whereas between th vernale and autumnal equinoxes, that is, for six months together, the course of irregular tides about the quarter moons, is, to run all day, that is, twelve hours, as from about $9\frac{1}{2}$ to $9\frac{1}{2}$, or $10\frac{1}{2}$ to $10\frac{1}{2}$, &c. eastward, and all night, that is, twelve hours more, westward; but during the other six months, from the autumnal to the vernal equinox, the current runs all day westward, and all night eastward.

This, though I had not the opportunity to be an eye-witness as of the other, I do not at all doubt, having received so credible information of it.

M. Auzout's Opinion respecting the Apertures of Object Glasses, and their relative Proportions, with the several Lengths of Telescopes. N° 4, p. 55.

This author observing in a small French tract, lately written by him to one of his countrymen, that large optic glasses have hardly ever so great an aperture as small ones, in proportion to what they magnify, and that consequently they must be more dim; takes occasion to inform the reader, that he has discovered that the apertures, which optic glasses can bear with distinctness, are in about the subduplicate proportion of their lengths; of which he tells us he intends to give the reason and demonstration in his Dioptrics, which he is now writing. In the mean time, he presents the reader with a table of such apertures, as follows:

A Table of the Apertures of Object-Glasses.

Lengths of glasses.	Apertures for			Lengths of glasses.	Apertures for			Lengths of glasses.	Apertures for		
	Excellent.	Good.	Ordinary.		Excellent.	Good.	Ordinary.		Excellent.	Good.	Ordinary.
ft. inch.	inch. lin.	inch. lin.	inch. lin.	feet.	inch. lin.	inch. lin.	inch. lin.	feet.	inch. lin.	inch. lin.	inch. lin.
0 4	0 4	0 4	0 3	9	1 11	1 9	1 5	60	5 2	4 6	3 8
0 6	0 5	0 5	0 4	10	2 1	1 10	1 6	65	5 4	4 8	3 10
0 9	0 7	0 6	0 5	12	2 4	2 0	1 8	70	5 7	4 10	4 0
1 0	0 8	0 7	0 6	14	2 6	2 2	1 9	75	5 9	5 0	4 2
1 6	0 9	0 8	0 7	16	2 8	2 4	1 11	80	5 11	5 2	4 5
2 0	0 11	0 10	0 8	18	2 10	2 6	2 1	90	6 4	5 6	4 7
2 6	1 0	0 11	0 9	20	3 0	2 7	2 2	100	6 8	5 9	4 10
3 0	1 1	1 0	0 10	25	3 4	2 10	2 4	120	7 5	6 5	5 3
3 6	1 2	1 1	0 11	30	3 8	3 2	2 7	150	8 0	7 0	5 11
4 0	1 4	1 2	1 0	35	4 0	3 4	2 10	200	9 6	8 0	6 9
4 6	1 5	1 3	1 0	40	4 3	3 7	3 0	250	10 6	9 2	7 8
5 0	1 6	1 4	1 1	45	4 6	3 10	3 2	300	11 6	10 0	8 5
6 0	1 7	1 5	1 2	50	4 9	4 0	3 4	350	12 6	11 9	9 0
7 0	1 9	1 6	1 3	55	5 0	4 3	3 6	400	13 4	11 6	9 8
8 0	1 10	1 8	1 4								

M. Auzout's Remarks on Mr. Hook's new Instrument for grinding Optic Glasses; with Mr. Hook's Reply. N° 4, pp. 56 and 63.

These two long papers contain a liberal controversy between these gentlemen, concerning the powers and apertures of telescopes, with their methods of grinding the glasses; now of very little importance, and especially as the papers only give account of two books, which are in the hands of the learned, viz. Hook's Micrographia, and Auzout's Reply to part of it.

Mr. Hook having, in that book, described a new engine for grinding optic glasses of very great lengths, M. Auzout, in a small French tract, states several difficulties and objections to it. He also thinks it impracticable to make any glasses of above 300 or 400 feet at most, fearing that neither matter nor art will be able to go even so far, and which consequently will be very far from showing us either plants or animals in the moon, as some had pretended. He farther proposes remedies for some of the inconveniences of the turning-engine. To all which Mr. Hook here replies, answering the objections, and rejecting the proposed expedients.

To illuminate an Object in any Proportion desired: and of the Distances requisite to burn Bodies by the Sun. By M. Auzout, N° 4, p. 68.

One of the means used by Mr. Auzout to enlighten an object in any proportion desired, is by some large object-glass, by him called a planetary one, because by that he shows the difference of light which all the planets receive from the sun, by making use of several apertures proportionate to their distance from the sun, provided that for every nine feet draught, or thereabouts, one inch of aperture be given for the earth. Doing this, one sees (says he) that the light which Mercury receives is far enough from being able to burn bodies, and yet that the same light is strong enough in Saturn to see clear there, since it appears greater in Saturn than it doth upon our earth when it is overcast with clouds; which, he adds, would scarcely be believed if by means of this glass it did not sensibly appear so. Of which he promises to speak more fully in his Treatise on the usefulness of great Optic Glasses; where also he intends to deliver several experiments, touching the quantity of light which is necessary to burn bodies; he having found, that not abating the light which is reflected by the surfaces of the glass, about fifty times as much light would be necessary as we have here for burning black bodies; and near nine times more for burning white bodies than for burning black ones; and so observing the intermediate proportions between these two, for burning bodies of other colours. Whence he has drawn some consequences, touching the distance at which we may hope to burn bodies here by the means of great glasses and great looking glasses. So that we must yet be seven times nearer the sun than we are to be in danger of being burned by it. Where he mentions, that having given instructions to certain persons gone to travel in hot countries, he has, among other particulars, recommended to them to try, by means of large burning glasses, with how much less aperture they will burn there than here, to know from thence whether there be more light there than here; and how much; since this perhaps may be the only means of trying it, supposing the same matters be used: although the difference of the air already heated, both in hot countries and in the planets that are nearer than we, may alter, if not the quantity of light, at least that of the heat found there.

A further Account, touching Signior CAMPANI'S *Book and Performances about Optic Glasses. By M.* AUZOUT. *N° 4, p.* 69.

In the above-mentioned French tract there is also contained M. Auzout's opinion of what he had found new in the Treatise of Signior Campani, which was spoken of in the first papers of these Transactions, concerning both the effect of the telescopes, contrived after a peculiar way by the said Campani at Rome, and his new observations of Saturn and Jupiter, made with them.

First, therefore, after M. Auzout had raised some scruple against the contrivance of Signior Campani for making great optic glasses without moulds, by the means of a turn-lath, he examines the observations made with such glasses: where, having commended Campani's sincerity in relating what he thought he saw in Saturn, without accommodating it to M. Huygens's hypothesis, he affirms, that supposing there be a ring about Saturn, Signior Campani could not see, in all those different times in which he observed it, the same appearances which he notes to have actually seen. For, having seen it sometimes in trine aspect with the sun, and oriental; sometimes in the same aspect, but occidental; sometimes in sextile aspect, and occidental; at another time again in trine, and oriental; this author cannot conceive how Saturn could in all these different times have a difference in its phasis, or keep always the same shadow; seeing that, according to the hypothesis of the ring, when it was oriental, it must cast the shadow upon the left side of the ring beneath, without casting any on the right side; and when it was occidental, it could not but cast it on the right side beneath, and nothing of it on the other.

M. Auzout believes, that he was one of the first that well observed this shadow of Saturn's body upon his ring; which he affirms happened two years since.

He confesses that he has not had the opportunity of observing Saturn in his oriental quadrat; yet he doubts not, but that the shadow appears on the left side, considering that the existence of the ring can be no longer doubted of after so many observations of the shadow cast by Saturn's body upon it, according as it must happen, following that hypothesis; there being no reason why it should cast the said shadow on one side and not on the other.

Concerning the observation of Jupiter and his satellites, the famous astronomer of Bononia, Cassini, having published, that on the 30th day of July 1664, at 2½ of the clock in the morning, he had observed, with Campani's glasses, that there passed through the broad obscure belt of Jupiter two obscurer spots, by him esteemed to be the shadows of the satellites, moving between Jupiter and the sun, and eclipsing him, and emerging from the western limb. This author first conceived that they were not shadows, but some sallies, or prominencies in that belt; but having been since informed of all the observations made by Cassini and Campani with the new glasses, and seen his figure, he candidly and publicly wishes that he had not spoken of that sally, or prominency; avowing that he can no longer doubt, but that it was the shadow of the satellite between Jupiter and the sun. He hopes also, that in time the shadow of Saturn's moon will be seen upon Saturn, although we have yet some years to stay for it, and to prepare also for better glasses.

From this curious observation, he infers the proportion of the diameter of the satellites to that of Jupiter: and judges, that no longer doubt can be made that these four satellites or moons turn about Jupiter, as our moon turns about the earth, and in the same way as the rest of the celestial bodies of our system move: whence also a strong conjecture may be made, that Saturn's moon turns likewise about Saturn.

Hence he also takes occasion to intimate, that we need not scruple to conclude, that if these two planets have moons wheeling about them, as our earth has one that moves about it, the conformity of these moons with our moon proves the conformity of our earth with those planets, which carrying away their moons with themselves, turn about the sun, and very probably make their moons turn about them in turning themselves about their axis; and also, that there is no cause to invent perplexed and incredible hypotheses, for the receding from this analogy, since, says he, if this be truth, the reason for not publishing this doctrine, which was formerly considered an innovation, will be done away, as one of the most zealous doctors of the contrary opinion has given cause to hope.

But to return to the matter in hand, this author, upon all these observations and relations of Cassini and Campani, finds no reason to doubt any longer of the superior excellency of the glass used by them, to his own, except this difference may be imputed to that of the air, or of the eyes.

Signior CAMPANI'S *Answer: and M.* AUZOUT'S *Animadversions upon it.* *N° 4, p. 74.*

The other part of this French tract, containing Campani's answer, and M. Auzout's reflections upon it, begins with the pretended shadows of the ring upon Saturn, and of Saturn upon the ring. Concerning which, the said Campani declares that he never believed them to be shadows made by the ring upon the disk of Saturn, or by the body of Saturn upon the ring, but the rims of these bodies, which being unequally luminous, did show these appearances. In which explication, forasmuch as it represents that the said Campani meant to note only the inequality of the light, which, he says, his glasses discovered, M. Auzout does so far acquiesce, that he only wishes that his own glasses would show him those differences.

Next, to the objection made by M. Auzout against Signior Campani, touching the proportion of the length of the ring to its breadth, Campani replies, that the glasses of M. Auzout show not all the particulars that his do, and therefore are unfit for determining the true figure and breadth of the apparent ellipsis of the ring. To which M. Auzout rejoins, that he is displeased at his being destitute of better glasses, but that it will be very hard in future to convince Campani, touching the proportion of the ring, seeing that the breadth of the ellipsis is always diminishing; although, if the declination of the ring remains always the same, one can at all times know which may have been its greatest breadth.

Further, to M. Auzout's change of opinion, and believing that the advance or sally seen by him in Jupiter, was the shadow of one of his moons, Campani declares, that he would not have him guilty of that change. Upon which M. Auzout wonders, why Campani then has not marked it in his figure; and would gladly know whether that sally be more easy to discover than the shadows of the satellites, which Campani believes Auzout has not seen; and whether he be assured, that those obscure parts, which he there distinguishes, do not change: for if they should not change, then Jupiter would not turn about his axis, which yet, he says, it does according to the observation made by Mr. Hook, May 9, 1665, inserted in the first papers of these Transactions. The full discovery of which particular also he makes to be a part of Cassini's and Campani's work, seeing that they so distinctly see the inequalities in the belts, and also sometimes other spots besides the shadows of the satellites: where he exhorts all the curious, that have an opportunity of observing, to endeavour the discovery of a matter of that importance, which would prove one of the greatest analogies for the earth's motion.

Adits and Mines wrought at Liege without Air-shafts. By Sir ROBERT MORAY. *N° 5, p. 79.*

Amongst the expedients that have been devised to remedy the inconvenience from the want of fresh air, there is one practised in the coal-mines near the town of Liege, or Luyck, that seems preferable to all others for efficacy, ease, and cheapness; the description of which is as follows:—

At the mouth or entry of the adit there is a structure raised of brick, like a chimney, about 28 or 30 feet high. At the bottom, two opposite sides are, or may be, 5½ feet broad; and the other two, five feet: the wall 1½ brick thick. At the lower part of it is an aperture nine or ten inches square, for taking out the ashes; which when done, this ash-hole is immediately stopped so close, that no air can possibly get in. About three feet above ground, or more, there is on the side that is next to the adit or pit, a square hole of eight or nine inches every way, by which the air enters to make the fire burn: Into this hole there is fixed a square tube or pipe of wood, the joints and chinks of which are so stopped with parchment, pasted or glued on them, that the air can have no admittance but at the end: And this pipe is always lengthened as the adit or pit advances, by fitting on new pipes, so that one end is always thrust into the other, and the joints and chinks still carefully cemented and stopped as before. So the pipe or tube being still carried on, as near as is necessary, to the wall or place where fresh air is requisite, the fire within the chimney always draws the air through the tube; there is thus a constant supply of air, which by its motion carries away with it all noxious vapours; so that men there breathe as safely as in the open air; and not only candles but fire burns, when upon occasion there is need of it for breaking the rock.

That there may be no want of such fresh air the fire must always be kept burning in the chimney: For which purpose there must be two iron grates or chimnies, that when any accident befals the one the other may be ready to supply its place; the coals being first well kindled in it: but when the fire is near spent, the chimney or grate, being haled up to the door, is to be supplied with fresh fuel.

The higher the shaft of the chimney is, the fire draws the air the better. And this invention may be used in the pits or shafts that are perpendicular, or any how inclining, when fresh air is wanted at the bottom, or in case of noxious fumes or vapours.

The whole contrivance is farther illustrated by the following references to fig. 2, pl. 1. Where,

A, Is the hole for taking out the ashes.

B, The square-hole, into which the tube or pipe for conveying the air is fixed.

C, The border or ledge, of brick or iron, on which the iron-grate or cradle, that holds the burning coals, is to rest; the one being exactly fitted for the other.

D, The hole where the cradle is set.

E, The wooden tube, through which the air is conveyed to the cradle.

F, The door, by which the grate or cradle is let in; which is to be set eight or ten feet higher than the hole D, the shutter being made of iron, or wood that will not shrink, that it may shut very close.

G, The grate or cradle, which is narrower below than above, that the ashes may the more easily fall, and the air excite the fire; the bottom being barred as the sides.

H, The border or ledge of the cradle, that rests on the ledge C.

I, Four chains of iron fastened to the four corners of the cradle, for taking of it up, and letting it down.

K, The chain of iron, to which the other chains are fastened.

L, The pulley, of iron or brass, through which the chain passes.

M, A hook, on which the end of the chain is fastened by a ring, the hook being fixed in the side of the door.

N, A bar of iron in the walls, to which the pulley is fastened.

M. Du Son's Method of breaking Rocks. Communicated by Sir Robert Moray. N° 5, p. 82.

The invention for breaking, with ease and dispatch, hard rocks is useful on several occasions; as in cutting adits, or passages through them, for draining water out of mines of lead, tin, or any other kind of minerals whatever.

The mine or adit is to be made seven or eight feet high; which though it seem to make more work downwards, yet will be found necessary for making the better dispatch, by rendering the invention more effectual.

The tool employed in this operation is of iron, well steeled at the end, which cuts the rock, of the shape in fig. 3, pl. 1, about 20 or 22 inches long, and $2\frac{1}{2}$ inches diameter at the steeled end; the rest being somewhat more slender. The steeled end is so shaped, as makes it most apt to pierce the rock, the angles at that end being always the more obtuse, as the rock is harder. This tool being held by the hand, in the place where the hole is to be made, which is usually in the middle between the sides of the rock, but as near the bottom as may be, it is to be struck upon with a hammer, the heavier the better, either suspended by a shaft turning upon a pin, or otherwise, so as one man may manage the hammer, while another holds the tool or piercer. After the stroke of the hammer, the point is to be turned a little, so that the edges or angles at the point may all strike upon a new place; by which means small chips will at every stroke be broken off, which must from time to time be taken out, as need requires. And thus the work is continued, till the hole be 18 or 20 inches deep; the deeper the better. To this hole a kind of double wedge is to be exactly fitted, as appears in fig. 4, each piece being 12 or 13 inches long, and so made, as being placed in their due position, they may make up a cylinder, cut diagonal-wise. The two flat sides that are contiguous, are to be greased or oiled, that the one may slip the more easily upon the other; and one of them, which is to be uppermost, having at the larger end a hollow crease or groove cut into it round about, for fastening to it, with a thread, a cartridge full of gunpowder, about half a pound or more as occasion may require. This wedge must have a hole drilled through the length of it, to be filled with priming powder, for firing the charge in the cartridge.

The wedge being first thrust into the hole with the cartridge, the round side where the priming-hole is being uppermost, the other wedge is to be thrust in, home to the due position, observing, that they both fit the hole in the rock exactly. Then, on the end of the lower wedge, which is to be about an inch longer than that of the upper, and flattened, priming powder is to be laid, and a piece of burning match, or thread dipt in brimstone, or other such prepared combustible matter, fastened to it, that may burn so long before it fire the powder that the workman may have time enough to retire, having first placed a piece of wood or iron so, that one end thereof being set against the end of the lower wedge, and the other against the side wall, that it cannot slip. When the powder takes fire it first drives out the uppermost wedge, as far as it will go, but by the slanting figure of it, the farther it goes backward the thicker it grows, till at last it can go no farther; then the inflamed powder tears the rock to get forth, and so cracks and breaks it all about, that at one time a vast deal of it will either be quite blown out, or so cracked and broken, as will make it easy to be removed.

Observables upon a monstrous Head. By Mr. Boyle. *N° 5, p.* 85.

In this monstrous head of a colt, the two eyes were united into one double eye, which was placed in the middle of the brow, the nose being wanting, which should have separated them. Hence the two orbits were united into one very large round hole, into which there entered from the brain one pretty large optic nerve. The tunica sclerotica which contained the united eyes was one and the same, but appeared to have a seam (by which the junction was formed) going quite round the ball, the anterior pellucid part of which was distinctly separated into two corneæ by a white seam. Each cornea had a distinct iris and pupil; and on opening the eye there were found two crystalline humours. The other parts could not be well distinguished, as the eye had been much bruised by handling.

Observables in the Body of the Earl of Balcarras. Anonymous. N° 5, p. 86.

The appearances in the heart of this nobleman were very remarkable. On opening the pericardium, none of that liquor was found which (in a healthy state) lubricates the heart. The external surface of the heart from its base to its apex was very rough. Being cut asunder, a quantity of white and inspissated fluid ran out; and beneath the base, between the right and left ventricle, two stones were found, one as big as an almond, the other two inches long and one broad, having three auricles or crisped angles. In the orifice of the right ventricle there was a fleshy fattish matter. The whole body was bloodless, emaciated, and of a black and bluish colour.*

Of the designed Progress to be made in the Breeding of Silkworms, and the Making of Silk, in France. N° 5, p. 87.

The French King Henry the Fourth having made a general establishment all over France for planting and propagating of mulberry-trees, and for breeding silkworms, in order to set up and entertain a silk trade there; and having prospered so well in that design, that in many parts of his dominions great stores of such trees were raised, and multitudes of silkworks propagated, to the great benefit of the French people, forasmuch as it was a considerable beginning to avoid the transport of several millions abroad for buying of silks, and withal an excellent means of well-employing abundance of poor orphans and widows, and many old, lame, and other indigent and helpless people; the present French king has lately revived and seconded that undertaking, by giving express order that it should be promoted by all possible means, and

* It is to be regretted that this account of the appearances on dissection was not accompanied with a history of the symptoms. The disease appears to have been an inflammation (followed by suppuration) of the heart, excited by the irritation arising from the large calculi formed within it. The "white inspissated fluid," seems to have been pus; and the "fleshy fattish matter," a polypus.

particularly in the metropolis of that kingdom, and round about it; and that for that end the whole way concerning that work and trade should be fully and punctually communicated in print; which has also been executed by one Monsieur Isnard, in a treatise published at Paris, in French, entitled, Instructions for the Planting of White Mulberries, the Breeding of Silkworms, and the Ordering of Silk in Paris, and the circumjacent Places; in which book, the method being represented, which that great prince Henry IV. used in establishing the said work and trade, together with the success thereof, and the advantages thence derived to his subjects, the author, from his own experience and long practice, delivers (and seems to do it candidly) all what belongs in this business in four main heads. First, he teaches the means of sowing, planting, and raising white mulberries (as the foundation of silkworks) showing how many several ways that may be done. Secondly, The breeding of silkworms, the choosing of good eggs, and their hatching, as also the feeding of the worms, and preserving them from sickness, and curing them of it, together with the way of making them spin to best advantage. Thirdly, The manner of winding their silk from their bottoms, adding the scheme of the instrument serving for that purpose. Fourthly, The way of keeping silkworms' eggs for the ensuing year.

Through the whole book are scattered many not inconsiderable particulars, though perhaps known to most. The white mulberry-tree, as it is in other qualities preferable to the black, so this author esteems it the best, not only for the durableness of the wood, and its large extent of usefulness in carpenters' and joiners' work; but also for the fitness of its leaves (besides their principal use for the food of silkworms) to fatten sheep, goats, cows, and hogs, only by boiling and mingling them with bran. The berries themselves he commends as very excellent to fatten poultry, and to make them lay eggs plentifully.

Silkworms, before they begin to spin, and about the latter end of their feeding, must, says the author, be often changed, and have air enough, by opening the windows of the room they are in, if it be not too severe weather; else, says he, the silk that is in their belly will cause so extraordinary a heat in them, that it will burn their guts, and sometimes burst them; and the same (being a substance that resembles gum or Burgundy pitch) will putrefy and turn into a yellowish matter.

As for their working, he gives this account of it: that the first day they make only a web; the second, they form in this web their cases, and cover themselves all over with silk; the third day, they are no longer seen; and the days following they thicken their cases, always by one end or thread, which they never break off themselves. This, he affirms, they put out with so much quickness, and draw it so subtle and so long, that, without an hyperbole, the end or thread of every case may have two leagues in length.* He advertises, that they must by no means be interrupted in their work, to the end, that all the silk they have in their bellies may come out.

Some eight days after they have finished their work, as many of the best cases as are to serve for seed, viz. the first done, the hardest, the reddest and best coloured, must be chosen, and put apart; and all diligence is to be used to wind off the silk with as much speed as may be, especially if the worms have nimbly dispatched their work.

Here he spends a good part of his book in giving very particular instructions concerning the way of winding off the silk, setting also down the form of the oven and instruments necessary for that work, which is the painfulest and nicest of all the rest.

Touching their generation, he prescribes that there be chosen as many male as female cases (which are discerned by this, that the males are more pointed at both ends of the cases, and the females more obtuse on the ends, and bigger-bellied), and that care be had, that no cases be taken, but such wherein the worms are heard rolling; which done, and they being come forth in the form of butterflies, having four wings, six feet, two horns, and two very black eyes, and put in a convenient place, the males fluttering with their wings, will join and couple with the females, after that these have first purged themselves of a kind of reddish humour: they are to be left from morning (which is the ordinary time of their coming forth) till evening, and then the females are to be gently pulled away, whereupon they will lay their eggs; but the males are then thrown away as useless.

Enquiries concerning Agriculture. Nº 5, *p.* 91.

The Royal Society, in prosecuting the improvements in natural knowledge, have it in design to collect histories of nature and arts; and for that purpose have already, according to the several inclinations and studies of their members, divided themselves into divers committees, to execute the said design. Those gentlemen who constitute the committee for agriculture, and the history and

* Monsieur Isnard's computation seems rather to border on the extravagant. The length indeed may be supposed to differ considerably in the different silk-balls, but, in general, will be found far short of that stated by Isnard. According to Boyle, as quoted by Derham, a lady, on making the experiment, found the length of a ball to be considerably more than three hundred yards, though the weight was only two grains and a half. The Abbe Le Pluche informs us, that of two balls one measured 924 feet, and the other 930. It may be proper to add, that the silk throughout its whole length is double, or composed of two conjoined or agglutinated filaments.

improvement of it, began their work with drawing up certain heads of inquiries, to be distributed to persons experienced in husbandry all over England, Scotland, and Ireland, for procuring faithful and solid information of the knowledge and practice now employed in these kingdoms; and to consider what improvements may be further made in this whole matter. They have accordingly proposed the inquiries following:

1. *For Arable Grounds.*

1. The several kinds of soils in England, being supposed to be either sandy, gravelly, stony, clayey, chalky, light-mould, heathy, marish, boggy, fenny, or cold weeping ground: what is the soil of each country, and how prepared for arable?

2. What peculiar preparation each soil undergoes for each kind of grain; with what kind of manure they are prepared; when, how, and in what quantity it is laid on?

3. At what seasons and how often they are ploughed; and what kind of ploughs are used for several sorts of ground?

4. How long the several grounds are let lie fallow?

5. How, and for what productions, heathy grounds may be improved?

6. What ground has marl? how deep commonly it lies from the surface? what is the depth of the marl itself? what the colour of it? upon what grounds it is used? what time of the year it is to be laid on? how many loads to an acre? what grains marled land will bear; and how many years together? how such marled land is to be used afterwards, &c.?

7. The kinds of grain or seed, usual in England, being either wheat, miscelane, rye, barley, oats, peas, beans, fitches, buck-wheat, hemp, flax, rape; what sorts of these are sown in each county, and how prepared for sowing? whether by steeping, and in what kind of liquor; or by mixing it, and with what?

8. There being many sorts of wheat, as the white or red lammas, the bearded Kentish wheat, the gray wheat, the red or gray pollard, the ducks-bill wheat, the red-eared bearded wheat, &c. And so of oats, as the common black, blue, naked, bearded in North-Wales, and the like of barley, peas, beans, &c. The inquiry is, which of these grow in each country, and in what soil; which of them thrive best there, and whether each of them require a peculiar tillage, and how they differ in goodness?

9. What are the chief particulars observable in the choice of seed-corn, and all kinds of grain; and what kinds of grain are most proper to succeed each other?

10. What quantity of each kind is sown on the statute acre? And in what season of the moon and year?

11. With what instruments they harrow, clod and rowl, and at what seasons?

12. How much an acre of good corn generally yields in very good, in less good, and in the worst years?

13. What are the causes and remedies of mildew, blasting, and smut; being some of the common diseases of corn in this country?

14. How weeds, worms, flies, birds, mice, moles, &c. are prevented.

15. Upon what occasions young corn is cut or fed in the blade; and what are the benefits thereof?

16. What are the seasons and ways of reaping and ordering each sort of grain, before it be carried off the ground?

17. What are the several ways of preserving grain in the straw, within and without doors, from all annoyance, as mice, heating, rain, &c.

18. What are the several ways of separating the different sorts of grain from the straw, and of dressing them?

19. What are the ways of preserving any stores of grain from the annoyances they are liable to?

2. For Meadows.

1. How the above-mentioned sorts of soil are prepared, when they are used for pasture or meadow?

2. How are prevented the common annoyances of these pasture or meadow grounds, as weeds, moss, sourgrass, heath, fern, bushes, briars, brambles, broom, rushes, sedges, gorse or furzes?

3. What are the best ways of draining marshes, bogs, fens, &c.?

4. What are the several kinds of grass, and which are accounted the best?

5. What are the chief circumstances observable in the cutting of grass; and what in the making and preserving of hay?

6. What kind of grass is fittest to be preserved for winter feeding? And what grass is best for sheep, for cows, oxen, horses, goats, &c.

Account of the Burning Concave Glass, made at Lyons by M. Villette, and compared with several others made formerly. N° 6, *p.* 95.

The following is the account of this glass, communicated in some letters from Paris.

Concerning the efficacy of M. de Villette's burning glass, all that P. Bertot has written of it, is true. We have seen its effects frequently repeated in the morning, noon, and afternoon, always performing very powerfully; burning or melting any matters, very few excepted. Its figure is round, being rather above thirty inches in diameter. On one side it has a circular frame of steel, that it may keep its just measure. It is easy to remove it from place to place, though it weigh above a hundred weight, and is easily put in all sorts of positions. The focus is distant from the centre of the glass about three feet, and is about the size of half a louis d'or. One may pass one's hand through it, if it be done nimbly; but if it remain there for one second only, there is danger of receiving much hurt.

Green wood takes fire in it in an instant, as do also many other bodies.

	Seconds.
A small piece of pot-iron was melted, and ready to drop down, in....	40
A silver piece of 15 pence was pierced in........................	24
A thick nail (called *le clou de paisan)* was melted in..............	30
The end of a sword-blade of olinde was burnt in..................	43
A brass counter was pierced in..................................	6
A piece of red copper was melted, ready to drop down in..........	42
A piece of a chamber quarry-stone was vitrified in................	45
Watch spring steel melted in.....................................	9
A mineral stone, such as is used in harquebusses *à rouët*, was calcined and vitrified in..	1 *just.*
A piece of mortar was vitrified in..............................	52

In short, there is hardly any body which is not destroyed by this fire. To melt by it any great quantity of metal much time would be required, the action of burning not being performed but within the size of the focus; so that usually only small pieces are exposed to it. M. d'Alibert bought it for 1500 livres.

This information has caused the following further particulars to be communicated from Paris:—

I see by some letters that you incline to believe that the glasses of Maginus and Septalius approach to that of Lyons. But I can assure you they come very far short of it. You may consult Maginus's book, where he describes his; and there are some persons here who have seen one of his best, which had but about twenty inches diameter: so that this of Lyons must perform at least twice as much. As to Septalius, we expect the account of it from intelligent and impartial men. It cannot well be compared to that of Lyons, unless in bigness; and in this case, if it have five palms, as you say, that would be about $3\frac{1}{2}$ feet French; so that would be again a foot larger in diameter, which would make it as much again greater in surface. But its effects, as it burns so far off, cannot be very violent. And I have heard one say, who had seen it, that it did not set wood on fire but after the time of saying a *miserere*. You may judge of the difference of the effects, since that of Lyons gathers its beams together within the space of seven or eight lines; and that of Septalius must scatter them in the compass of three inches.

Of the Optic Glasses of M. Hevelius and M. Huygens; and other Improvements in Telescopes. N° 6, p. 98.

That eminent astronomer of Dantzic, M. Hevelius,* writes to his correspondent in London as follows:—

What has been done in the grinding of optic glasses in your parts, and how those beginnings, mentioned by you formerly, do continue and succeed, I am very desirous of hearing. It is now above ten years since I myself invented a peculiar way of grinding such glasses, and reduced it to practice; by which it is easy, without any considerable danger of failing, to make and polish optic glasses of any conic section, and, what is of most consequence, in any dish of any section of a sphere. This invention I have as yet discovered to none, my purpose being, for the improvement of natural knowledge, to describe the whole method in my celestial machine, and to propose it to the examination and judgment of the Royal Society; not doubting at all but they will find the way true and practicable, myself having already made several glasses by it, which many learned men have seen and tried.

Monsieur Huygens inquiring also in a letter, newly written by him to a friend of his in England, of the success of the attempts made by an ingenious Englishman for perfecting such glasses, and urging the prosecution of the same, so as to show, by the effects, the practicability of the invention, says, that he intends very shortly to try something of that kind, of the success of which he has good hopes.

M. du Son, that excellent mechanician, is also now employing himself in London, to bring telescopes to perfection, by grinding glasses of a parabolical figure; by means of which he hopes to enable the curious to discover more by a tube of about one foot long, furnished with glasses thus figured, than can be done by any other tubes of many times that length.

* John Hevelius was born at Dantzic in 1611, and died there in 1687. His education was liberal and his studies extensive, but mathematics were his chief pursuit. He built an observatory for the purpose of astronomical observations, the result of which he published in 1647, under the title of *Selenographia, sive Lunæ descriptio, &c.* His *Cometographia,* which appeared in 1668, occasioned a controversy between himself and Dr. Hook, on this question, "Whether distances and altitudes could be taken with plain sights nearer than to a minute?" This dispute was carried on with great warmth and considerable enmity. The principal of his numerous works is that entitled *Machina Cœlestis.*

Of a Method of making more lively Representations of Nature in Wax than are extant in Painting: And of a new kind of Maps in Bas Relief. Both practised in France. By Mr. John Evelyn. N° 6, p. 99.

This was communicated by the ingenious Mr. John Evelyn, to whom it was sent from Paris in a letter, as follows:—

Here is a Frenchman who makes more lively exhibitions of nature in wax than ever I yet saw in painting; having an extraordinary address in modelling the figures, and in mixing the colours and shadows; also making the eyes like nature.

I have also seen a new kind of maps in bas relief, or sculpture: For example, the isle of Antibe, on a square of about eight feet, made of boards, with a frame like a picture. There is represented the sea, with ships and their cannons and tackle of wood fixed upon the surface, after a new and most admirable manner. The rocks about the island exactly formed, as they are in nature; and the island itself, with all its inequalities, hills and dales; the town, the fort, the small houses, platform, and cannons mounted; and even the gardens and platforms of trees, with their green leaves standing upright, as if they were growing in their natural colours. In short, men, beasts, and whatever you may imagine to have any protuberancy above the level of the sea. This new, delightful, and most instructive form of map, or wooden country, affords equally a very pleasant object, whether it be viewed horizontally or sidelong.

Some Observations on a White Fluid resembling Milk, found in the Veins, instead of Blood: (Anonymous). And on Grass found in the Wind Pipes of some Animals. By Mr. Boyle. N° 6, p. 100.

This observation of a fluid resembling milk being found in the veins instead of blood, is explained by a further communication at the end of this same number, p. 117, where it is related, on the authority of Dr. Lower, that a maid after eating a good breakfast about seven in the morning, was let blood about eleven the same day in her foot. The blood first drawn, which was received in a porringer, soon turned very white: that which was last drawn was received in a saucer, and turned white immediately, like the white of a custard. Within five or six hours after the physician saw both, and found that contained in the porringer to be half blood and half chyle, swimming upon it like a serum as white as milk; and that in the saucer all chyle, without the least appearance of blood. When heated over a gentle fire they both hardened, as the white of an egg or the serum of the blood do under similar circumstances, but they were far whiter.*

The second part of this communication states, that a considerable quantity of grass has often been found in the trachea and bronchia of sheep; and that the wind-pipe of an ox, which had died of a disease, was stuffed with the same vegetable matter as if it had been thrust there by main force.

Of a Place in England where, without petrifying Water, Wood is turned into Stone. N° 6, p. 101.

According to Dr. Plott, the place where the petrified wood commemorated in this paper is found, is a gravelly ground in the parish of Wendlebury in Oxfordshire, not far from the church. The kind of osteocolla mentioned in this paper is found in the rubble-quarries at Heddington in the same county; when scraped it has the smell of burnt bone.

Of the [medicinal] Nature of a certain Stone found in the Indies in the Head of a Serpent. By PHILIBERT VERNATI. N° 6, p. 102.

This communication relates to a superstitious story of a stone found in the head of certain serpents, having the property of healing their venomous bite when applied to the wound. We shall not detain the reader with more particulars on this subject.

Of the Way used in the Mogul's Dominions to make Saltpetre. From THEVENOT'S Travels. N° 6, p. 103.

Saltpetre is found in many places of the East Indies, but chiefly about Agra, and in the villages that heretofore have been numerously inhabited, but are now deserted. They extract it from three sorts of earth, black, yellow, and white. The best is that which is drawn out of the black, for it is free from common salt. They work it in this manner: They make two pits, flat at the bottom, like those wherein common salt is made, one of them having much more compass than the other, they fill that with (the aforesaid) earth, upon which they let water run, and by the feet of people they tread it to the con-

* In the blood of this patient there appears to have been an over-proportion of serum, rendered whiter than usual by a considerable quantity of new chyle. The manner in which the original title is worded might lead to an erroneous conception; since it was not "MILK that was found in veins INSTEAD of blood," (p. 100), but blood itself, containing such an excess of the fluids just mentioned as to have a *milky appearance*.

sistency of a pap, letting it stand for two days, that the water may extract all the salt that is in the earth: then they pass this water into another pit, in which it crystallizes into saltpetre. When they would have it whiter and purer, they boil it in a cauldron once or twice, scumming it continually, and emptying it out, whilst it is hot, into earthen pots which hold each 25 or 30lbs. These they expose to clear nights, and if there be any impurity remaining it falls to the bottom. Afterwards they break the pots, and dry the salt in the sun.

An Account of HEVELIUS'S *Prodromus Cometicus, with some Animadversions made upon it, by a French Philosopher. N° 6, p.* 104.

Hevelius, in his Prodromus, so called, because it is as an introduction to his Cometography, gives an account of the observations he has made on the first of the two late comets; reserving those on the second for that great treatise, in which he also intends to enter more into detail on the first.

In this account he represents the rise, place, course, swiftness, phases and train of this comet, and the causes of the generation of comets in general.

He finds its apparent motion was not made in an exact great circle, but deviating considerably from it.

He is positive, that without the annual motion of the earth no rational account can be given of any comet, but that all is involved in perplexities, and deformed by absurdities.

He inquires what kind of line they describe by their motion, whether circular, or straight, or curved, or partly straight and partly curved? And if curved, whether regular or irregular? if regular, whether elliptic, parabolical, or hyperbolical? He answers, that this motion is conical; and is of opinion that by the conic path all the phænomena of comets can, without any incoveniency, be readily solved.

He puts it out of doubt that they are in the sky itself, producing reasons for it that are very considerable, and alleging among others, that it is clearly evinced by the parallaxes, which he finds far less in comets than in the moon, and sometimes than in the sun itself. Where he also represents that he has deduced the horizontal parallax of this very comet from one observation only, by which he found that then it was distant from the earth 5000 semidiameters of the same. From this distance from the earth, he deduces, that on that day when it was so remote from the earth, its true diameter was 2560 German miles, which is three times larger then the diameter of the earth, and almost six times larger than that of the moon, whose diameter, according to his theory, is 442 German miles.

He finds the matter of comets to be in the æther itself; and that the planets emit their exhalations, and have their atmospheres like our earth. He affirms that the sun alone may cast out so much matter at any time in one year, as that thence shall be produced not one or two comets, equalling the moon in diameter, but very many; which if so, what contribution may not be expected from the other planets.*

He conceives that all comets respect the sun as their centre as planets do, making them a kind of spurious planets that emulate the true ones in their motion almost in all things.

The train, according to him, is only the beams of the sun falling on the head of the comet, and passing through the same, refracted and reflected.

Whether the same comets return again, as the spots in the sun? and, Whether in the time of great conjunctions they are more easily generated? and, Whether they can be certainly foretold? with several other inquiries, he refers for to his great work.

As to prognostications, he complains, that men inquire more what comets signify than what they are, or how they are generated and moved; professing himself to be of the opinion of those who would have comets rather admired than feared; there appearing indeed no cogent reason, why the Author of Nature may not intend them rather as monitors of his glory and greatness than of his anger or displeasure; especially seeing that some very diligent men (among whom is Gemma Frisius) take notice of as great a number of good as bad events consequent to comets.

Some animadversions have been made on this work by M. Auzout, in a letter to M. Petit; in which he conceives that this prodromus contains some mistakes, which he particularises.

Of the Mundus Subterraneus of ATHANASIUS KIRCHER.† *N° 6, p.* 109.

This paper contains a long and tedious account of this work, after the manner of a modern review of it, enumerating all the particulars of the contents: an account now no longer either curious or useful.

* It is curious, and not unprofitable, sometimes to observe the strange whims and puerile notions which some of the greatest men have formed in the early periods of certain natural and scientific discoveries.

† Athanasius Kircher, or Kirker, a noted philosopher, was born at Fulda in 1601. He entered into the society of Jesuits in 1618, and taught philosophy, mathematics, and the oriental languages in the university of Wirtsburg, till the year 1631. He was afterwards called to Rome, where he taught mathematics in the Roman college, and died in 1680, being the 80th year of his age.—Kirker collected a rich cabinet of machines and antiquities; and the quantity of his works is immense,

A farther Account of an Observation on white Blood.

The substance of this paper is incorporated with a former account at p. 37.

Of M. de Sons' Progress in working Parabolic Glasses. N° 7, p. 119.

Since what was mentioned in a former paper respecting M. de Sons' noble attempt to grind glasses of a parabolic figure, the publisher of these papers has himself seen two eye-glasses of that shape, about one inch and a half deep, and one inch and a quarter broad, wrought by this eminent artist with a rare steel instrument of his own contrivance and workmanship, and by himself also polished to admiration. And certainly it will be wondered at by those who shall see these glasses, how they could be truly wrought to such a figure with such a cavity; and yet more, when they shall hear that the author undertakes to excavate other such eye-glasses to above two inches, and object-glasses of five inches diameter. He has likewise already begun his object-glasses for the above-mentioned two ocular ones, of the same figure, of about two inches diameter, which are to be left all open, yet without causing any colours.

M. Auzout's Speculations on the Changes likely to be discovered in the Earth and Moon, by their respective Inhabitants. N° 7, p. 120.

I have, says he, sometimes thought on the changes which it is likely the supposed inhabitants of the moon might discover in our earth, to see whether reciprocally I could observe any such in the moon. For example, that the earth would appear to the people of the moon to have a different face in the several seasons of the year; and to have another appearance in winter, when there is scarcely any thing green on a very great part of the earth; when there are countries all covered with snow, others all covered with water, others all obscured with clouds, and that for many weeks together. Another face in spring, when the forests and fields are green. Another in summer, when all are yellow, &c. Methinks such changes are considerable enough, by the force of the reflections of light, to be observed, since so many differences of lights are seen in the moon. We have rivers considerable enough to be seen, and they enter far enough into the land, and have a breadth sufficient to be observed. There are fluxes in certain places, that reach into large countries, capable of making there some apparent change; and in some of our seas there float sometimes

amounting to 22 volumes in folio, 11 in quarto, and 3 in octavo. Most of them indeed are rather curious than useful; many of them visionary and fanciful; and it is not to be wondered at, if they are not always accompanied with much exactness or precision.

such bulky masses of ice, that are far larger than the objects which we are assured we can see in the moon. Again, we cut down whole forests, and drain marshes, of an extent large enough to cause a considerable alteration: And men have made such work, as have produced changes large enough to be perceived. In many places also are volcanoes sufficiently large to be distinguished, especially in the shadow: And when forests or great towns are on fire, it can hardly be doubted, but these luminous objects would appear, either in an eclipse of the earth, or when such parts of the earth are not illuminated by the sun. But I know no man who has yet observed such things in the moon;* and one may be rationally assured that no volcanoes exist there, or that none of them burn at this time. And to this all curious men, that have good telescopes, ought well to attend. And I doubt not, that if we had a very particular map of the moon, we or our posterity should find some changes in her. And if the maps of the moon of Hevelius, Divini, and Riccioli, be exact, I can affirm, that I have seen there some places considerable enough, where they put parts that are clear, whereas I there see dark ones. It is true that if there be seas in the moon, it can hardly happen otherwise than on our earth, where alluviums or new accessions of land are made in some places, and the sea gains on the land in others. I say, if those spots we see in the moon are seas, as it is commonly believed; whereas I have many reasons for being of a contrary opinion. And I have sometimes thought whether it might not be, that all the seas of the moon, if there must be seas, were not on the side of the other hemisphere, and that for this cause it might be that the moon turns not her axis, as our earth, in which the lands and seas are, as it were, balanced. This also may be the cause why there appear not any clouds there, nor any vapours considerable enough to be seen, that are raised from the earth; and that this absence of vapours may also be the reason that there is no twilight there, as it seems there is none, I myself at least not being able to discern any: For I think the reputed inhabitants of the moon might see our twilight, since it is much stronger than the light afforded us by the moon, even when full; for a little after sun-set, when we receive no more the first light of the sun, the sky is far clearer than it is in the fairest night of the full moon. And since we observe in the moon, when she is increasing or decreasing, the light she receives from the earth, we cannot doubt but that the people of the moon should likewise see in the earth the light with which the moon illuminates it, with perhaps the difference there is between their magnitude. Much more then should they see the crepuscular light, being, as

* Such things have of late been observed, in consequence of the great improvements in telescopes by Dr. Herschel: thus verifying the speculations of the ingenious French astronomer.

was said, incomparably greater. But yet we see not any faint light beyond the section of the light, which is almost every where equally strong, and we there distinguish nothing at all, not so much as that clearest part called aristarchus, or porphyrites, as I have often tried; although one may there see the light which the earth sends thither, which is sometimes so strong, that in the moon's decrease I have often distinctly seen all the parts of the moon that were not enlightened by the sun, together with the difference of the clear parts and the spots, so far as to be able to discern them all. The shadows also of all the cavities of the moon seem to be stronger than they would be if there were a second light. For although afar off the shadows of our bodies, environed with light, seem to us almost dark; yet they do not appear so in the same degree as the shadows of the moon; and those on the edge of the section should not appear in the like manner. But I will determine nothing of any of these things.

To measure Distances at one Station. By M. Auzout. N° 7, p. 124.

It is long since I found out a method of measuring, with a large telescope, from one station, the distance of objects on the earth. The practice indeed does not altogether answer the theory, because the length of the telescopes admits of some latitude; yet it comes near enough, and is perhaps as just as most of the ways commonly used with instruments. If we consider the sole theory only, an ordinary telescope may be used, having its eye-glass convex: for, by putting the glasses at a little greater distance than they are, proportionably to the distance for which it is to serve, and by adding to it a new eye-glass, the object will be seen distinct, though obscure; and if the eye-glass be convex, the object will appear erect. It may be done two ways; either by leaving the telescope in its ordinary situation, the object-glass before the eye-glass; or by inverting it, and putting this before that. But if two object-glasses be used, of which the foci are known, the distance of them will be also known. If we suppose the focus of the first to be B, that of the second C, and the given distance $B+2D$, and that D minus C is equal to F, for this distance will be equal to $B+C+F-r\ F^2-C^2$. And if you have the focus of the first object-glass equal to B, the distance at which the second glass is to be put, equal to $B+C+D$, the focus of the second glass will be found equal to $\frac{CD}{C+D}$. And if you would have the object magnified as much with these two glasses, as it would be with a single one, of which the focus should be of the given distance, having the focus of the object-glass given equal to B, and the distance given equal to $B+D$; then the distance between the first and second glass will be equal to $\frac{2B^2+2BD}{2B+D}$ from which subducting B (the given focus of the object-glass) there remains $\frac{BD}{2B+D}$; and if this sum be supposed equal to C, we shall easily know, by the preceding rule, the focus of the second glass.

A Way of preparing a Liquor that shall sink into and colour the whole Body of Marble; causing a Picture, drawn on a Surface, to appear also in the inmost Parts of the Stone. By A. Kircher. N° 7, p. 125.

The colours are thus prepared: Take of aqua fortis and aqua regia, two ounces each; of sal ammoniac one ounce; of the best spirit of wine, two drams; as much gold as can be had for four shillings and six pence; and of pure silver, two drams. These materials being provided, let the silver, when calcined, be put into a vial; and having poured upon it the two ounces of aqua fortis, let it evaporate, and you will have a water yielding first a blue colour, and afterwards a black. Likewise put the gold, when calcined, into a vial, and having poured the aqua regia upon it, set it by to evaporate: then pour the spirit of wine upon the sal ammoniac, leaving it also till it be evaporated; and you will have a golden coloured water, which will afford divers colours. After this manner you may extract many tinctures of colours out of other metals. This done, you may, by means of these two waters, paint what picture you please on white marble of the softer kind, renewing the figure every day for several days with some fresh superadded liquor; and you will find in time, that the picture has penetrated the whole solidity of the stone, so that cutting it into as many parts as you will, it will always represent to you the same figure on both sides.

But whether this experiment will succeed, or not, it is certain that Mr. Bird, a stone-cutter in Oxford, has many years ago found out a way of doing the same thing, and has practised it for many years. That is, he can apply a colour to the outside of polished marble, that shall sink a considerable depth into the body of the stone, and there represent the same figures as those on the outside, deeper or shallower as he continues the application a longer or shorter time. Several pieces of which are to be seen in Oxford, London, and elsewhere. And some of them being shown to his majesty, they were broken in his presence, and found to answer expectation.

China Wares made in Europe. N° 7, p. 127.

Notice was lately given by a Parisian to a friend in London, that he had been informed that Signior Septalio, a canon in Milan, had the secret of making as good porcelain as is made in China itself, and transparent: adding that he had seen him make some.

Account of an odd Spring in Westphalia; also of Salt Springs, and the straining of Salt Water. N° 7, p. 127.

In the diocese of Paderborn, in Westphalia, there is a spring which disappears twice in 24 hours, returning always after six hours with a great noise, and so forcibly as to drive three mills not far from its source. The inhabitants call it the bolderborn, that is, the boisterous spring.

It is remarked that no salt water which contains any metal in it, can well be boiled to salt in a vessel of the same metal which itself contains, except vitriol in copper vessels.

It is added, that to separate salt from salt water without fire, if you take a vessel of wax, hollow within, and every where tight, and plunge it into the sea, or into other salt water, there will be such a separation made, that the vessel will be full of sweet water, the salt remaining behind: but though this water have no saltish taste, yet there will be found a salt in the essay, which is the spirit of salt, subtle enough with the water to penetrate the wax.

An Account of the Method of conveying Liquors immediately into the Mass of the Blood. By Mr. OLDENBURG. *N° 7, p.* 128.

In this account it is asserted, that the discovery of a method of conveying liquors immediately into the mass of blood is due to Dr. Christopher Wren, at that time Savillian professor in the university of Oxford. The method which he followed was to make a ligature on the veins, and having made an opening in them on the side of the ligature towards the heart, to introduce into them slender syringes or quills fastened to bladders (in the manner of clyster pipes) containing the matter to be injected; performing the operation upon pretty big and lean dogs, that the vessels might be large enough and easily accessible. These experiments were made at different times upon several dogs. Opium and the infusion of crocus metallorum were injected into the veins of the hind legs of these animals. The opium soon stupified, but did not kill the dog; but a large dose of the crocus metallorum induced vomiting and death in another dog. These experiments are more circumstantially related by Mr. Boyle, in his excellent book on the *Usefulness of Experimental Philosophy*, Part II. Essay II. pp. 53-55. See also an account of other experiments of this sort in No. 27 of these Transactions.

Trials, made in Italy, of CAMPANI'S *new Optic-Glasses. N° 8, p.* 131.

Intelligence was lately received from Rome, importing that Campani has had the advantage of Divini.* The great Duke of Tuscany, and Prince Leopold, his brother, upon trial made of both their glasses, have found that those of Campani excel the other; and that, with them, they have been able easily to distinguish people at four leagues distance.

A further Relation of the Whale-Fishing about the Bermudas, and on the Coast of New-England and New-Netherland. N° 8, p. 132.

The person who communicated the particulars respecting the new Whale-fishing near the Bermudas, mentioned in the first of these tracts, gives this further information: That about two years since, there stranded on the coast of New-England a dead whale, of the species which is called Trumpo, having teeth resembling those of a mill, and its mouth at a good distance from, and under the nose or trunk, having several boxes or partitions in the nose, like those in the tails of lobsters. And that being opened, there run out of it a thin oily substance, which congealed; after which, the remainder, being a thick fatty substance, was taken out of the same part with a scoop: And this substance, he affirmed, is the spermaceti; adding further, that the blubber of the same sort of whales, when stewed, yields on the top a creamy substance, which when thrown upon white wine lets fall a dirty heterogeneous sediment; but what remains above affords a spermaceti-like matter.

He concluded by observing, that these whales were to be met with between the coast of New-England and New-Netherland, where they might be caught eight or nine months in the year, whereas those about the Bermudas are to be found only in the months of February, March, and April.†

Of a remarkable Spring near Paderborn in Germany. N° 8, p. 133.

In the diocese of Paderborn, about two leagues from that town, is a spring, called metborn, with three streams, two of which are not above one foot and a half distant from each other, and yet of such different qualities, that one of

* Eustachio Divini was an ingenious Italian artist of those times, very eminent for his telescopes, though, as it seems, inferior to Campani. He wrote against the discovery of Saturn's ring, made by Huygens, contesting the truth of it. Divini was living in 1663; but the time of his birth or death is not ascertained.

† The species of whale here meant is the Physeter macrocephalus, Lin. The large fish mentioned in the latter part of the paper (and which is here omitted) seems to be the Xiphias platypterus or broad-finned sword-fish, which Dr. Bloch rather chuses to consider as a species of Scomber. It is a great enemy to whales, which it wounds, and probably sometimes kills by striking them with its horn or snout. It sometimes attacks ships in the same manner, and is supposed to mistake them for whales; breaking the horn or snout with the violence of the effort, and leaving a part imbedded in the timber.

them is limpid, bluish, lukewarm, and bubbling, containing sal-ammoniac, oker, iron, vitriol, alum, sulphur, nitre, and orpiment,* used against epilepsies, diseased spleens, and the worms; the other is ice-cold, turbid, and whitish, much stronger in taste, and heavier than the former, containing much orpiment, salt, iron, nitre, and some sal-ammoniac, alum, and vitriol. All birds that drink of the latter are observed to die; which I have also made experiment of, by taking some of it home, and giving it to poultry, after having eaten oats, barley and bread-crums: For soon after drinking it, they became giddy, reeled and tumbled upon their backs, with convulsions, and so died with their legs much extended. Giving them common salt immediately after they had drunken, they lived longer; giving them vinegar, they died not at all, but seven or eight days after were troubled with the pip. Those that died being opened, their lungs were found quite shrivelled. Yet some persons who are troubled with worms, taking a little quantity of it diluted with common water, have been observed by this means to kill the worms in their bodies, and discharged great numbers of them: and though it makes them sick, yet not so as to endanger their lives.

The third stream, lying lower than the other two, and about 20 paces distant from them, is of a greenish colour, very clear, and of a sourish sweet taste, agreeable enough. Its weight is a medium between that of the other two; whence it is probable that it is a mixture of both, meeting there together: To confirm which, we mixed equal quantities of those two with a little common well water, and found, on stirring them together, and permitting them to settle, that they produced water of the same colour and taste as this third stream.

Of other uncommon Springs at Basil and in Alsace. N° 8, *p.* 134.

At Basil there is a spring of a bluish colour, and somewhat troubled, holding copper, bitumen, and antimony; about three parts of the first, one of the second, and two of the last. The tanners water their skins in it; and, being a well-tasted and wholesome water, it is both much drunken, and used to bathe in.†

In the same town, which abounds with spring waters, there are two, among the rest, called Bandulph's Well, and Brun Zum Brunnen, that are more observable than the others, the former of them having a camphory and drying

* The chemical analysis of mineral waters was so imperfectly understood in the 17th century, that little reliance can be placed on the number and proportion of ingredients assigned in this and other instances.

† If this water had really held copper and antimony in solution, it could not have been well-tasted; much less could it have been a wholesome water. Antimony (we believe) has never yet been found in any mineral water, and copper only in the streams which run from copper mines.

quality, and used against dropsical distempers; the latter containing some sulphur, saltpetre and gold,* and being an excellent water to drink, is much used in the principal tavern of the city.

In Alsace, in the valley called Leberthal, near Geesbach, an ancient mine work, there runs out of a cavern a foul, fattish, oily liquor, which, though the countrymen of that place employ in greasing their wheels instead of ordinary wheel-grease, yet it affords an excellent balsam, by putting a quantity of it into an earthen pot well luted, that no steam may exhale; and then, with a gentle fire at first, but stronger afterwards, boiling it for three hours together; when it will have decreased a fourth part, and an earthen matter, like pitch, will settle itself at the bottom: but on the top, when cold, there will swim a fatty substance, like lyne-oil, limpid and somewhat yellowish, which is to be decanted from the thick sediment, and then gently distilled in an alembic in arena; by which means there will come over two different liquors, one phlegmatic, the other oily, which latter, swimming on the phlegm, is to be severed from it. The phlegm is used as an excellent resister and curer of putrefactions of the lungs and liver; and it heals all foul wounds and ulcers. The oily part, being diluted with double its quantity of distilled vinegar, and three times distilled over, yields a rare balsam, used against all inward and outward corruptions, stinking ulcers, hereditary scurfs and scabs. It is also much used against apoplexies, palsies, consumptions, giddinesses, and head-aches. Inwardly they take it with succory water against all corruptions of the lungs. It is a kind of petroleum, and contains no other mineral juice, but that of sulphur, which seems to be thus distilled by nature under ground; the distillation of an oil out of sulphur by art, being not so easy to perform.

Of the richest Salt Springs in Germany. N° 8, p. 136.

The salt springs at Hall in Saxony are four, called Gutighr, the Dutch spring, the Mettritz, and the Hackel-dorn. The first three contain each about seven parts of salt, three of marcasite, and 14 of water. The last contains less, but yields the purest salt. They are, besides their ordinary use, employed medicinally to bathe in; and a spirit is extracted from them, used with good success against venom, and the putrefaction of the lungs, liver, reins, and spleen.

The salt water at Lunenburg, being more greenish than white, and not very transparent, is nearly of the same nature and contents with that of Hall. It has a mixture of lead in it, which hinders it from being boiled in pans of that metal;† and if it contained no lead at all it would not be so good, that metal being

* The existence of gold in this mineral water is wholly imaginary.

† This mixture of lead is supposititious, but were it true, it could be no impediment to the boiling of the water in pans of that metal.

judged to purify the water: whence also Lunenburg salt is preferred before any from salt springs.

Swarms of strange and mischievous Insects, in New England. *N° 8, p.* 137.

Some few years since in New England, there was such a swarm of a certain kind of insects,* that for the space of 200 miles they destroyed all the trees of the country. There appeared innumerable little holes in the ground, out of which they broke forth in the form of maggots, which turned into flies, with a kind of tail or sting, which they struck into the tree, and thereby envenomed and killed it.

The like plague is said to happen frequently in the country of the Cosaks or the Ukrane, where in dry summers they are infested with such swarms of locusts, driven thither by an east or south-east wind, that they darken the air in the fairest weather, and devour all the corn of that country; laying their eggs in autumn, and then dying; but the eggs, of which every one lays two or three hundred, hatching the next spring, produce again such a number of locusts, as to be far more destructive than before, unless rains fall, which kill both eggs and the insects themselves, or unless a strong north or north-west wind arise, which drives them into the Euxine sea.

The Brooding of Snakes and Vipers. *N° 8, p.* 138.

There is this difference between the brooding of snakes and vipers, that the former lay their eggs in dung-hills, by the warmth of which they are hatched; but the latter brood their eggs within their bellies, and bring forth live vipers.

Some Observations of odd Constitutions of Bodies. By Mr. OLDENBURG. *N° 8, p.* 138.

The first of these observations gives an account of a person becoming dropsical, from taking cold by spending many nights in the open air, in making astronomical observations—The second contains a relation of a young girl of 13, who from the time she was six years old and began to be about her mother in the kitchen, would, as often as she was bid to bring her salt, fill her pockets with it and eat it as other children do sugar; whence she was so dried up and grown so stiff, that she could not stir her limbs, and was thereby starved to death—The third

* What these insects were it is not very easy to determine.

and last part of these observations contains another instance (in addition to those mentioned in a former communication) of a white fluid, resembling milk, being drawn from the vein of a patient who was ordered to be blooded.

To preserve Ice and Snow by Chaff. By Mr. Wm. Ball. N° 8, p. 139.

The snow and ice-houses at Leghorn in Italy are commonly built on the side of a steep hill, being only a deep hole in the ground, by which means they easily make a passage out from the bottom of it, to carry away all the water, which, if it should remain stagnating there, would melt the ice and snow. They are thatched with straw in the shape of a saucepan-cover, that the rain may not enter them. This pit is filled full of snow or ice, which must be of the purest water, to be used in their wine, after first spreading the bottom very well with chaff; I think they use barley chaff. Then, as they put in the ice or the snow, (which latter they ram down,) they line the sides very thick with such chaff, and afterwards cover it well over with the same. In half a year's time it is found not to have lost above an eighth part of its first weight. Whenever they take it out into the air, they wrap it in this chaff, and it thus keeps exceeding well.

The other usual way, both in Italy and other countries, to preserve snow and ice, is with straw or reed; as is particularly related by Mr. Boyle, in his Experimental History of Cold.

Directions for Seamen bound for long Voyages. By Mr. Rooke. N° 8, p. 140.

It being the design of the Royal Society, for the better attaining the end of their institution, to study nature rather than books, and, from the observations made of the phænomena and effects she presents, to compose such a history as may lay a foundation for solid and useful philosophy: For this purpose they gave orders to several of their members, and among others to Mr. Rooke,*

* Mr. Lawrence Rooke, a distinguished astronomer and mathematician, was born at Deptford near Greenwich, in 1623. He was educated at Eton school, whence he removed to King's College, Cambridge, in 1639. But in the year 1650 he settled at Oxford to enjoy the company of several eminently learned men residing there, where he associated with those philosophers who afterwards formed the Royal Society. In 1652 Mr. Rooke was chosen professor of astronomy in Gresham College, London, which appointment in 1657 he exchanged for the geometry professorship. Having enjoyed these situations some years before the restoration in 1658, most of those gentlemen who had been accustomed to assemble with him at Oxford, coming to London, joined with other philosophical persons, and usually met at Gresham College to hear Mr. Rooke's lectures, and after the

professor of geometry in Gresham College, to draw up heads of enquiries for the use of seamen in making observations in their voyages, which he accordingly executed, as below:—

1. To observe the declination of the compass, or its variation from the meridian; noting also the latitude and longitude of the place where such observation is made, and stating the method of taking them.

2. To carry dipping needles with them, and observe their inclination.

3. To observe carefully the tides in as many places as they can, with all the circumstances; such as the precise time of ebbing and flowing in rivers, and at capes; the set of their currents, the perpendicular distance between the highest tide and lowest ebb, in spring and neap tides; what day of the moon's age, and what times of the year, the highest and lowest tides happen.

4. To make plots and draughts of coasts, promontories, islands and ports, marking the bearings and distances as near as may be.

5. To sound and mark the depths of coasts, ports, and other places near the shore.

6. To observe, in all soundings, the nature of the ground at the bottom of the sea, whether it be clay, sand, rock, &c.

7. To keep a register of all changes of the wind and weather at all hours, both day and night, the point the wind blows from, and whether strong or weak; the rains, hail, snow, &c., the precise times of their beginnings and continuance, especially hurricanes and spouts; and above all, carefully to observe the trade-winds, about what degrees of latitude and longitude they first begin, where and when they cease, or change, or grow stronger or weaker, and how much.

8. To observe all extraordinary meteors, lightnings, thunders, *ignes fatui*, comets, &c., marking the places and times of their appearing, continuance, &c.

9. To carry with them good scales, and glass-vials of about a pint, with narrow mouths, to be filled with sea-water in different latitudes, in order to know its weight, both of the water near the surface and at greater depths.

Of the Shadow of one of Jupiter's Satellites, seen by a Telescope, passing over the Body of Jupiter. *N°* 8, *p.* 143.

On the 26th of September last, at half an hour after seven o'clock, was seen, both in Holland and in France, the shadow of one of the satellites of Jupiter,

lectures withdrew into his apartments to converse together: These meetings at length gave rise to the Royal Society itself; of which great and useful institution Mr. Rooke was a zealous promoter; though he did not live till it received its establishment by the royal charter, as he died the 27th of June, 1662, in the 40th year of his age, universally respected and regretted.

passing over his body. One of those small stars moving round his body, which are therefore called his satellites, coming between the sun and it, made a small eclipse, appearing on the face of Jupiter as a little round black spot.

Of a permanent Spot in Jupiter: by which is manifested the Rotation of Jupiter about his own Axis. N° 8, p. 143.

Besides that transient shadow last mentioned, there has been observed, by Mr. Hook first, (as is mentioned in Number I of these Transactions) and since by M. Cassini, a permanent spot in the disk of Jupiter; by means of which they have been able to observe, not only that Jupiter turns about his own axis, but also the time of such rotation; which he estimates to be nine hours and 56 minutes.

For, as Kepler before conjectured, from the motion of the primitive planets about the sun as their centre, that the sun moved about his own axis, but could not prove it, till by Galileo and Schiner the spots in the sun were discovered; so it has been thought probable, from the secondary planets moving about Jupiter, that Jupiter is also moved about his axis; yet till now it has not been evinced by observation that he does so move, much less in what period of time. And the like reason there is to judge so of Saturn, because of the secondary planet discovered by M. Huygens to move about him, though such motion be not yet evinced from observation, as well as that of the earth, from its attendant the moon.

Of some Philosophical and curious New Books. N° 8, p. 145.

Of these it may be sufficient to mention two, viz.

1. A narration of the establishment of the Lyncei, an Italian academy, and of their design and statutes: the Prince Cesi being the head of them, who also intended to establish such philosophical societies in all parts of the world, and particularly in Africa and America, to be by that means well informed of what considerable productions of Nature were to be found in those parts.

2. A book just printed at Oxford, being a catalogue of fixed stars, with their longitudes, latitudes, and magnitudes, according to the observations of Uleg-Beig, a king and noted astronomer, who was great grandson to the famous Tamerlane, and one of his successors in some of his kingdoms. These observations were made at Samarcand, in the year of Christ 1437: for, not finding the tables of Ptolemy to agree sufficiently with the heavens, he, with great diligence and expense, made observations anew; as Tycho Brahe has done since. It is only a small part of a larger astronomical treatise of his, of which there are several Persian manuscript copies in Oxford. Out of which this is translated.

and published, both in Persian and Latin, by Mr. Thomas Hyde, now library keeper to the Bodleyan Library in Oxford, with commentaries of his annexed; like as another part of it has formerly been by Mr. John Graves. And it would be a commendable thing to translate the whole, that we might be the better acquainted with what the eastern astronomy at that time really was.

Appendix to the Directions for Seamen bound for long Voyages. By Mr. Hook. N° 9, p. 147.

This paper is repeated, and much enlarged, in N° 24, where it is given in a more perfect state.

On the Difference between two learned Men, about an Observation made of the first of the two late Comets. N° 9, p. 150.

As it has been noticed in Number 6, that there was some difference between those two deservedly celebrated philosophers, M. Hevelius and M. Auzout, concerning an observation made by the former on the $\frac{8}{18}$ of February 1665; and that thereupon some eminent English astronomers, considering the importance of the dispute, had undertaken to examine it; it is conceived that it will not be unacceptable to present the result to the reader of these papers. Having therefore compared the printed writings of the two disputants, and consulted the observations made with telescopes at home, by some of the most intelligent astronomers amongst them, who have attentively observed the relative position of that comet to the telescopic stars that lay in its way, they conclude that whatever that appearance was which was seen near the first star of Aries, by M. Hevelius, (the truth of whose relation concerning the same they by no means question) the said comet did not come near that star in the left ear of Aries, where the said M. Hevelius supposes it to have passed, but took its course near the bright star in its left horn, according to Bayer's tables. And the same is also confirmed by the observations of other astronomers in France, Italy, and Holland.

Of finding the true Distance of the Sun and Moon from the Earth, by the observed Parallax. N° 9, p. 151.

The discovery of this distance may prove of important use for the perfecting of astronomy, and for better establishing the doctrine of refractions.

To perform which the following method is proposed, viz. That, at certain times agreed on by two observers, using large and proper telescopes, with a measuring rod, placed within the eye-glass at a convenient distance, that it may be distinctly seen, and serve for measuring small distances to minutes and seconds; let each of them, thus furnished, observe the visible way of the moon among the fixed stars, by taking her exact distance from any fixed star that lies in or very near her way, with the exact time of such appearing, and also the apparent diameter of her disk: continuing these observations every time for two or three hours, so that two exact observations of her apparent place among the fixed stars being made, at two places distant in latitude, and nearly under the same meridian, hence her true and exact distance may be collected, not only for that time, but at all other times, by any single observer viewing her with a telescope, and measuring exactly her apparent diameter.

It is also desirable, when there happens any considerable eclipse of the sun, that they would observe exactly the measure of the greatest obscuration, compared with the apparent diameter of his disk. For by this means, after the distance of the moon has been exactly found, the distance of the sun will easily be deduced.

The fittest time for making observations on the moon will be when she is about a quarter or somewhat less illuminated, which is about four or five days before or after her change; because then her light is not so bright but that with a good telescope she may be observed to pass close by, and sometimes over several fixed stars. Or else, at any other time when the moon passes near or over some of the larger fixed stars, which may be easily calculated and foreseen. Or, best of all, when there is any total eclipse of the moon; for then the smallest telescopic stars may be seen close to her body.

An Observation on Saturn. N° 9, p. 152.

This observation was made by Mr. William Ball, accompanied by his brother, Dr. Ball, October 13, 1665, at six o'clock, at Mainhead near Exeter in Devonshire, with a very good telescope near 38 feet long, and a double eye-glass, as the observer himself takes notice, adding, that he never saw that planet more distinct. This observation has induced the supposition that Saturn is surrounded, not by *one* circular body or ring only, but by *two*. And the further observation of this appearance is earnestly recommended to the author (Huygens) of the System of Saturn.

Of Barometers, and some Observations made with them. By Dr. Beal. N° 9, p. 153.

The barometer is an instrument for measuring the weight of the atmosphere, in order to determine the changes of the weather. It is founded on the Torricellian experiment, so called from its inventor Torricelli; and is only a glass tube filled with mercury, hermetically sealed at one end, with the other end open, and immerged in stagnant mercury. The barometer was first made public by Mr. Boyle, and employed by him and others to discover all the minute variations in the pressure and weight of the air. With this instrument he made many observations in 1659 and 1660, before any others were public or heard of by him.

Dr. Beal is so well pleased with the discoveries already made with this instrument, that he looks upon it as one of the most extraordinary inventions in the world. "Who could have thought (says he) that men should find an art "to weigh the air that hangs over their heads in all its changes; and even "distinguish by weight the winds and clouds? Or, who could have imagined "that the clearest air is the heaviest, and the thickest air, when loaded with "clouds, ready to dissolve and fall, should then be the lightest?" Hence the doctor descends to particular observations.

And first, he says, he could never fill his wheel-barometer so exactly with mercury as to exclude all air; and therefore he depended more on the mercurial cylinder, from which he took all his notes. Its length is but 35 inches, of a narrow bore, and a thick glass.

2. In all his observations from May 28, 1664, to December 9, 1665, the quicksilver ascended but very little above 30¼ inches.

3. It ascended seldom so high as that, especially on December 13, 1664, the weather being changeable, and the evening fair.

4. By his calendar of June 22, 1664, at five in the morning, in a long tract of fair settled weather, the mercury ascended about half an inch higher than 30. So that the mercury may rise as high in the hottest summer as in the coldest winter.

5. He had observed it ascend higher in cold weather; and very often both in winter and summer to be higher in the cold mornings and evenings than in the warmer mid-day.

6. Generally, in settled and fair weather, both winter and summer, the mercury is higher than a little before, or after, or in rainy weather.

7. Again, it descended generally lower after rain than it stood before rain.

8. It falls also generally in great winds, and it seemed to sink a little upon opening a wide door to let in stormy winds: Yet he found it to continue very high in a long stormy wind of three or four days.

9. Again, it is generally higher in an east and north wind than in a south and west wind.

10. He tried several times to alter the air in his closet by fumes and thick smoaks; but the mercury seemed not to be affected more than what might

be expected from some increase of heat: Such as have exact wheel-barometers may try whether odours or fumes make the air lighter.

11. He did not find in all this time the greatest changes of the quicksilver to amount to more than $2\frac{3}{4}$ or $2\frac{7}{8}$ inches at most.

12. He very often found great changes in the air without any perceptible change in the barometer; as in dewy nights, when the moisture descends plentifully. On the preceding and following days the vapours have been raised so invisibly, that the air seemed very clear. Which rising and falling of vapours import gravity and levity of air, and yet the barometer was not affected by it.

13. The barometer is sometimes not moved by very great changes in the air; as December 18th, an extraordinary bright and clear day, and the next following quite dark, some snow and rain falling, but the mercury kept at the same height. So in high winds and calms, the same.

14. December 16, 1665, was a clear cold day, with a very sharp and strong east wind; the mercury very near 30 inches high; about three in the afternoon he saw a large black cloud approaching from the east and south-east, with the wind at east. The mercury changed not that day nor the following; the stars and most of the sky were very bright and clear till nine o'clock; and then the sky was suddenly overcast, yet no change of weather happened. December 17th, the frost held, and it was a clear day till about two o'clock in the afternoon; and then many thick clouds appeared low in the west; yet no change of weather; the wind, frost, and quicksilver the same. December 18th, the mercury fell almost $\frac{1}{4}$ of an inch, and yet the sky and air were clear, bright, and cold, with an east wind; but accidentally sending his servant abroad, he discovered the remote hills, about 20 miles off, covered with snow.

15. He seldom observed the change to be very great at any one time; so that he once wondered to see that in one day it subsided about $\frac{3}{4}$ of an inch.

16. January 13, 1665-6, the mercury stood a quarter above 30 inches, as it did also the day before; yet both very dark and cloudy, and sometimes very thick and misty air; which is an uncommon case, for generally it stands higher in the clearest settled weather than in such cloudy and misty fogs. This thick air and darkness lasted above a week; lately more cold, and east and north-east wind.

17. In January 1665-6, for many days it continued very dark, so that great rains were apprehended, and though sometimes thick mists arose and some small rain fell, yet the mercury stood at a great height; which indicated no great change of weather, and he was not disappointed.

The following by the same, from other Numbers of the Transactions, viz. Nos 10 and 55.

18. If the mercury rises a good height after the fall of rain, as sometimes it does, he then looks for a settled serenity; but if it falls, then he expects a series of broken and showery weather.

19. The weather and our bodies are more chill, cold, and drooping, when the mercury is lowest and the air lightest. Air being to us, what water is to fishes.

20. The lowest descent of the mercury, in all his observations, was October 26, 1665, in the evening; when it was very near at 27½ inches, as he found by his following notes: October 25, morning, mercury at 28½ inches, great storms and much rain. October 26, morning, mercury at 28, winds quiet, thick dark clouds. October 26, evening, mercury at 27½; that day and the following days the weather was variable, and there were frequent rains.

21. He set a wind-vane of a large brass streamer over the place where the mercurial tube stood, pointing to a board indented in the margin, that he could take at a good distance the 32 points of the wind, with the half and quarter points. It would be proper to have an index of winds.

22. By change of weather and wind, the mercury sunk, since March 12th, above an inch; and March 18th at night, by rain and south wind, half an inch.

23. He found the quicksilver, December 13, 1669, higher than ever he observed it, it being half an inch above 30. It continued the 14th, and a part of the 15th at about that height, and sometimes higher by an eighth or tenth part of an inch. For this barometer he had two glass tubes in one vessel of stagnant mercury; and both of them agreed in this indication. The weather was at first very bright and clear, and there was a mild frost: The air was very still, and no wind stirring, and by the wind-vane, the wind stood east, all the first day, viz. December 13th; on the 14th it blew a little from north-west, and returned again to the east or north-east. During this shifting of the winds the mercury descended a little; and again after the settling of the wind, the mercury ascended a little higher than it had done the preceding day.

The house and study where the barometer was kept stood on the side of a hill, on the higher side of the country, and nearly on a level with the head of a river that falls into the Severn sea, about 20 or 30 miles westward of Bristol, so that they cannot be much above the level of the sea.

Some Observations on Vipers. By Sig. REDI. *N° 9, p.* 160.

These observations are taken from the account given in the Journal des Sçavans for January 1665-6, of Francis Redi's treatise, wherein it is stated—

1. That the poison of vipers is neither in their teeth, nor in their tail, nor in their gall; but in two vesicles or bladders which cover their teeth, and which being compressed when the vipers bite, emit a certain yellowish liquor, that runs along the teeth and poisons the wound.* Whereof he gives this proof, that he hath rubbed the wounds of many animals with the gall of vipers, and pricked them with their teeth,† and yet no considerable ill accident followed upon it; but that as often as he rubbed the wounds with the said yellow liquor not one of them escaped.

2. Whereas commonly it hath hitherto been believed that the poison of vipers being swallowed, was present death: this author after many reiterated experiments is said to have observed, that in vipers there is neither humour nor excrement, nor any part that being taken into the body kills: And he asserts that he hath seen men eat, and has often made brute animals swallow, all that is esteemed most poisonous in a viper, yet without the least mischief to them; agreeably to the doctrine of the ancients. *Venenum serpentis, ut quædam etiam venatoria venena, non gustu, sed in vulnere nocent.* Celsus.‡

* We owe to Redi the discovery of the fluid which constitutes the viper's poison; but he mistook its exact situation when he placed it in the membrane which covers the upper fangs (the canine teeth); whereas it is lodged (as our countryman Mead has shown) in a peculiar bag or secretory vesicle seated at the basis of those teeth. Moreover, it escaped Redi that the venomous fangs were perforated, and that when the animal bites, the poison is squeezed into the wound, not along the outside of the teeth, but through their interior, along the conduits or perforations which terminate near their apices or points. Mead has described only a single perforation in each of the venomous fangs; but Fontana asserts there are two in each. See the last mentioned author's Traité sur le Venin de la Vipere, tom. i. p. 6—8.

† This assertion of Redi's is not accurate. It is evident that, in the before-mentioned perforations or canals of the canine teeth, there will always remain some of the venomous fluid, and therefore there will always be a greater or less risk of being poisoned when pricked by them. In two experiments Fontana killed animals by wounding them with a viper's tooth, several hours after it had been drawn out of the head.

‡ This assertion is contradicted by the later experiments of Fontana, who having forced into the œsophagus of a pigeon a tea spoonful of the poison, it was quickly seized with strong convulsions and died in less than six minutes. He doubts not it would produce the same fatal effects, when taken into the stomach, upon man and other larger animals, provided it were swallowed in sufficient quantities. Traité sur le Venin, &c. tom. ii. pp. 308, 309. It would appear, however, to be true, as Redi has stated, that the flesh of animals killed by vipers may be safely eaten, and the wound inflicted by their bite sucked with impunity; because, in either of these ways, the dose of the poison swallowed will be too inconsiderable to produce, in so large an animal as man, any powerful effect.

3. He adds, that although Galen and many modern physicians affirm, that there is nothing which causeth so much thirst as viper's flesh, yet he hath experienced the contrary.

4. There is no purging virtue at all (he asserts) in the salt of vipers, which some chemists have held in such high esteem.

5. He denies what Aristotle asserts, and what Galen says he so often tried, that the spittle of a fasting person kills vipers, and ridicules many other particulars concerning the antipathy of vipers unto certain things, their manner of conception and generation, and several other properties commonly ascribed to them.

An Earthquake near Oxford, Anno 1665. *By Dr.* Wallis,* *and Mr.* Boyle. *N°* 10, *p.* 166, *and N°* 11, *p.* 179.

On the 19th of January, 1665-6, towards the evening, at divers places near Oxford, was felt a small earthquake. At Oxford it was not noticed as an earthquake; though the Doctor says, that about that time he was sensible of a kind of odd shaking or heaving in his study, which he supposed owing to carts and coaches, though a little different from what is usual on these occasions.

It was perceived at Blechington, about five miles northward of Oxford, and also at Bostol, Horton, Stanton St. John's, and so towards Whately, which is four miles to the east of Oxford: It was not felt at the same time at all these places, but moved successively from Blechington to Whately.

* Dr. John Wallis, one of the most early and active members of the Royal Society, was born Nov. 23, 1616, at Ashford, in Kent. He was educated at Cambridge, where, about the year 1640, he entered into orders, and became fellow of Queen's College. Academical studies being much interrupted, by the civil wars, in both the universities, many learned men from them resorted to London, and formed assemblies there. Dr. Wallis belonged to one of these, the members of which met once a week, to discourse on philosophical subjects; and this society gave rise to the incorporated Royal Society, of which he thus became one of the first members. In politics Dr. Wallis managed so well as to keep fair with both parties; he was made professor of mathematics at Oxford, in 1649, by the parliament's visitors, and held that and other appointments from the king after the restoration. In 1658, he was chosen, at Oxford, Custos Archivorum of the university; and was appointed one of the divines for revising the book of common prayer. He was universally learned, but his application was chiefly to theology and mathematics, on both of which subjects he wrote many treatises; he was very skilful in deciphering letters written in secret characters, in which capacity he was of great service to the state. He was skilled in the art of teaching persons born deaf to speak. He died at Oxford, Oct. 28, 1703, in the 87th year of his age. Dr. Wallis was always a very useful member of the Royal Society, and he kept up a literary correspondence with many learned men in an amicable manner; but with some he had obstinate disputes, as with Hobbes, Stubbe, and others. It is said he was of a vigorous constitution, and of a mind strong, serene, and not easily ruffled or discomposed.

Mr. Boyle riding between Oxford and a lodging he had about four miles from that town, in that short space of time, from a settled frost, the wind turned and it began to rain. Soon after his getting home, he felt a manifest trembling in the house, which stands high comparatively with Oxford. But he should not have taken notice of it as an earthquake, had it not been perceived by the people of the house. Soon after there happened a brisk storm; on which he sent to make inquiry at a place called Brill, which standing higher might be supposed more liable to the effects of the earthquake; and he was informed that it was very considerable there; and that a gentleman's house in the neighbourhood shook very much, so that the stones in the parlour manifestly moved to and fro. The hill on which this Brill stands, is stored with mineral substances of several sorts. Mr. Boyle adds, that he has been told that this earthquake reached a great many miles.

Observations on the Barometer. By Dr. WALLIS. *N°* 10, *p.* 166.

The Doctor never observed the quicksilver higher than 30 inches, nor lower than 28, at least within $\frac{1}{10}$ of an inch of these numbers, either over or under.

In thick foggy weather, he found the quicksilver rise; which he ascribes to the heaviness of the vapours in the air.

In sun-shiny weather it rises also, and commonly the clearer the weather the higher it is; which may be owing partly to the vapours raised by the sun and increasing the weight of the air; partly to the heat which adds to the elasticity of the air; which latter he mentions, because in sun-shiny weather, which became afterwards cloudy for an hour or two, the quicksilver has fallen; and then on the sun's breaking out again, it has risen as before.

In rainy weather it falls, because the air is light in proportion to the quantity of vapours that falls; and also in snowy weather, but not so much as in rain; and sometimes it has fallen upon a hoar-frost in the night.

In windy weather it generally falls, and more discernibly than in rainy, owing possibly to the winds moving the air laterally; and thereby preventing its pressure downwards; and he never found it lower than in high winds.

He observed the quicksilver fall without any visible cause, but upon looking abroad, he found it had rained at some distance; whereby the heavier air might have in part discharged itself on the lighter.

The Rotation of Jupiter on his Axis. By Mr. HOOK *and M.* CASSINI. *N°* 10, *p.* 171, *and N°* 82, *p.* 4039.

About nine o'clock at night, May 9th, anno 1664, Mr. Hook observed, with a good 12 foot telescope, a small spot in the largest of the three obscurer belts of Jupiter; and, observing it from time to time, he found that within two hours after, the said spot had moved from east to west, about half the length of the diameter of Jupiter.

According to M. Cassini there are two sorts of spots to be seen in the disk of Jupiter; the one being only the shadows of his satellites, the other sort resembling those that are seen in the moon; and they are perhaps of the same nature with those called belts. They move from the eastern to the western limb; their apparent motion is unequal, and swifter near the centre than the circumference; and they are never seen so well as when they approach the centre; for in approaching the circumference they become very narrow, and almost imperceptible; which seems to argue that they are flat and superficial.

Among these spots, there is none so observable as that situated in the northern part of the southern belt. Its diameter is $\frac{1}{10}$ of Jupiter's; its centre when nearest is distant from that of Jupiter about $\frac{1}{3}$ of the semidiameter of that planet.

M. Cassini, after many observations during the summer 1665, found that the period of its apparent revolution is 9 hours 56m. He continued to observe this spot till the beginning of 1666, when Jupiter approached to the beams of the sun; but after he got out of them it was difficult to be discerned: This giving grounds to think that it might be of the nature of the solar spots, which appearing for a while, disappear for ever, M. Cassini intermitted his observations.

But, Jan. 19, 1672, N. S. observing Jupiter at 4$\frac{3}{4}$ hours in the morning, he perceived in the same place of his disk the figure of the same spot, adhering to the same southern belt. It had already gone over the half of this belt, and he saw it advance gradually towards the western limb, to which it seemed very near at 6$\frac{1}{4}$ hours.

By the celerity of its motion near the centre, and by the place where he had begun to see it, he judged it might have been in the middle of the belt at 4 hours 35m. in the morning. And as he set about making ephemerides of its motion for 1672, he perceived that in those he made for 1666, this spot had been in the middle of Jupiter the same day, viz. the 19th of Jan. at the same hour, so that in six years, of which one is a bissextile, it is found to have made, in respect of the earth, at least 5294 revolutions, each of 9 hours, 55m. 58 sec. one revolution with another; and at most, 5295 revolutions of 9 hours, 55m. 51 sec. forasmuch as he was assured of the preciseness of one mean revolution to $\frac{1}{8}$ of a minute.

Till that time he never observed an immediate return of this spot after 9 hours 56m.; because that after the appearing of the spot, Jupiter had not continued long enough above the horizon to observe him with due distinctness. But the night after, March 1st, at $7\frac{1}{2}$ hours in the evening, he saw this spot in the middle of the belt, and at 5 hours 26m. in the morning, he saw it again return precisely to the same place.

An Account of some Books, lately published. *N° 10, p.* 173.

I. Hydrostatical Paradoxes, made out by New Experiments, for the most part physical and easy, by the Honourable Robert Boyle. This treatise was occasioned by the perusal of the learned M. Pascall's tract, Of the Equilibrium of Liquors, and of the Weight of the Air.

II. Nicolai Stenonis de Musculis et Glandulis Observationum specimen; cum duabus Epistolis Anatomicis.

III. Regneri de Graeff, de Succi Pancreatici Natura et usu, Exercitatio Anatomico medica.

The accounts here given of these three books, being a description of the contents, are omitted, as of no use at present.

Observations and Directions concerning the Barometer. By Mr. BOYLE. *N° 11, p.* 181.

It will be requisite for observers with this instrument to give an account of the situation of the place where the barometers stand; because we may thus not only be capable of judging whether the instruments were duly constructed, but also because the observations may disagree, even when the atmosphere is in the same state, as to weight, if one of them stand in a higher part of the country than the other. For Mr. Boyle found, by comparing two barometers which he had, the one at Oxford, the other at Stanton St. John's, that though the former was a very good one, and the latter very carefully filled, yet because at Stanton, which is the higher ground, the incumbent part of the atmosphere must be lighter, than at Oxford, which is the lower place, there is generally between 2 and 3 eighths of an inch difference.

But as most barometrical observations are subject to exceptions, so he found that to be the case with the preceding. For riding one evening from Oxford to Stanton, and before he took horse looking on the barometer, he was surprized to find at his coming to the latter place, which was at no great distance, and also considering the shortness of the time, which was less than an hour and a half, that the barometer at Stanton was short of its usual height from the other near a quarter of an inch; though, the weather being fair and calm, there appeared no manifest change in the air; and though also since that time the mercury in the two instruments had for the most part risen and fallen together as before.

Mr. Boyle has observed the heights of the mercury to be greatest in droughts, which he supposes owing to the elevation of steams from the earth, which may gradually increase the weight of the atmosphere; for March 12, 1665-6, at Oxford the quicksilver was higher than had been observed in England, viz. about $\frac{5}{16}$ above 30 inches; but upon the first considerable showers that interrupted the long drought, he foretold many hours before, that the mercury would be very low, and so he found it at Stanton to fall $\frac{3}{8}$ below 29 inches, there being also a blustering wind with the rain.

It is rather difficult to settle any general rule about the rising and falling of the mercury; yet in those parts one that seems to hold oftenest is, that when high winds blow the mercury is lowest, and yet even this sometimes fails.

General Heads for a Natural History of a Country. By Mr. BOYLE. *N° 11, p.* 186.

The things to be observed in such a history may be variously divided: As into supraterraneous, terrestrial, and subterraneous. But we will at present distinguish them into those things that respect the heavens, or concern the air, the water, or the earth.

1. To the first sort of particulars belong the longitude and latitude of the place, and consequently the length of the longest and shortest days and nights, the climate, the parallels, &c.; what fixed stars are, and what are not seen there.

2. Concerning the air, may be observed its temperature, as to the first four qualities and the measures of them: its weight, clearness, refractive power; its subtilty or grossness; its abounding with or wanting salts, its variations according to the seasons of the year, and the times of the day; what duration the several kinds of weather usually have; what meteors it mostly produces, and in what order they are generated; and how long they usually last; especially, what winds it is subject to; whether any of them be stated and ordinary, &c. What diseases are epidemical; what is the usual salubrity or insalubrity of the air; and with what constitutions it agrees better or worse than others.

3. Concerning the water, may be observed the sea, its depth, degree of saltness, tides, currents, &c. Next rivers, their width, length, course, inundations, goodness, lightness of waters, &c. Then lakes, ponds, springs, and especially

mineral waters, their kinds, qualities, virtues, and how examined. To the waters belong also fishes, their kinds, whether salt or fresh water fish; their quantity, size, goodness, seasons, haunts, peculiarities of any kind, and the ways of taking them, especially those that are not purely mechanical.

4. In the earth may be observed,

1. Itself. 2. Its inhabitants and its productions, both external and internal.

First, In the earth itself may be observed, its dimensions, situation, east, west, north and south: its figure, its plains and valleys, and their extent; its hills and mountains, and their height; and whether they lie scattered or in ridges, and in what directions they run, &c. What promontories, fiery or smoaking hills, &c. What the magnetical declination is in several places, and the variations of that declination in the same place: what the nature of the soil is, whether clay, sandy, &c. or good mould; and what grains, fruits, and other vegetables, do the most naturally agree with it: also, by what particular arts the inhabitants improve the advantages and remedy the inconveniences of their soil?

Secondly, There must be given a careful account of the inhabitants themselves, particularly their stature, shape, colour, features, strength, agility, beauty, complexions, hair, diet, inclinations, and customs. Of the women, there may be observed their fruitfulness or barrenness, their hard or easy labour, &c. What diseases both women and men are subject to, and unusual symptoms attending them.

As to the external productions of the earth, the inquiries may be such as these: What grasses, grains, herbs, flowers, fruit-trees, timber-trees, coppices, groves, woods, forests, &c. What peculiarities are observable in any of them: What soils they best thrive in. What animals the country has, either wild or tame.

The internal productions or concealments of the earth, are here understood to be the riches that lie hid under the ground, and are not already referred to other inquiries: what sorts of minerals and quarries the country affords, and the particular conditions both of the quarries and the stones: also, how the beds of stone lie, in reference to north and south, &c. What clays and earths it affords, as tobacco pipe-clay, marls, fullers-earths, earths for potters wares, boluses and other medicated earths: What other minerals it yields, as coals, salt-mines, or salt-springs, alum, vitriol, sulphur, &c. What metals the country yields, and a description of the mines, their number, situation, depth, signs, waters, damps, quantities of ore, goodness of ore, extraneous things, and ways of reducing their ores into metals, &c.

Preserving of Ships from being Worm-eaten. N° 11, p. 190.

There is in the Indian Seas a kind of small worms * that fasten themselves to the timber of the ships, and so pierce them that they take in water every where; and so weaken the wood, that it is almost impossible to repair them. Many things have been tried to prevent this evil, but without success. Some have lined their ships with deal, hair and lime, &c. but, besides that this does not altogether prevent the worms, it much retards the ship's way. The Portuguese scorch their ships to such a degree, that in the quick works there is formed a coally crust of about an inch thick. But this is dangerous, as often burning the whole ship; yet the reason why worms do not so destroy Portugal ships, is conceived to be the exceeding hardness of the timber employed by them.

A person in London suggests, that the tar extracted out of sea coals may be a good remedy against these noxious worms.†

Account of a Book lately published, entitled, The Origin of Forms and Qualities, illustrated by Considerations and Experiments. By the Hon. ROBERT BOYLE. N° 11, p. 191.

This curious and excellent piece is a kind of introduction to the principles of the mechanical philosophy, explaining, by observations and experiments, what may be according to such principles conceived of the nature and origin of qualities and forms; the knowledge of which either makes or supposes the fundamental and useful part of natural philosophy. In doing of which, the author writes rather for the corpuscularian philosophers, as he is pleased to call them, in general, than any party of them, keeping himself thereby disengaged from adopting an hypothesis, with which perhaps he is not so thoroughly satisfied, and of which he does not conceive himself to be under the necessity of making use here; and accordingly forbearing to employ arguments that are either grounded on, or suppose atoms, or any innate motion belonging to them; or that the essence of bodies consists in extension; or that a vacuum is impossible; or that there are such *globuli cœlestes,* or such a *materia subtilis,* as the Cartesians employ to explain most of the phænomena of nature.

The remainder of this memoir consists of a rather minute description of the contents of this ingenious book, which is now in the possession of all the learned.

New Observations on the Planet Mars. By Mr. HOOK. N° 11, p. 198.

There was very lately produced a paper, containing some observations, made

* The teredo navalis. Linn.

† The most effectual method of preserving the bottoms of ships is that now in use, sheathing them with copper.

by Mr. Hook, on the planet Mars; in the face of which he affirmed that he had discovered, in the late months of February and March, that there are several maculæ or spotted parts, changing their place, and not returning to the same position till the next ensuing night, near about the same time. Whence it may be collected that Mars, as well as Jupiter and the Earth, &c. revolves about his own axis.

Preserving of Birds taken out of the Egg. By Mr. BOYLE. *N° 12, p.* 199.

In order to observe the process of nature in the formation of a chick, Mr. Boyle opened the eggs at different periods after incubation, and carefully taking out the embryos, embalmed each of them in spirit of wine in a distinct glass carefully stopped.

In making these experiments some circumstances are to be observed; one is, that there be generally mixed with the spirit of wine a little spirit of sal ammoniac. The other, that it is proper to put the fœtuses for some time in ordinary spirit of wine, to wash off the looser filth, and then let them soak in the same kind of spirit or better, that the fœtus, being removed into more pure and dephlegmated spirit of wine, may not discolour it.

Of an unusual Method of propagating Mulberry Trees in Virginia. N° 12, p. 201.

I have planted ten thousand mulberry trees, and hope within two or three years to have good silk by this means. My method, which is uncommon, accelerates their growth two or three years sooner than if they were sown in seed. I intend likewise to plant them as thick as hedges, like currants or gooseberries. By this method they will always be young tender plants, and be easily cut in great quantities with a pair of garden-sheers, whereby one man may gather as much as four could do when they are in trees at distance from each other. But possibly the best way would be to sow some acres with mulberry seed, and to cut them with a scythe, and thus keep them under ever after.

To make a Glass of a small Plano-convex Sphere, to refract the Rays of Light to a Focus at a greater Distance than usual. By Mr. HOOK. *N° 12, p.* 202.

Prepare two glasses, the one exactly flat on both sides, the other flat on one and convex on the other, of any sphere you please. Let the flat glass be a little broader than the other; and let them both be put into a ring of brass, and so fastened with cement, that their plane surfaces may be exactly parallel, and the convex side of the plano-convex glass lie inward, so as not to touch the flat of the other. Then fill the space between, by a small hole in the side of the brass ring, with water, or oil of turpentine, or spirit of wine, or saline liquors, &c. after which stop the hole with a screw. Then, according to the different refraction of the liquors, shall the focus be longer or shorter.

This is but one instance, among many, of the possibility of making a glass, ground in a smaller sphere, to constitute a telescope of a much greater length. Though I must add, that of spherical optic glasses, those are the best which are made of the greatest sphere, and whose substance has the greatest refraction.

Shining Worms in Oysters. By M. Auzout. N° 12, p. 203.

These observations occur in the French journal of April 12, 1666, in two letters, written by M. Auzout to M. de la Voye; the substance of which may be reduced to the following particulars:

M. Auzout causing more than 20 dozen of oysters to be opened by candle light, saw on removing the light shining worms in them of three sorts. One sort was whitish, having 24 or 25 feet on each side, forked; a black speck on one side of the head (taken by him for a chrystallin), and the back like an eel, stripped of her skin. The second red, and resembling the common glow-worm, with folds on their backs, and feet like the former; and with a nose like that of a dog, and one eye in the head. The third sort was speckled, having a head like that of a sole, with many tufts of whitish hair on the sides of it.

Among them he saw two more firm than the rest, which shone all over; and when they fell from the oyster, twinkled like a great star, shining strongly, and emitting rays of a violet-light by turns, for the space of 20 seconds. Which scintillation the observer imputes to this, that those worms being alive, and sometimes raising their head, sometimes their tail, like a carp, the light increased and lessened accordingly; seeing that when they ceased to shine, by bringing back the light, he found them dead.

Some Observations on the [medical] Effects of Touch and Friction. By Mr. Oldenburg. N° 12, p. 207.

Touch and friction (the author observes) have been considered by some as no inefficacious agents in the cure of many diseases and infirmities.

1. The illustrious Lord Verulam, in his History of Life and Death (Hist. 6. § 3.) observes, that the motion and warmth excited by friction draw forth

into the parts new juice and vigour: And, canon XIII, he affirms, that frictions conduce much to longevity.

2. The Hon. Robert Boyle, in his Usefulness of Experimental Philosophy (§ 2. ch. 15.) considering the body of a living man or any animal as an engine so composed, that there is a conspiring communication betwixt its parts, by virtue whereof a very slight impression of adventitious matter upon some one part may be able to work on some other distant part, or perhaps on the whole engine, a change far exceeding what the same adventitious matter could do upon a body not so contrived:—Representing (I say) an animal in this manner, and thence inferring how it may be altered for the better or worse by motions or impulses confessedly mechanical, observes, how some are recovered from swooning fits by pricking; others grow faint and vomit by the motion of a coach; others by the agitation of a ship, recovering by rest and going a shore. Again, how in our stables a horse well curried is half fed, &c. The same writer upon the authority of Piso, refers to the Brasilian empirics, whose rude frictions do strange things, both in preserving health and curing diseases; curing cold and chronic complaints by friction, as they do acute disorders by unction.

3. In this section we have (on the authority of Dr. John Beale) an account of a very large wen of two or three years standing, being cured by the application of a dead man's hand to it; and of warts being removed in another person by the same means.* Also, of a gentlemen being cured of a great pain in his feet (probably of a gouty nature) by having them licked night and morning by a spaniel. In the last part of these observations, we are told of a blacksmith, who possessed the particular faculty of causing vomitings by stroking the stomach, of giving stools by stroking the belly, and of appeasing the gout and other pains by stroking the parts affected.†

Some Particulars communicated from Abroad, concerning the Permanent Spot in Jupiter; and a Contest between two Artists on Optic Glasses, &c. N° 12, p. 209.

Eustachio Divini, says the informer, has written a long letter, pretending that the permanent spot‡ in Jupiter was first of all discovered with his glasses; and

* The influence of this uncertain and disgusting remedy in the removal of glandular tumors is referable to the imagination; through the medium of which a considerable effect is sometimes produced upon the absorbent system.

† Thus it appears that animal magnetism is of a more remote date than modern empirics imagine.

‡ See Number 1, of these Transactions; by the date of which it will appear, that that spot was observed in England, a good while before any such thing was so much as heard of elsewhere.

that P. Gotignies is the first that has from thence deduced the motion of Jupiter about his axis; and that Signior Cassini opposed it at first; to whom the said Gotignies wrote a letter of complaint on this matter.

The same Eustachio pretends likewise, that his great glasses excel those of Campani; and that in all the trials made with them, they have performed better; and that Campani was not willing to do what was necessary for properly comparing the one with the other, viz. To put equal eye-glasses in them, or to exchange the same glasses.

The said Divini affirms also, that he has found out a way to know whether an object-glass be good or not, by only looking upon it, without trying. This would be of good use, especially if it should extend so far as to discern the goodness of such a glass, while it is yet on the cement.

An Account of Dr. Sydenham's Book, entitled, Methodus Curandi Febres, Propriis observationibus superstructa. N° 12, p. 210.

Dr. Sydenham's work being in the hands of every medical practitioner, it would be superfluous to insert an analysis of it here; but on this occasion it may be proper to notice that the merit of this great physician and original writer consist, in his accurate descriptions of what he terms epidemic constitutions; in his history and treatment of the small-pox; and in his rejection of the hot and cordial method of treatment in this eruptive disorder as well as in fevers in general. His theoretical reasonings however are by no means satisfactory; and it has been justly observed by many medical writers, that he carried one part of his cooling method of cure (viz. venesection) to too great a length. A short account of his life is prefixed to the English translation of his works by Swan.

Certain Problems in Navigation. By Mr. Nicholas Mercator. N° 13, p. 215.*

The line of artificial tangents, or the logarithmical tangent-line,† beginning at 45 degrees, and taking every half degree for a whole one, is found to agree

* Nicholas Mercator, an ingenious mathematician, and a learned member of the Royal Society, was a native of Holstein, in Germany; but he spent most of his time in England, where he died in the year 1690, at only 50 years of age. He was the author of many works in geometry, geography, astronomy, astrology, logarithms, &c.

† It does not appear by whom, nor by what accident, was discovered the noted property which is the subject of the above memoir by Mercator, being the analogy between a scale of logarithmic tangents, and Wright's protraction of the nautical meridian line, which consisted of the sums of the secants. It appears however to have been first published, and introduced into the practice of navigation, by Henry Bond, who mentions this property in an edition of Norwood's Epitome of Navigation, printed about 1645; and he again treats of it more fully in an edition of Gunter's works,

pretty nearly with the meridian-line of the sea chart; both of them growing, as it were, after the same proportion. But the table of meridional degrees, being calculated only to every sexagesimal minute of a degree, shows some small difference from the said logarithmical tangent-line. Hence it may be doubted, whether that difference does not arise from that little error which is committed by calculating the table of meridional degrees only to every minute.

Mr. Oughtred, in chap. vi. of his Navigation, mentions a method discovered by himself, by which it may be proved that the small parts of the meridian may not be one minute (which on the face of the earth answers to above an English mile) nor the hundred-thousandth, or, if necessary, the millionth part of a minute, scarce exceeding one fifteenth part of an inch: which thing, he says, he is able to perform in tables, to the radius 10000000; yet nothing at all differing either in their form or manner of working from those that are now commonly in use.

How this is to be done, this author has not made known to the public. And, though such tables to the radius 10000000 had been brought to light, yet would they not be sufficient to prove the identity or sameness of the said two lines, as to continue the comparison between them as far as the one of them, viz. the logarithmical tangent-line, is already calculated, that is, to ten places, besides the characteristic.

Now, therefore, if a certain rule could be produced, by which the agreement or disagreement of the said two lines might be shewn, not only to that extent of places to which that tangent line is already calculated, but also to as many more as the same may be yet further extended to *in infinitum*, surely that rule would not only save us the labour of making tables to the radius 10000000, but also the helix or spiral line of the ship's course would be reduced to a more precise exactness than ever was pretended by him: and this most noble and useful science (as he justly calls it) which is the bond of most distant countries,

printed in 1653, where he teaches, from this property, how to resolve all the cases of Mercator's or Wright's sailing by the logarithmic tangents, independent of the table of meridional parts. This analogy had only been found to be nearly true by trials, but not demonstrated to be a strict mathematical property. Such demonstration seems to have been first discovered by Mercator, the author of the above memoir, who, wishing to make the most advantage of this and another concealed invention in navigation, in the above paper invites the public to enter into a wager with him, on his ability to prove the truth or falsehood of the supposed analogy. But this mercenary proposal seems not to have been taken up by any one, and Mercator reserved his demonstration. The proposal however excited the attention of mathematicians to the subject itself, and a demonstration was not long wanting. The first was published about two years after, by James Gregory, in his Exercitationes Geometricæ, and from thence, and other similar properties, there demonstrated, he shows how the tables of logarithmic tangents and secants may easily be computed from the natural tangents and secants.

and the consociation of remotest nations, would attain its full lustre and perfection.

Besides that the same rule would also discover a far easier way of making logarithms than ever was practised or known, and therefore might serve, whenever there should be occasion, to extend the logarithms beyond the number of places already known.

Moreover, such a rule would enable men to draw the meridian line geometrically, that is, without tables or scales: which indeed might also be done by setting off the secants of every whole or half degree, if there were not this inconvenience in it, that a line, composed of so many small parts, would be subject to many errors, especially in a small compass.

The same rule also will serve to find the course and distance between two places assigned, as far as practice shall require it; and that without any table of meridional parts, and yet with as much ease and exactness.

And as all these things depend on the solution of this question, whether the artificial tangent-line be the true meridian-line ? I undertake, by God's assistance, to resolve the said question. And in order to let the world know with what readiness and confidence I undertake it, I am willing to lay a wager against any one or more persons that have a mind to engage, for so much as another invention of mine, which is of less subtlety, but of far greater benefit to the public, may be of worth to the inventor.

For the great advantage which all merchants, mariners, and consequently the commonwealth may receive from this other invention, is, in my judgment, highly valuable; as it will often make a ship sail against even a contrary wind, and yet as near to the place intended as if the wind had been favourable: or will enable one to gain something in the intended way whether the wind be fair or not (except only when you go directly south or north), but the advantage will be most where there is most need of it, that is, when the wind is contrary: So that one may very often gain a fifth, fourth, third part, or more of the intended voyage, according as it is longer or shorter, but always most in a longer voyage.

All this the proposer is to make good by the verdict of some able men, who also may give a guess what this latter invention may be worth to the owner: And for so much, and no more, he will stand engaged against any one or more persons, that he will and shall resolve the question above-mentioned, viz. Whether the artificial tangent-line be the true meridian line; and if not, then he will lose, and transfer to the other party the whole benefit of the last mentioned invention. But if, on the contrary, he prove or disprove the identity of the said two lines, to the judgment of some able mathematicians, that then so much money be paid him by the other party as the said invention was valued at.

A new Contrivance of a Wheel-Barometer. By Dr. Hook. *N° 13, p.* 218.

This is only an easy way of applying an index to any common baroscope, whether the glass be only a single tube, or have a round bolthead at the top. And by it the variation in the altitude of the mercurial cylinder, which at most is hardly three inches, may be made as distinguishable as if it were three feet, or three yards.

The manner is evident by figure 5, pl. 1, where A B C represent the tube, which may be either blunt or with a head, as A. This is to be filled with quicksilver, and inverted as usual into a vessel of stagnant mercury of the shape I K, that is, having its sides about three or four inches high, and the tube equally wide both above and below; and, if possible, of equal capacity with the hollow of the tube about B: for then the quicksilver rising as much in the hollow of I, as it descends at B, the difference of the height in the receiver I, will be just half the usual difference. And if the receiving vessel I K have a larger cavity, the difference will be less; but if less, the difference will be greater: but whether the difference be made more or less, it is no great matter, since by the contrivance of the wheel and index, the least variation may be made as sensible as is desired, by diminishing the width of the cylinder E, and lengthening the index F G, according to the proportion required.

Of four Suns and two uncommon Rainbows observed in France. *N° 13, p.* 219.

On the 9th of April 1666, about half an hour past 9, there appeared three circles in the sky. One of them, S C H N, fig. 6, pl. 1, was very large, a little interrupted, and white every where, without the mixture of any other colour. It passed through the middle of the sun's disk, and was parallel to the horizon. Its diameter was above 100 degrees, and its centre not far from the zenith A.

The second D E B O, was much less, and deficient in some places, having the colours of a rainbow, especially in that part which was within the great circle. It had the true sun for its centre.

The third H D N, was less than the first, but greater than the second; it was not entire, but only an arch or portion of a circle, whose centre was far distant from that of the sun, and whose circumference about its middle D was joined to that of the least circle, intersecting the greatest circle at its two extremities H N. In this circle were discerned also the colours of a rainbow, but they were not so strong as those of the second.

At the part where the circumference of this third circle closed with that of the second, there was a great brightness of rainbow-colours mixed together: And at the two extremities, where this second circle intersected the first, appeared two parhelias or mock-suns H N; which shone very bright, but not so bright or so well defined as the true sun. The false sun H towards the south, was larger, and far more luminous than that towards the east.

Besides those two parhelias which were on the two sides of the true sun, in the intersection of the first and third circle, there was also upon the first great circle, a third mock sun C, situated to the north, which was less and less bright than the two others. So that at the same time there were seen four suns in the heavens. There was also a very dark space I, between D and R.

This appearance is considered as one of the most remarkable that can be seen, by reason of the eccentricity of the circle H D N, and because the parhelia were not in the intersection of the circle D E B O with the great circle S C H N, but in that of the semicircle H D N. Which are different from the position of those five suns seen at Rome on March 29, 1629, between two and three o'clock A. M, two of them appearing in the intersection of a circle passing through the sun's disk, with another that was concentric with the sun, as in fig. 7.

As for the two uncommon rainbows, they appeared at Chartres the 10th of August 1665, about half an hour past six in the evening; crossing each other nearly at right angles, as seen in fig. 8.

That opposite to the sun, in the usual manner, was more deeply coloured than that which crossed it; though the colours of the first Iris were not indeed so strong as they are seen at other times.

The greatest height of the stronger rainbow was about 45 degrees; the feebler rainbow lost one of its legs by growing fainter, about 20 degrees above the stronger; and the leg below appeared continued to the horizon.

The fainter seemed to be a portion of a great circle; and the stronger but a portion of a small circle, as usual.

The sun at their appearance was about six degrees high above the horizon: and the river of Chartres, which runs nearly from south to north, was between the observer and the rainbow; and he stood level with this river, at the distance of 150 paces from it.

A Relation of an Accident by Thunder and Lightning at Oxford. By Dr. Wallis. N° 13, p. 222.

Two scholars of Wadham College being in a boat, without a waterman, and having just pushed off from shore at Medley to return home, were by a stroke of lightning, as they stood at the head of the boat, both forced out of the boat into the water. One of them was instantly struck dead, no appearance of life being discernible in him, though he was taken out of the water after he had been scarcely a minute in it. The other was stuck fast in the mud (with his feet downwards and his upper parts above water) like a post, not able to help himself out; but, except a present stunning or numbness, had no other hurt, but was so confused, that he knew not how he came there out of the boat, and had no recollection of the thunder and lightning. He was very feeble and faint, and though he was immediately put into a warm bed, he had not thoroughly recovered by the next night; and whether he afterwards recovered or not, was not known.—The body of him who was killed was examined the next morning by Dr. Willis, Dr. Mallington, Dr. Lower, and myself, with some others. We found no wound at all in the skin; the face and neck were swart and black, but not more than might be ordinary by the settling of the blood. On the right side of the neck was a little blackish spot about an inch long, and about $\frac{1}{4}$ of an inch broad, and was as if it had been seared with a hot iron; and as I remember, one somewhat bigger on the left side of the neck, below the ear. Straight down the breast, but towards the left side of it, was a large place about three quarters of a foot in length, and about two inches in breadth, in some places more, in some less, which was burnt and hard like leather burnt with the fire, of a deep blackish red colour, not much unlike the scorched skin of a roasted pig. On the forepart of the left shoulder there was a similar spot about the size of a shilling, but that in the neck was blacker, and seemed more seared. From the top of the right shoulder, sloping downwards towards that place in his breast, was a narrow line of the like scorched skin, as if somewhat had come in at the neck and had run down to the breast, and there spread broader. The buttons of his doublet were for the most part torn off, and the collar thereof just over the forepart of the left shoulder was quite broken asunder, as if cut or chopped with a blunt tool. His hat was strangely torn, not just on the crown, but on its side and brim. The hole made on its side was large enough to admit one's fist, being gashed and torn as if cut with a dull tool.

The night following the three doctors above mentioned and myself, with some surgeons (besides a multitude of others) were present at the opening of the head; the vessels of the brain were pretty full of blood, but nothing amiss could be discerned. (The examination was made by candle light, and hastily, as the corpse was soon to be interred, and the crowds of people were an impediment.)—Some of the hair on the right temple was singed, and the lower part of the ear was blacker than the rest of the body. On opening the breast, it was found that the burning reached quite through the skin, which was in those scorched places hard and horny, and shrunk up, so as not to be so thick as the soft skin about it; but there was no appearance of any injury deeper than the skin, the muscles being not at all altered or discoloured. On removing the sternum, the lungs and heart exhibited a natural appearance. The whole body was, by night, very much swelled, more than in the morning, and smelt very strong and offensively; which might be owing partly to the warmth of the weather, (it was the month of May) and partly to the heat of the place occasioned by the multitude of people.*

Of Shining Fish. By Dr. BEALE. *N° 13, p. 226.*

May 5, 1665, fresh mackrel were boiled in water, with salt and sweet herbs; and they were left in the water for pickle.

May 6, more fresh mackrel were boiled, and, May 7, both water and mackrel were put into the former water, together with the former mackrel: but May 8, in the evening, the cook stirring the water, to take out some of the mackrel, found the water at the first motion become very luminous, and the fish shining through the water, as adding much to the light which the water yielded. The water, by the mixture of salt and herbs in the boiling, was of itself thick and rather blackish, than of any other clear colour: yet being stirred, it shined, and all the fish appeared more brightly luminous in their own shapes.

Wherever the drops of this water fell, after stirring, they emitted a light. On the cook's turning up the lower side of the fish, there was no light: and after the water was for some time settled, and fully at rest, it did not shine at all.

May 9, we repeated the same trial, and found the same effects. The water, till it was stirred, gave no light, but was thick and dark. But as soon as the cook's hand was thrust into the water, it began to glimmer; and being gently stirred by the hand moving round, it shone in such a manner, that those at

* In some future accounts of deaths occasioned by lightning, we shall offer a remark or two on the appearances which it induces, and on the manner in which it proves fatal.

some distance took it for the light of the moon through a window upon a vessel of milk; and by brisker circulation it seemed to flame.

The fish at that time shone both from the inside and outside, but chiefly from the throat, and such places as seemed a little broken in the boiling. The observer took a piece that shone most, and fitted it both to his great microscope, and afterwards to the little one; but he could discern no light by any of the glasses; nor from any drops of the shining water, when put into the glasses. And May 10, in the brightest rays of the sun, he examined in the great microscope a small broken piece of the fish, which shined most the night before, but could find nothing on the surface of the fish very remarkable. It seemed whitish, and in a manner dried, with deep inequalities; and a steam rather darkish than luminous seemed to arise like a very fine dust from the fish; with here and there very small and almost imperceptible sparkles in the fish.

The great microscope being fitted in the day-light for this piece of fish, we examined it that night, and it yielded no light at all, either by the glass or otherwise. Finding it dry, he thought that the moisture of spittle, and touching of it, might cause it to shine; which it did, though but a very little, in a few small sparks, which soon became extinguished. This was observed with the naked eye.

He caused two fish to be kept for further trial, two or three days longer, in very hot weather, till they were fetid, expecting more brightness, but could find none either in the water by stirring it, or in the fish taken out of the water.

Remarks on a Letter in the Journal des Scavans of May 24, 1666. N° 13, p. 228.

Whereas the French author is of opinion, that it is unknown how much time a heavy body requires to sink in water, acccording to a certain depth; he may please to take notice, that that has been made out in England by frequent experiments; by which several depths, found by this method of sounding without a line, were examined by trying them over again in the same place with a line, after the common way. And as to that quære of his, whether a heavy body descends in the same proportion of swiftness in water, that it would do in air? the answer is, that it does not; but that, after it is sunk one or two fathoms into the water, it has there arrived to its greatest swiftness, and keeps after that an equal degree of velocity; the resistance of the water being then found equal to the endeavour of the heavy body downwards.

And, when the same author alleges that it must be known, when a light body reascends from the bottom of the water to the top, in what proportion of time and swiftness it rises, he seems not to have considered, that in this experiment, the times of the descent and ascent are both taken, and computed together; so that, for this purpose, there needs not that nicety which he mentions.

Also, whereas it is further excepted, that this way of sounding depths is no new invention; the answer is ready, that neither is it pretended to be so, in the often quoted tract; it being only intimated there, that the manner of performing it, as it is in that place represented and described, is new.

A New Statical Baroscope. By Mr. BOYLE. *N°* 14, *p.* 231.

I caused to be blown, at the flame of a lamp, some glass bubbles, as large, thin, and light, as I could then procure, and choosing among them one about the size of a large orange, and weighing one dram ten grains, I counterpoised it in a pair of scales, that would lose their equilibrium with about the 30th part of a grain, and were suspended in a frame. I placed both the balance and the frame by a good baroscope, from whence I might learn the present weight of the atmosphere. Though the scales were not able to show me all the variations of the air's weight that appeared in the mercurial baroscope, yet they did what I expected, by showing me variations so small as altered the height of quicksilver half a quarter of an inch, and perhaps much smaller than those: nor did I doubt, that if I had nicer scales I should have discerned much smaller alterations of the weight of the air, since I had the pleasure to see the bubble sometimes in equilibrium with the counterpoise; and sometimes, when the atmosphere was high, preponderate so manifestly, that the scales being gently touched, the cock would play altogether on that side at which the bubble was hung; and at other times, when the air was heavier, that which was at the first but the counterpoise only would preponderate, and, upon the motion of the balance, make the cock vibrate altogether on its side. And this would continue sometimes many days together, if the air so long retained the same weight; and then, upon any change of weight, the bubble would regain an equilibrium, or a preponderance, so that I had oftentimes the satisfaction, by looking first upon the statical baroscope, to foretel whether in the mercurial baroscope the mercury were high or low.

So that, the matter of fact having been made out by variety of repeated observations, and by sometimes comparing several of those new baroscopes together, I shall add some of these notes about this instrument, which readily occur to my memory, reserving the rest till another opportunity.

And first, if the ground on which I went in framing this baroscope be demanded, the answer in short may be; 1. That, though the glass bubble and the glass-counterpoise, at the time of their first being weighed, be in the air, exactly of the same weight; yet they are very different in bulk; the bubble being perhaps a hundred or two hundred times larger than the metalline counterpoise. 2. That, according to hydrostatical laws, if two bodies of equal gravity, but unequal bulk, come to be weighed in another medium, they will be no longer equiponderant. If the new medium be heavier, the greater body, being specifically lighter, will lose more of its weight than the less and more compact one; but if the new medium be lighter than the first, then the larger body will outweigh the smaller: and this disparity, arising from the change of mediums, will be so much the greater, by how much the greater inequality of bulk there is between the bodies. 3. Comparing these two together, I considered that it would be all one, as to the effect, whether the bodies were weighed in mediums of different gravity, or in the same medium, in case its specific gravity were subject to considerable alterations.

Though a single bubble of competent size be much preferable, by reason that a far less quantity and weight of glass is requisite to comprise an equal capacity, when the glass is blown into a single bubble, than when it is divided into two; yet if the balance be strong enough to bear so much glass, without being hurt, by employing two or a greater number of large bubbles, the effect may be more conspicuous than if only a single bubble were employed.

This instrument may be much improved by divers accommodations. As

1. There may be fitted to the cheeks of the balance an arch of a circle, divided into 15 or 20 degrees, more or less according to the goodness of the balance, that the cock pointing to these divisions may readily, and without calculation, show the quantity of the angle, by which, when the scales incline either way, the cock declines from the perpendicular, and the beam from its horizontal parallelism.

2. Instead of the ordinary counterpoise of brass, one of gold may be employed, or at least of lead, whereof the latter being of equal weight with brass, is much less in bulk, and the former amounts not to half its magnitude.

3. Those parts of the balance made of copper or brass will be less subject to rust than steel; which yet, if well hardened and polished, may last a good while.

4. Instead of the scales, the bubble may be hung at one end of the beam, and only a counterpoise to it at the other, that the beam may not be burdened with unnecessary weight.

5. The whole instrument, if placed in a small frame, like a square lantern

with glass windows, and a hole at the top for the air, it will be more free from dust, and irregular agitations.

6. This instrument being accommodated with a light wheel and an index, such as have been applied by Dr. Chr. Wren to weather glasses, and by Mr. Hook to baroscopes, may be made to show minuter variations than otherwise.

7. And the length of the beam, and niceness of the balance, may make the instrument still more exact.

Though in some respects this statical baroscope be inferior to the mercurial; yet in others it has its own advantages and conveniences. As, first, it affords an ocular proof that the falling and rising of the mercury depends upon the varying weight of the atmosphere; since in this baroscope it cannot be pretended that a *fuga vacui*, or a *funiculus*, is the cause of the changes we observe. 2. It shows, not only that the air has weight, but heavier than some learned men will allow; since even the variation of weight in so small a quantity of air as is but equal in bulk to an orange, is manifestly discoverable by such balances. 3. This statical baroscope will often be more easily prepared than the other. 4. The essential parts of the scale-baroscope may very easily in a little room be carried any where, without the hazard of being spoiled or injured. 5. Mercurial barometers contain air, more or less, but in the other, that consideration does not take place. 6. It being possible to discover hydrostatically both the size of the bubble and the contents of the cavity, as also the weight and dimensions of the glassy substance, we may easily discover by this instrument this absolute and relative weight of the air. For, when the mercury is either very high or very low, or at a medium height, bringing the scale-barometer to an exact equilibrium, and observing when the mercury is risen or fallen just an inch, or a half or fourth of an inch, &c. and putting in the like minute divisions of a grain to the lighter scale, till you have again brought the balance to an exact equilibrium, you may determine what weight in the statical baroscope answers to the several altitudes of quicksilver. And if the balance be accommodated with a divided arch, or a wheel and index, these observations will assist you for the future to determine readily what the bubble has gained or lost in weight by the change of the atmosphere's weight. 7. By this statical instrument we may be enabled to compare the mercurial baroscopes of several places, and to make some estimate of the gravities of the air. As if, for instance, it is found by observation that the bubble weighed just a dram when the mercurial cylinder was at the height of $29\frac{1}{2}$ inches; and that the addition of the 16th part of a grain is requisite to keep the bubble in an equilibrium, when the mercury is risen an 8th, or any determinate part of an inch above the former height: and when in another place, where there is a mercurial barometer, as well freed from air as mine, if it appear to weigh precisely a dram, and the mercury in the baroscope there stand at just 29½ inches, we may conclude the gravity of the atmosphere to be sensibly equal in both those two places, though very distant. And though there be no baroscope there, yet if there be an additional weight, as for instance, the 16th part of a grain requisite to be added to the bubble, to bring the scales to an equilibrium, it will appear that the air at this second place is, at that time, so much heavier than the air of the former place was, when the mercury stood at 29½ inches.

But in making such comparisons, we must consider the situations of the several places; for if one of them be in a vale or bottom, and the other on the top or some elevated part of a hill, it is not to be expected that the atmosphere in this latter place should gravitate as much as the atmosphere in the former, on which a longer pillar of air insists. And this suggests a method of ascertaining the absolute and comparative height of mountains, &c. by noticing, by the baroscope, the difference in the weight of the air at the bottom and at the top.*

The Phases and Revolutions of the Planet Mars about his Axis. By Mr. Hook. N° 14, p. 239.

On the third of March, 1665-6, though the disposition of the air was not good, yet I could see now and then the body of Mars appearing of the form A, fig. 1, pl. 2, which I presently described by a scheme; and about 10 minutes after, it had through the glass the appearance as in the scheme B. This I was sufficiently satisfied, by very often observing it through the tube, and changing my eye into various positions, that so there might be no kind of fallacy in it, could be nothing else, but some more dusky and spotted parts of the face of this planet.

March 10, finding the air very thick, I made use of a very shallow eye-glass, as nothing appeared distinct with the greater charge; and saw the appearance of the planet as in C, which I imagined might be the representation of the former spots by a lesser charge. About 3 o'clock the same morning, the air being very bad (though to appearance exceeding clear, and causing all the stars to twinkle, and the minute stars to appear very thick) the body seemed like D; which I still supposed to be the representation of the same spots through a more confused and glaring air.

* Thus we find that the method of measuring heights by the barometer, is nearly as old as the instrument itself.

But observing March 21, I was surprised to find the air, though not so clear as to the appearance of small stars, so exceeding transparent, and the face of Mars so very well defined, and round and distinct, that I could manifestly see it of the shape in E, about half an hour after nine at night. The triangular spot on the right side (as it was inverted by the telescope, according to the appearances, through which all the preceding figures are drawn) appeared very black and distinct, the other towards the left more dim; but both of them sufficiently plain and defined. About a quarter before 12 o'clock the same night, I observed it again with the same glass, and found the appearance exactly as in F; which I imagined to show a motion of the former triangular spot.

Also March 22, about half an hour after 8 at night, finding the same spots in the same posture, as at G, I concluded that the preceding observation was only the appearance of the same spots at another height and thickness of the air; and thought myself confirmed in this opinion, by finding them in much the same posture March 23, about half an hour after 9, as at H, though the air was not so good as before.

And though I desired to make observations about 3 o'clock those mornings, yet something or other intervened, that hindered me, till March 28, about 3 o'clock, the air being light in weight, though moist and a little hazy, when I plainly saw it to have the form represented in I; which is not reconcileable with the other appearances, unless we allow a turbinated motion of Mars upon its centre: which, if such there be, from the observations made March 21, 22, and 23, we may guess it to be once or twice in about 24 hours, unless it may have some kind of librating motion; which seems not so likely.

Observations made in Italy, confirming the former, and fixing the Period of the Revolution of Mars. By J. D. Cassini. N° 14, *p.* 242.

That with a telescope of 24 palmes, or of about 16 feet, wrought after S. Campani's way, he began to observe February 6, 1666, N. S. in the morning, and saw two dark spots in the first face of Mars, as represented at K, pl. 2, fig. 2.

That with the same glass he observed Feb. $\frac{14}{24}$, in the evening, in the other face of this planet, two other spots, like those of the first, but larger; as L.

That afterwards continuing the observations, he found the spots of these two faces to turn by little and little from east to west, and to return at last to the same situation wherein he had seen them first.

That S. Campani having also observed at Rome, with glasses of 50 palmes, or about 35 feet, likewise of his own contrivance, had seen in the same planet the same phænomena; as M the first face March 3 in the evening, and N the 2d face March 18 in the evening

That sometimes he saw during the same night, the two faces of Mars, one in the evening, the other in the morning.

That the motion of these spots in the inferior part of the apparent hemisphere of Mars is made from east to west, as that of all the other celestial bodies, and is performed by parallels, that decline much from the equator, and little from the ecliptic.

That the spots return the next day to the same situation, 40 minutes later than the day before; so that in every 36 or 37 days, about the same hour they come again to the same place.

He states that some other astronomers have also made at Rome several observations of these spots of Mars, from March $\frac{14}{24}$ to March $\frac{20}{30}$, with glasses wrought by Eustachio Divini, of 25 and 45 palmes: which spots he makes but little different from his own of the first face; as will by and by appear, by the direction to the schemes.

But he adds that those other Roman astronomers who have observed with Divini's glasses, will have the conversion of Mars to be performed, not in 24h. 40m, as he maintains it is, but in about 13h.

And to evince that they are mistaken in these observations of theirs, he alleges, that they assure that the spots which they have seen in this planet by an Eustachian telescope, the $\frac{20}{30}$ of March, were small, very distant from one another, remote from the middle of the disk, and the oriental spot was less than the occidental, as is represented by the fig. O, like that of the first face of Mars; whereas, on the contrary, Cassini pretends to evidence by his observations, made at the same time at Bononia, that the same day and hour those spots were very large, near one another, in the midst of the disk, the oriental larger than the occidental, as appears by fig. P, which is that of the second face of Mars.

Besides, he declares that those astronomers were too hasty in determining, after five or six observations only, in how much time Mars finished his revolution; and denies it to be performed in 13 hours: adding, that though he himself had observed for a much longer time than they, yet he durst not for a great while define whether Mars made but one turn in 24 hours 40 minutes, or two; and that all that he could for a long time affirm, was only this, that after 24 h. 40 m. this planet appeared in the same manner he did before.

But since those first observations, he affirms to have found cause to determine that the period of this conversion is made in the said space of 24 h. 40m; and not oftener than once within that time; alleging for proof;

1. That whereas Feb. 6, N. S. he saw the spots of the first face of Mars moving from eleven o'clock in the night until break of day, they appeared not afterwards in the evening after the rising of that planet; whence he infers, that after 12 hours and 20 minutes, the same spots did not come about; since that the same which in the morning were seen in the middle, upon the rising of Mars, after 13 or 14 hours might have appeared near the occidental limb. But because he might be imposed upon by vapours, whilst Mars was yet so near the horizon, he gives this other determination, viz.

2. Whereas he saw the first face of Mars the 6th of February at 11 o'clock of the night following; he did not see the same after 18 days at the same hour, as he ought to have done, if the period is performed in the space of 12h. 20m.

3. Again, whereas he saw, Feb. 24 in the evening, the other face of Mars, he could not see the same the 13th and 15th day of March, to wit, after 17 and 19 days, as he should have done, if the revolution were made in the newly mentioned time.

4. Again, whereas the 27th of March in the evening he saw the second face of Mars, he could not see it the 14th and 16th of April.

From all which observations he judges it to be evident, that the period of this planet's revolution is not performed in the space of 12 hours 20 min. but in about 24 hours 40 min. more exactly to be determined by comparing distant observations: and that those who affirm the former must have been deceived by not well distinguishing the two faces, and by having seen the second, mistaking it for the first.

All which he concludes with this hint, that, when he defines the time of the revolution of Mars, he does not speak of its mean revolution, but only of that which he observed whilst Mars was opposite to the sun, which is the shortest of all.*

The Phases of the Planet Jupiter. By Mr. Hook. *N° 14, p. 245.*

Anno 1666, June 26th, between three and four o'clock in the morning, I observed the body of Jupiter through a 60 foot-glass, and found the apparent diameter of it through the tube to be somewhat more than two degrees, that is, about four times as large as the diameter of the moon appears to the naked eye.

* The more nice observations of later observers have proved that Cassini was right in his suspicions above expressed; the period of the rotation of Mars having been settled by modern astronomers at 24h. 39m. 22 s.

I saw the limb pretty round, and very well defined without radiation. The parts of the phasis of it had various degrees of light. About *a* and *f*, its north and south poles, (in the fig. Q. pl. 2) it was somewhat darker, and gradually grew brighter towards *b* and *e*, two belts or zones; the one of which (*b*) was a small dark belt crossing the body southward; adjoining to which was a small line of a somewhat lighter part; and below that again, southwards, was the large black belt *c*. Between that and *e*, the other smaller black belt, was a pretty large and bright zone; but the middle *d*, was somewhat darker than the edges. I perceived, about 3h. 15m. near the middle of this, a very dark round spot, like that represented at *g*, which was not to be perceived above half an hour before. And I observed it in about 10 minutes time to be got almost to *d*, keeping equal distance from the satellite *h*, which moved also westwardly, and was joined to the disk at *i*, at 3h. 25m. After which, the air growing very hazy, and as appeared by the baroscope very light also in weight, I could not observe it; so that it was sufficiently evident that this black spot was nothing else save the shadow of the satellite *h*, eclipsing a part of the face of Jupiter. About two hours before, I had observed a large darker spot in the larger belt about *k*, which in about an hour or little more, moving westwards, disappeared. About a week before I discovered also, together with a spot in the belt *c*, another spot in the belt *e*, which kept the same way and velocity with that of the belt *c*. The other three satellites in the time of this eclipse, made by the satellite, were westwards of the body of Jupiter; appearing as bright through the tube as the body of Jupiter did to the naked eye; and I was able to see them longer through the tube, after the day-light came on, than I was able to see the body of Jupiter with my naked eye.

Observation of Saturn. By Mr. HOOK. *N° 14, p. 246.*

June 29, 1666, between 11 and 12 at night, I observed the body of Saturn through a 60 foot telescope, and found it exactly of the shape represented in the figure R, pl. 2. The ring appeared of a somewhat brighter light than the body; and the black lines *a a*, crossing the ring, and *b b* crossing the body (whether shadows or not, I dispute not) were plainly visible; whence I could manifestly see that the southern part of the ring was on this side of the body, and the nothern part behind, or covered by the body.

A sad Effect of Thunder and Lightning. By THOMAS NEALE, *Esq. N° 14, p. 247.*

On the 24th of January 1665-6, Mr. Brooks of Hampshire, going from Winchester towards his house near Andover, in very bad weather, was killed by lightning, together with his horse. At about a mile from Winchester he was found with his face beaten into the ground, one leg in the stirrup, the other in the horse's mane; his cloaths all burnt off his back, and his hair and all his body singed; and his cloaths were so scattered and consumed, that not enough to fill the crown of a hat could be found. His nose was beaten into his face, and his chin into his breast, in which was a wound cut almost as low as his navel. His gloves were whole, but his hands in them singed to the bone. The hip-bone and shoulder of his horse burned and bruised, and his saddle torn into small pieces.

Of some Books lately published. N° 14, *p*. 248.

I. Relations of Divers Curious Voyages, by Mons. Thevenot, vol. 3. in French. This book contains chiefly, the Embassy of the Dutch into China, translated out of the Dutch manuscript: a Geographical Description of China, translated out of a Chinese author by Martinius; and the account which the Directors of the Dutch East-India Company made to the States General, touching the state of affairs in the East-Indies, when their late fleet parted from thence.

II. A Discourse on the Causes of the Inundation of the Nile. The author of this book is Monsieur de la Chambre, who being persuaded, from several circumstances which accompany the overflowing of this river, that it cannot proceed from rain, ventures to assign as a cause, as well as for other effects of its swelling, the nitre with which that water abounds.*

III. De Principiis et Ratiocinatione Geometrarum; Contra Fastum Professorum Geometriæ; authore Thoma Hobbes. It seems that this author is angry with all geometricians but himself; yea he plainly says in the dedication of his book, that he invades the whole nation of them; and unwilling, it seems, to be called to an account for doing so; he will acknowledge no judge of this age; but is full of hopes that posterity will pronounce for him. Meanwhile he ventures to advance this *Dilemma*; *Eorum qui de iisdem rebus mecum aliquid ediderunt, aut solus insanio Ego, aut solus non insanio*; *tertium enim non est, nisi (quod dicet forte aliquis) insaniamus omnes.* Doubtless, one of these will be granted him.

* It is now well known that the overflowing of the Nile in summer, is owing to the torrents of rain and the melting of the snow on the mountains in Abyssinia and Upper Egypt.

As to the book itself, he professes that he does not write it against Geometry, but geometers; and that his design in it is to show, that there is no less uncertainty and falsity in the writings of mathematicians, than there is in those of naturalists, moralists, &c. though he judges that physics, ethics, and politics, if they were well demonstrated, would be as certain as the mathematics.

Attacking the mathematical principles as they are found in books, and withal some demonstrations, he takes to task Euclid himself, instead of all, as the master of all geometricians, and with him his best interpreter Clavius, examining in the first place the principles of Euclid: Secondly, declaring false what is superstructed upon them, whether by Euclid or Clavius, or any geometer whatsoever that has made use of those or other (as he is pleased to entitle them) false principles. Thirdly, pretending that he means so to combat all, both principles and demonstrations, undertaken by him, as that he will substitute better in their room, lest he should seem to undermine the science itself.

IV. King Solomon's Portraiture of Old Age; by John Smith, M. D. This treatise being a philosophical discourse, though upon a sacred theme, may certainly claim a place among Philosophical Transactions. Among other ingenious observations on the subject, the author gives the following very curious one; that the expression of Solomon (Eccles. chap. 12.) probably denotes the same doctrine as the discovery of the sagacious Harvey, of the Blood's Circulation. The *pitcher* being interpreted for the *veins*, the *fountain* for the *right ventricle of the heart*; *the cistern* for the *left*; the *wheel* the *circulation.*

A new Frigorific Experiment, showing how a considerable Degree of Cold may be suddenly produced, without the help of Snow, Ice, Hail, Wind, or Nitre, and that at any Time of the Year. By Mr. BOYLE. *N° 15, p. 255.*

Among the several ways by which I have made infrigidating mixtures with sal ammoniac (muriate of ammonia) the most simple and easy is this: Take 1 lb. of powdered sal ammoniac and about three pints or pounds of water; put the salt into the liquor, either altogether, if your design be to produce an intense but short coldness; or, at two, three, or four several times, if you desire that the produced coldness should rather last somewhat longer than be so great. Stir the powder in the liquor with a stick or whalebone (or some other thing that will not be acted upon by the brine) to hasten the dissolution of the salt, upon the quickness of which depends very much the intensity of the cold that will ensue.

I. That a considerable degree of cold is really produced by this is evident, 1st. To the touch; 2dly, By the dew which collects on the sides of the vessel containing the solution; 3dly, By plunging into it (which is the best and surest test) a good sealed weather-glass furnished with tinctured spirit of wine.* For the ball [bulb] of this being put into our frigorific mixture, the crimson liquor will nimbly enough descend much lower than when it was kept either in the open air or in common water of the same temperature with that wherein the sal ammoniac was put to dissolve.

II. The duration of the cold produced by this experiment depends upon several circumstances; as, 1st, Upon the season of the year and present temperature of the air; 2dly, Upon the quantity of the salt and water; 3dly, Upon the goodness and fitness of the particular parcel of salt that is employed; 4thly, Upon the way of putting the salt into the water; for if you cast it in all at once, the water will sooner acquire an intense degree of coldness, but it will also the sooner return to its former temperature: whereas, if you desire but an inferior degree of that quality, but that may last longer (which will usually be the most convenient for the cooling of drinks) then you may put in the salt little by little. For keeping a long weather-glass (thermometer) for a good while in our impregnated mixture, I often purposely tried that when the tinctured liquor subsided but slowly or was at a stand, by putting in, from time to time, two or three spoonfuls of fresh salt, and stirring the water to quicken the solution, the spirit of wine would begin again to descend. The refrigerating process may be lengthened by having part of the sal ammoniac but grossly pounded, so as to be longer in dissolving. In spring I have found by a good weather-glass (thermometer) a sensible artificial cold, made by a pound of sal ammoniac at the utmost, to last about two or three hours.

III. To cool drinks with this mixture, you may put them in thin glasses, the thinner the better; which (their orifices being stopped and kept above the mixture) may be moved to and fro in it, the liquor being then immediately poured out and drank; or, if the glass be conveniently shaped, it may be drank out of that, without pouring it into another, which lessens the coolness.

IV. Whether sal ammoniac mixed with sand or earth, and not dissolved, but only moistened with a little water sprinkled upon it, will answer for cooling

* In the subsequent part of the paper this weather-glass is more fitly termed a thermoscope or thermometer.

bottles of wine or other liquors, Mr. Boyle says he did not satisfy himself by a sufficient number of trials.*

V. For the cooling of air and liquors, and to adjust weather-glasses or thermometers, (to be able to do which at all times of the year was one of the chief aims that made me think of this experiment†) or to give a small quantity of beer, &c. a moderate degree of coolness, it will not be requisite to employ near so much as a whole pound of sal ammoniac at a time. A few ounces dissolved in about four times their weight of water will suffice.

VI. In this section Mr. Boyle relates, that about the end of March he was able, with a pound of sal ammoniac and a requisite proportion of water, to produce ice in a very short space of time. His sealed thermoscope (containing tinctured spirit of wine) was 16 inches long, the ball [bulb] about the size of a walnut, and the diameter of the tube about an 8th or 10th of an inch. Being put into the water before the salt was added, the coloured spirit was first at $8\frac{5}{8}$ inches, and after some time a little beneath $7\frac{5}{8}$; but in about a quarter of an hour after the sal ammoniac was added, it descended to $2\frac{1}{16}$ inches, and in seven or eight minutes before that time, the vapour and drops of water on the outside of the glass began to freeze. When the frigorific power was at its height, water thinly placed on the outside of the glass, whilst the mixture was quickly stirred up and down, would freeze in a quarter of a minute. After three hours from the beginning of the operation, the crimson liquor of the thermometer was at $4\frac{3}{4}$ inches, the height to which strong and durable frosts had reduced it in the winter.

VII. The sal ammoniac employed in these experiments may be recovered (to save expense) by evaporating the solution, and crystallizing it. The salt thus obtained will serve again for fresh frigorific mixtures.‡

* As the absorption of heat or production of cold depends upon the conversion of the salt from its solid form into a fluid state; it is evident that, when it is only partially dissolved, as in the case of its being mixing with sand and merely sprinkled with water, the degree of refrigeration will be very inconsiderable.

† Since Mr. Boyle's time thermometers have been more accurately adjusted by employing snow or ice just beginning to melt for the freezing point, and water boiling under a pressure of the atmosphere corresponding to 29,8 of the barometer, for the boiling point.

‡ A solution of sal ammoniac (muriate of ammonia) and nitre (nitrate of potash) produces a greater degree of cold than a solution of sal ammoniac alone. But there are other salts which produce this effect in a much stronger degree, such as the nitrate of ammonia dissolved in water, the phosphate of soda dissolved in diluted nitric acid, the sulphate of soda dissolved in the same acid, or in diluted sulphuric acid, &c. See Mr. Walker's experiments on artificial cold, Phil. Trans. vols. 77, 78. When we come to give an account of these experiments, we shall have an opportunity of noticing the intense degrees of cold which may be produced by means of snow and the diluted nitric and sulphuric acids, or of snow and muriate of lime.

An Account of two Books lately published in London. N° 15, p. 261.

I. Euclidis Elementa Geometrica, novo ordine ac methodo demonstrata. In this edition the anonymous author pretends to have rendered these elements more expeditious, by bringing into one place what belongs to one and the same subject; comprising, 1. What Euclid had said of lines, straight, intersecting one another, and parallel. 2. What he has demonstrated of a single triangle, and of triangles compared one with another. 3. What of the circle, and its properties. 4. What of proportions in triangles and other figures. 5. What of quadrats and rectangles, made of lines diversely cut. 6. What of plane superficies. 7. What of solids. After which follow the problems. The definitions are put to each chapter as need requires. The axioms, because they are few, and almost every where necessary, are not thus distributed in chapters. The postulata are not subjoined to the axioms, but reserved for the problems, the author considering that they being practical principles, had only place in problems.

II. The English Vine-yard vindicated. The author (Mr. John Rose, his Majesty's gardener at his royal garden in St. James's) in this small tract directs Englishmen in the choice of the fruit, and the planting of vineyards, heretofore very frequently cultivated, though of late almost neglected by them.

Hypothesis on the Flux and Reflux of the Sea. Addressed to Mr. Boyle *by Dr.* John Wallis. N° 16, p. 263.

You were earnest with me, when you last went hence, that I would put in writing what at divers times, for these three or four years, I have been discoursing with yourself and others concerning the common centre of gravity of the earth and moon, for the solving the phænomena as well of the sea's ebbing and flowing, as of some perplexities in astronomical observations of the places of the celestial bodies.

How much the world and the great bodies therein are managed according to the laws of motion and static principles, and with how much more clearness and satisfaction many of the more abstruse phænomena have been solved on such principles, within this last century than formerly; I need not discourse to you who are well versed in it. For since Galilæo, and after him Torricellio and others, have applied mechanic principles to the solving of philosophical difficulties, natural philosophy is well known to have been rendered more intelligible, and to have made a much greater progress in less than a hundred years, than before for many ages.

The sea's ebbing and flowing has so great a connexion with the moon's motion, that in a manner all philosophers have attributed much of its cause to the moon, which either by some occult quality, or particular influence which it has on moist bodies, or by some magnetic virtue, drawing the water towards it,* which should therefore make the water highest where the moon is vertical, or by its gravity and pressure downwards upon the terraqueous globe, which should make it lowest, where the moon is vertical, or by whatever other means, has so great an influence on, or at least connexion with, the sea's flux and reflux, that it would seem very unreasonable to separate the consideration of the moon's motion from that of the sea: the periods of tides, to say nothing of the greatness of them near the new and full moon, so constantly waiting on the moon's motion, that it may be well presumed, that either the one is governed by the other, or at least both by some common cause.

But the first that I know who took in the consideration of the earth's motion, diurnal and annual, was Galilæo, who, in his System of the World, has a particular discourse on this subject; which, from the first time I read it, seemed to me so very rational, that I could never be of other opinion, than that the true account of this great phenomenon was to be referred to the earth's motion as the principal cause of it; yet that of the moon not to be excluded as to the determining the periods of tides, and other circumstances concerning them. And though it be manifest enough, that Galilæo, as to some particulars, was mistaken in the account which he there gives of it; yet that may be very well allowed, without any blemish to so deserving a person, or prejudice to the main hypothesis: for that discourse is to be looked upon only as an Essay of the general hypothesis; which as to particulars was to be afterwards adjusted, from a good General History of Tides; which it is manifest enough that he had not, and which is in a great measure yet wanting.

And what I say of Galilæo, I must in like manner desire to be understood of what I am now ready to say to you. For I do not profess to be so well skilled in the history of tides, as to undertake presently to accommodate my general hypothesis to particular cases; or indeed to undertake for the certainty of it, but only as an essay propose it to further consideration, to stand or fall, as it shall be found to answer matter of fact.

I consider therefore, that in the tides, or the flux and reflux of the sea, besides extraordinary extravagances, or irregularities, whence great inundations or strangely high tides follow, (which yet perhaps may prove not to be so merely

* It is curious to observe here how near the conjectures of Wallis approached to the true cause and theory of the tides, afterwards more fully developed and demonstrated by Newton.

accidental as they have been thought to be, but might from the regular laws of motion, if well considered, be both well accounted for, and even foretold;) these three notorious observations are made of the reciprocation of tides.

1. The diurnal reciprocation; whereby twice in somewhat more than 24 hours, we have a flood and an ebb; or a high-water and low-water. 2. The menstrual; whereby in one synodical period of the moon, suppose from full-moon to full-moon, the time of those diurnal vicissitudes moves round through the whole compass of the Νυχθήμερον, or natural day of 24 hours; as for instance, if at the full moon the full sea be at such or such a place just at noon, it shall be the next day at the same place somewhat before one of the clock; the day following, between one and two; and so onward, till at the new moon it shall be at midnight; the other tide, which in the full moon was at midnight, now at the new moon coming to be at noon; and so forward, till at the next full moon the full sea shall at the same place come to be at noon again: Again, that of the spring tides and neap tides; about the full moon and new moon the tides are at the highest, at the quadratures the tides are at the lowest; and at the times intermediate, proportionably. 3. The annual, whereby it is observed, that at some part of the year, the spring tides are yet much higher than the spring tides at others, which times are usually taken to be at the spring and autumn, or the two equinoxes; but I have reason to believe, as well from my own observations for many years, as of others who have alike observed it, that we should rather assign the beginnings of February and November, than the two equinoxes.

Now in order to give account of these three periods, according to the laws of motion and mechanic principles, we shall first take for granted, what is now pretty commonly entertained by those who treat of such matters, that a body in motion is apt to continue its motion, and that in the same degree of celerity, unless hindered by some contrary impediment, like as a body at rest is apt to continue so, unless by something acting on it put into motion; and accordingly, if on a board or table some loose incumbent weight be for some time moved, and have thereby contracted an impetus to motion at such a rate; if that board or table chance by some external obstacle or otherwise to be stopped or considerably retarded in its motion, the incumbent loose body will shoot forward upon it; and contrarywise, in case that board or table chance to be accelerated or put forward with a considerably greater speed than before, the loose incumbent body, not having yet obtained an equal impetus with it, will be left behind, or seem to fly backward upon it. Or, which is Galilæo's instance, if a broad vessel of water, for some time evenly carried forward with the water in it, chance to meet with a stop, or to slack its motion, the water will dash forward and rise higher at the fore part of the vessel: and, contrarywise, if the vessel be suddenly put forward faster than before, the water will dash backwards, and rise at the hinder part of the vessel. So that an acceleration or retardation of the vessel which carries it, will cause a rising of the water in one part, and a falling in another; which yet, by its own weight, will again be reduced to a level as it was before. And consequently, supposing the sea to be but as a loose body, carried about with the earth, but not so united to it, as necessarily to receive the same degree of impetus with it as its fixed parts do, the acceleration or retardation in the motion of this or that part of the earth will cause such a dashing of the water, or rising at one part with a falling at another, as what we call the flux and reflux of the sea.

Now, this premised, we are next, with him, to suppose the earth carried about with a double motion; the one annual, as (Fig. 1. pl. 3.) in B E C the great orb in which the centre of the earth B is supposed to move about the sun A. The other diurnal, whereby the whole moves upon its own axis, and each point in its surface describes a circle, as D E F G.

It is then manifest, that if we suppose that the earth moved but by any one of these motions, and that regularly, the water having once attained an equal impetus thereunto, would still hold equal pace with it; but the true motion of each part of the earth's surface being compounded of those two motions, the annual and diurnal; while a point in the earth's surface moves about its centre B, from G to D and E, and at the same time its centre B be carried forwards to C, the true motion of that point forwards is made up of both those motions; to wit, of B to C, and of G to E; but while G moves by D to E, E moves backward by F to G, contrary to the motion of B to C; so that the true motion of E is but the difference of B C and E G; for beside the motion of B about the centre, G is also put forward as much as from G to E, and E put backward as much as from E to G; so that the diurnal motion in that part of the earth which is next the sun, as E F G, abates the progress of the annual, and most of all at F; and in the other part which is from the sun, as G D E, it increases it, and most of all at D, that is, in the day time there is abated, and in the night time added to the annual motion, about as much as is G E, the earth's diameter. Which would afford us a cause of two tides in twenty-four hours; the one upon the greatest acceleration of motion, the other upon its greatest retardation.

And thus far Galilæo's discourse holds well enough; but then in this it comes short, that as it gives an account of two tides, so those two tides are always to be at F and D, that is at noon and midnight; whereas experience tells us that the time of tides moves in a month's space through all the 24 hours. Of which he gives us no account. For though he takes notice of a menstrual period, yet he does it only as to the quantity of the tides, greater or less; not as to the time of the tides, sooner or later.

To supply this,* Jo. Baptista Balianus makes the earth to be but a secondary planet; and to move not directly about the sun, but about the moon, the moon meanwhile moving about the sun; in like manner as we suppose the earth to move about the sun, and the moon about it.

But this, though it might furnish us with the foundation of a menstrual period of accelerations and retardations in the compound motion of several parts of the earth's surface; yet I am not at all inclined to admit this as a true hypothesis, for divers reasons, which if not demonstrative are yet so consonant to the general system of the world, as that we have no good ground to disbelieve them. For, 1st, The earth being undeniably the greater body of the two, it cannot be thought probable that this should be carried about by the moon, less than itself: the contrary being seen not only in the sun, which is larger than any of the planets which it carries about; but in Jupiter larger than any of his satellites; and Saturn than his. 2d, As the sun, by its motion about its own axis is with good reason judged to be the physical cause of the primary planets moving about it, so there is the like reason to believe that Jupiter and Saturn moving about their axes is the physical cause of their satellites moving about them; which motion of Jupiter has been of late discovered, by the help of a fixed spot discerned in him; and we have reason to believe the like of Saturn. Whether Venus and Mercury, about whom no satellites have been yet observed, be likewise so moved, we have not yet the like ground to determine: but we have of Mars, from the observations of Mr. Hook, made in February and March last, consonant to the like observations of Jupiter made by him in May 1664. Now that the earth has such a motion about its own axis, whereby it might be fitted to carry about the moon, is evident by its diurnal motion. And it seems as evident that the moon has not, because of the same side of the moon always turned towards us; which could not be if the moon carried the earth about: unless we should say, that it carries about the earth in just the same period in which it turns upon its own axis: which is contrary to that of the sun carrying about the planets; the shortest of whose periods is yet longer than that of the sun's moving about its own axis. And the like of Jupiter, shorter than the period of any of his satellites. Of Saturn we have not yet any period assigned, but it is likely to be shorter than that of his satellites. And therefore we have reason to believe, not that by the moon's

* Vid. Riccioli Almagest. novum, Tom. 1. lib. 4. cap. 10. n. 111, p. 216, 2.

motion about its axis the earth should be carried by a contemporary period, whereby the same face of the moon should be ever towards us; but that by the earth's revolution about its axis in 24 hours, the moon should be carried about it in about 29 days, without any motion on its own axis: and accordingly, that the secondary planets about Jupiter and Saturn are not, like their principals, turned about their own axes. And therefore I am not at all inclined to believe that the menstrual period of the tides with us is to be solved by such an hypothesis.

Instead of this, that surmise of mine, for I dare not yet with confidence give it any better name, is to this purpose.

The earth and moon being known to be bodies of so great connexion (whether by any magnetic, or what other tie I will not determine) as that the motion of the one follows that of the other, may well enough be looked upon as one body, or rather one aggregate of bodies which have one common centre of gravity; which centre, according to the known laws of statics, is in a straight line connecting their respective centres, so divided as that its parts be in reciprocal proportion to the gravities of the two bodies. As for example, suppose the magnitude, and therefore probably the gravity of the moon to be about the one and fortieth part of that of the earth; and the distance of the moon's centre from the centre of the earth to be about 56 semidiameters of the earth, the distance of the common centre of gravity of the two bodies will be from that of the earth, about the 42d part of 56 semidiameters; that is, about $\frac{56}{42}$ or $\frac{4}{3}$ of a semidiameter, that is about $\frac{1}{3}$ of a semidiameter of the earth above its surface in the air, directly between the earth and moon.

Now supposing the earth and moon jointly as one body, carried about by the sun in the great orb of the annual motion; this motion is to be estimated according to the laws of statics, as in other cases, by the motion of the common centre of gravity of both bodies. For we are accustomed in statics to estimate a body or aggregate of bodies to be moved upwards, downwards, or otherwise, so much as its common centre of gravity is so moved, howsoever the parts may change places amongst themselves.

And accordingly the line of the annual motion will be described, not by the centre of the earth, nor by the centre of the moon, but by the common centre of gravity of the bodies, the earth and moon, as one aggregate.

Now supposing A B C D E, (fig. 2 and 3, pl. 3) to be a part of the great orb of the annual motion described by the common centre of gravity, in so long time as from a full moon at A to the next new moon at E; the centre of the earth at T, and that of the moon at L, must each of them, supposing their common centre of gravity to keep the line A E, be supposed to describe a periphery about that common centre, as the moon describes her line of menstrual motion. Of which I have in the scheme only drawn that of the earth, as being sufficient to our present purpose; parallel to which, if need be, we may suppose one described by the moon, whose distance is also to be supposed much greater from T than in the figure is expressed, or was necessary to express. And in like manner E F G H I, from that new moon at E, to the next full moon at I.

From A to E, from full moon to new moon, T moves, in its own epicycle, upwards from the sun: and from E to I, from new moon to full moon, it moves downwards toward the sun. Again from C to G, from last quarter to the following first quarter, it moves forwards according to the annual motion; but from G forward to C, from the first quarter to the ensuing last quarter, it moves contrary to the annual motion.

It is manifest therefore, according to this hypothesis, that from last quarter to first quarter, from C to G, while T is above the line of the annual motion, its menstrual motion in its epicycle adds somewhat of acceleration to the annual motion, and most of all at E, the new moon; and from the first to the last quarter, from G forward to C, while T is below the line of the annual motion, it abates of the annual motion, and most of all at I, or A the full moon.

So that in pursuance of Galilæo's notion, the menstrual adding to or detracting from the annual motion, should either leave behind or cast forward the loose waters incumbent on the earth, and thereby cause a tide or accumulation of waters; and most of all at the full moon and new moon, where those accelerations or retardations are greatest.

Now this menstrual motion, if nothing else were superadded to the annual, would give us two tides in a month, and no more; the one upon the acceleration, the other on the retardation, at new moon and full moon; and two ebbs at the two quarters; and in the intervals rising and falling water.

But the diurnal motion superadded, doth the same to this menstrual, as Galilæo supposes it to do to that annual; that is, adds to, or subtracts from, the menstrual acceleration or retardation; and so gives us tide upon tide.

For in whatsoever part of its epicycle we suppose T to be; yet because, while by its menstrual motion the centre moves in the circle L T N, fig. 4; each point in its surface, by its diurnal motion, moves in the circle L M N: whatever effect, accelerative or tardative, the menstrual would give, that effect by the diurnal is increased in the parts L M N, or rather the semicircle l M n, and most of all at M; but diminished in the parts N O L, or rather n O l, and most of all at O. So that at M and O, that is when the moon is in the meridian below or above the horizon, we are to have the diurnal tide or high water, occasioned by the greatest acceleration or retardation, which the diurnal arch gives to that of the menstrual; which seems to be the true cause of the daily tides. And withal gives an account, not only why it should be every day, but likewise why at such a time of the day, and why this time should in a month run through the whole 24 hours, viz. because the moon's coming to the meridian above and below the horizon, or as the seamen call it, the moon's southing and northing, doth so; as likewise of the spring tides and neap tides. For when it so happens that the menstrual and diurnal accelerations or retardations are coincident, as at new moons and full moons, the effect must needs be the greater. And although this happen but to one of the two tides, that is the night tide at the new moon, when both motions do most of all accelerate, and the day tide at full moon, when both do most retard the annual motion, yet this tide being thus raised by two concurrent causes, though the next tide have not the same cause also, the impetus contracted will have influence upon the next tide; for a like reason as a pendulum, let fall from a higher arch, will make the vibration on the other side, beyond the perpendicular, to be also greater; or, of water in a broad vessel, if it be so jogged as to be cast forward to a good height above its level, will upon its recoiling, by its own gravity, mount so much the higher on the hinder part.

But here also we are to take notice, that though all parts of the earth by its diurnal motion do turn about its axis, and describe parallel, yet not equal circles, but greater near the equinoctial, and lesser near the poles; which may be a cause why the tides in some parts may be much greater than in others. But this belongs to the particular considerations, not to the general hypothesis.

Having thus endeavoured to give an account of the diurnal and menstrual periods of tides, it remains that I endeavour the like as to the annual. Of which there is at least thus much agreed, that at some times of the year, the tides are noted to be much higher than at other times. But here I have a double task; first, to rectify the observation, and then to give an account of it.

As to the first, it having been observed that those high tides have used to happen about the spring and autumn; it has been generally taken for granted that the two equinoxes are the proper times to which these annual high tides are to be referred, and such causes sought for as might best suit with such a supposition.

But it is now the best part of twenty years since I have had frequent occasions to converse with some inhabitants of Romney-marsh in Kent; where the sea being kept out by great earthen walls, that as at high water not to overflow the level; and the inhabitants' livelihood depending most on grazing, or feeding sheep, they are very vigilant and observant at what times they are most in danger of having their lands drowned. And I find them generally agreed, by their constant observations, and experience dearly bought, that their times of danger are about the beginning of February and of November: that is, at those spring tides which happen near those times; to which they give the names of Candlemass-stream and Allhallond-stream: And if they escape those spring tides, they apprehend themselves out of danger for the rest of the year. And as for March and September, the two equinoxes, they are as little solicitous of them as of any other part of the year.

This I confess I much wondered at, and suspected it to be a mistake of him that first told me. But I soon found that it was not only his but a general observation of others too, both there and elsewhere along the sea coast. And though they did not pretend to know any reason of it, nor so much as to enquire after it, yet none made doubt of it, but would rather laugh at any that should talk of March and September as being the dangerous times. And since that time, I have myself very frequently observed, both at London and elsewhere, as I have had occasion, that in those months of February and November, especially November, the tides have run much higher than at other times; though I confess I have not been so diligent to set down those observations as I should have done. Yet this I do particularly very well remember, that in November 1660, having occasion to go by coach from the Strand to Westminster, I found the water so high in the middle of King-street, that it came up not only to the boots but into the body of the coach; and the Palace-yard overflowed, as likewise the market place, and many other places; and the cellars generally filled up with water. And in November last, 1665, it may yet be very well remembered what very high tides there were, not only on the coasts of England, where much hurt was done by them, but much more in Holland, where, by reason of those inundations, many villages and towns were overflowed. And though I cannot so particularly name other years, yet I have very often observed tides strangely high about those times of the year.

This observation for several years caused me much to wonder, not only because it is so contrary to the received opinion of the two equinoxes, but because I could not think of any thing signal at those times of the year; as being neither the two equinoxes, nor the two solstices, nor the sun's apogee and perigee, (or earth's aphelium and perihelium;) nor indeed at contrary times of the year, which at least would seem to be expected. From Allhallondtide to Candlemas being but three months, and from thence to Allhallondtide again nine months.

At length it came into my mind, about four years since, that though there do not about these times happen any single signal accident, which might cast it on these times, yet there is a compound of two that may do it: which is, the inequality of the natural day (I mean that of 24 hours from noon to noon) arising at least from a double cause; either of which singly would cast it upon other times, but both jointly on those.

It is commonly thought, how unequal soever the length be of the artificial days, as contradistinguished to nights, yet that the natural days, reckoning from noon to noon, are all equal: but astronomers know well that even these days are unequal.

For this natural day is measured not only by one entire conversion of the equinoctial, or 24 equinoctial hours, which is indeed taken to be performed in equal times, but increases by so much as answers to that part of the sun's (or earth's,) annual motion as is performed in that time. For when that part of the equinoctial which with the sun was at the meridian yesterday at noon, is come thither again to-day, it is not yet noon, because the sun is not now at the place where yesterday he was, but is gone forward about one degree more or less, but we must stay till that place where the sun now is comes to the meridian before it be now noon.

Now this additament, above the 24 equinoctial hours, or entire conversion of the equinoctial, is upon a double account unequal: First, Because the sun, by reason of its apogee and perigee at all times of the year, dispatches in one day an equal arch of the ecliptic, but greater arches near the perigee, which is about the middle of December, and lesser near the apogee, which is about the middle of June; as will appear sufficiently by the tables of the sun's annual motion. Secondly, Though the sun should in the ecliptic move always at the same rate, yet equal arches of the ecliptic do not in all parts of the zodiac answer to equal arches of the equinoctial, by which we are to estimate time; because some parts of it, as about the two solstitial points, lie nearer to a parallel position to the equinoctial than others, as those about the two equinoctial points, where the ecliptic and equinoctial do intersect; whereupon an arch of the ecliptic, near the solstitial points, answers to a greater arch of the equinoctial, than an arch equal thereunto near the equinoctial points; as doth sufficiently appear by the tables of the sun's right ascension.

According to the first of these causes, we should have the longest natural days in December, and the shortest in June, which if it did operate alone, would give us at those times two annual high waters.

According to the second cause, if operating singly, we should have the longest days at the two solstices in June and December, and the two shortest at the equinoxes in March and September; which would at those times give occasion of four annual high waters.

But the true inequality of the natural days, arising from a complication of those two causes, sometimes crossing and sometimes promoting each other, though we should find some increases or decreases of the natural days at all those seasons answerable to the respective causes, and perhaps of tides proportionably thereunto, yet the longest and shortest natural days absolutely of the whole year, arising from this complication of causes, are about those times of Allhallondtide and Candlemas, about which those annual high tides are found to be; as will appear by the tables of equation of natural days. And therefore I think we may with very good reason cast this annual period upon that cause, or rather complication of causes. For, as we before showed in the menstrual and diurnal, there will, by this inequality of natural days, arise a physical acceleration and retardation of the earth's mean motion, and accordingly a casting of the waters backward or forward, either of which will cause an accumulation or high water.

It is true, that these longest and shortest days do fall rather before than after Allhallondtide and Candlemas, to wit the ends of October and January; but so do also sometimes those high tides: and it is not yet so well agreed amongst astronomers what are all the causes, and in what degrees, of the inequality of natural days, but that there be diversities among them, about the true time: and whether the introducing of this new motion of the earth in its epicycle about this common centre of gravity, ought not therein also to be accounted for, I will not now determine; having already said enough, if not too much, for the explaining of this general hypothesis, leaving the particularities of it to be adjusted according to the true measures of the motions; if the general hypothesis be found fit to be admitted.

Yet this I must add, that whereas I cast the time of the daily tides to be at all places when the moon is there in the meridian; it must be understood of open seas, where the water hath such free scope for its motions, as if the whole globe of earth were equally covered with water; well knowing that in bays and inland channels, the position of the banks, and other like causes, must needs make the times to be much different from what we suppose in the open seas; and likewise, that even in the open seas, islands and currents, gulfs and shallows, may have some influence, though not comparable to that of bays and channels. And moreover, though I think that seamen do commonly reckon the time of high water in the open seas to be when the moon is there in the meridian, as this hypothesis would cast it; yet I do not take myself to be so well furnished with a history of tides, as to assure myself of it, much less to accommodate it to particular places and cases.

Having thus dispatched the main of what I had to say concerning the sea's ebbing and flowing: had I not been already too tedious, I should now proceed to give a further reason, why I do introduce this consideration of the common centre of gravity in reference to astronomical accounts. For indeed that which may possibly seem at first to be an objection against it, is with me one reason for it.

It may be thought, perhaps, that if the earth should thus describe an epicycle about the common centre of gravity, it would, by this its change of place, disturb the celestial motions, and make the apparent places of the planets, especially some of them, different from what they would otherwise be. For though so small a removal of the earth as the epicycle would cause, especially if its semidiameter should not be above $1\frac{1}{3}$ of the earth's semidiameter, would scarce be sensible, if at all, to the remoter planets, yet as to the nearer it might.

Now though what Galilæo answers to a like objection in his hypothesis, that it is possible there may be some small difference which astronomers have not yet been so accurate as to observe, might here perhaps serve the turn; yet my answer is much otherwise, to wit, that such difference hath been observed, and hath very much puzzled astronomers to give an account of. About which you will find Mr. Horrocks, in some of his letters, whereof I did formerly, upon the command of the Royal Society, make an extract, was very much perplexed; and was fain, for want of other relief, to have recourse to somewhat like Kepler's amicable fibres, which did, according to the several positions of the moon, accelerate or retard the moon's motion; which amicable fibres he had no affection to at all, as there appears, if he could any other ways give account of those little inequalities; and would much rather, I doubt not, have embraced this notion of the common centre of gravity, to solve the phenomenon, had it come to his mind, or been suggested to him. And you find that other astronomers have been seen to bring in, some upon one supposition some upon another, some kind of menstrual equation, to solve the inequalities of the moon's motion, according to her synodical revolution, or different aspects, of new moon, full moon, &c. beside what concerns her own periodical motion.

For which this consideration of the common centre of gravity of the earth and moon, is so proper a remedy, especially if it shall be found precisely to answer those phænomena, which I have not examined, but am very apt to believe, that it is so far from being with me an objection against it, that it is one of the reasons which make me inclinable to introduce it.

I must, before I leave this, add one consideration more; that if we shall upon these considerations think it reasonable, thus to consider the common centre of gravity of the earth and moon; it may as well be thought reasonable, that the like consideration should be had of Jupiter and his four satellites, which, according to the complication of their several motions, will somewhat change the position of Jupiter as to that common centre of gravity of all these bodies; which yet, because of their smallness, may chance to be so little, as that, at this distance, the change of this apparent place may not be discernible. And what is said of Jupiter is in the like manner to be understood of Saturn and his satellite, discovered by Huygens: For all these satellites are to their principals as so many moons to the earth. And I do very well remember, in the letters before cited, Mr. Horrocks expresses some such little inequalities in Saturn's motion, of which he could not imagine what account to give, as if (to use his expression) this crabbed old Saturn had despised his youth. Which for aught I know might well enough have been accounted for, if at that time the satellite of Saturn had been discovered, and that Mr. Horrocks had thought of such a notion as the common centre of gravity of Saturn and his companion, to be considerable as to the guiding of his motion.

You have now, in obedience to your commands, an account of my thoughts as to this matter, though yet immature and unpolished: What use you will please to make of them I shall leave to your prudence, &c.

An Appendix, written by Way of Letter to the Publisher, being an Answer to some Objections made by several Persons to the preceding Discourse. N° 16, p. 281.

I received yours, and am very well contented that objections be made against my hypothesis concerning tides: being proposed but as a conjecture to be examined; and upon that examination rectified, if there be occasion; or rejected if it will not hold water.

1. To the first objection of those you mention, That it appears not how two bodies that have no tie can have one common centre of gravity; that is, for so I understand the intendment of the objection, can act or be acted in the same manner as if they were connected: I shall only answer, that it is harder to show how they have than that they have it. That the loadstone and iron have somewhat equivalent to a tie, though we see it not, yet by the effects we know. And it would be easy to show that two loadstones at once applied in different positions to the same needle, at some convenient distance, will draw it, not to point directly to either of them, but to some point between both; which point is, as to those two, the common centre of attraction; and it is the same as if some one loadstone were in that point. Yet have these two loadstones no connection or tie, though a common centre of virtue, according to which they jointly act. And as to the present case, how the earth and moon are connect-

ed, I will not now undertake to show, nor is it necessary to my purpose; but that there is somewhat that does connect them, as much as what connects the loadstone and the iron which it draws, is past doubt to those who allow them to be carried about by the sun, as one aggregate or body, whose parts keep a respective position to one another: Like as Jupiter with his four satellites, and Saturn with his one. Some tie there is that makes those satellites attend their lords, and move in a body; though we do not see that tie, nor hear the words of command. And so here.*

2. To the second objection, that at Chatham and in the Thames the annual spring tides happen about the equinoxes; not (as this hypothesis doth suppose elsewhere to have been observed) about the beginning of February and November: if their meaning be, that annual high tides do then happen, and then only; if this prove true it will ease me of half my work. For it is then easily answered, that it depends upon the obliquity of the zodiac; the parts of the equinoctial answering to equal parts of the zodiac, being near the solstitial points greatest, and near the equinoctial points least of all. But beside this annual vicissitude of the equinoxes, not to say of the four cardinal points, which my hypothesis doth allow and assert, I believe it will be found that there is another annual vicissitude answering to the sun's apogæum and perigæum. And that the greatest tides of all will be found to be upon a result of these two causes co-operating: which (as doth the inequality of natural days, depending on these same causes) will light nearer the times I mention. To what is said to be observed at Chatham and in the Thames, contrary to that I allege as observed in Rumney Marsh, I must at present ἀπέχειν, and refer to a *melius inquirendum.* If those who object this contrary observation, shall after this notice find upon new observations heedfully taken, that the spring-tides in February and November are not so high as those in March and September; I shall then think the objection very considerable. But I do very well remember, that I have seen in November very high tides at London, as well as in Rumney Marsh. And the time is not yet so far past, but that it may be remembered (by yourself or others then in London) whether in November last, when the tides were so high at Dover, at Deal, at Margate, and all along the coast from thence to Rumney Marsh, as to do in some of those places much hurt, (and in Holland much more,) whether I say, there were not also at the same time at London very high tides. But a good diary of the height and time both of high-water and low-water, for a year or two toge-

* It is curious to observe how near, in these ingenious speculations, Dr. Wallis approached to the universal principle of attraction of all matter, almost ever since so successfully employed in the system of the universe.

ther, even at Chatham or Greenwich; but rather at some place in the open sea, or at the Land's End in Cornwall, or on the west parts of Ireland; or at St. Helen's, or the Bermudas, &c. would do more to the resolving of this point than any verbal discourse without it.

3. To the third objection, that supposing the earth and moon to move about a common centre of gravity; if that the highest tides be at the new moon, when the moon being nearest to the sun, the earth is farthest from it, and its compound motion at the swiftest; and that the tides abate as the earth approaches nearer, till it comes into the supposed circle of her annual motion: it may be demanded why do they not still abate as the earth comes yet nearer to the sun, and the swiftness of its compound motion still slackens? And so, why have we not spring tides at the new moon, when the motion is swiftest, and neap tides at full moon, when the motion is slowest, but spring tides at both?—the answer, if observed, is already given in my hypothesis itself. Because the effect is indifferently to follow either upon a sudden acceleration, or a sudden retardation. (Like as a loose thing lying on a moving body; if the body be thrust suddenly forward, that loose thing is cast back or rather left behind, not having yet obtained an equal impetus with that of the body on which it lies; but if stopped or notably retarded, that loose incumbent is thrown forward by its formerly contracted impetus not yet qualified, or accommodated to the slowness of the body on which it lies.) Now both of these happening, the one at the new moon, the other at the full moon, do cause high tides at both.

4. To the fourth objection, that the highest tides are not at all places about the new moon and full moon; and particularly, that in some places of the East Indies the highest tides are at the quadratures: I must first answer in general, that as to the particular varieties of tides in several parts of the world, I cannot pretend to give a satisfactory account, for want of a competent history of tides, &c. Because, as is intimated in what I wrote in the general, the various positions of channels, bays, promontories, gulfs, shallows, currents, trade-winds, &c. must needs make an innumerable variety of accidents in particular places, of which no satisfactory account is to be given from the general hypothesis, though never so true, without a due consideration of all those: which is a task too great for me to undertake, being so ill furnished with materials for it. And then as to the particular instance of some places in the East Indies, where the highest tides are at the quadratures, I suppose it may be chiefly intended of those about Cambaia and Pegu; at which places, beside that they are situated at the inmost parts of vast bays or gulfs, they have also vast in-draughts of some hundred miles within land; which when

the tides are out do lie in a manner quite dry; and may therefore very well be supposed to participate the effect of the menstrual tides many days after the cause of them happens in the open sea; upon a like ground as in straits and narrow channels, the diurnal tides happen some hours later than in the ocean. And a like account must be given of particular accidents in other places, from the particular situation of those places, as bays, channels, currents, &c.

5. To the fifth objection, that the spring tides happen not with us just at the full and change, but two or three days after; I should with the more confidence attempt an answer, were I certain whether it be so in the open seas, or only in our channels. For the answers will not be the same in both cases. If only in our channels, where the tides find a large in-draught, but not in the open seas, we must seek the reason of it from the particular position of these places. But if it be so generally in the wide open seas; we must then seek a reason of it from the general hypothesis. And till I know the matter of fact, I know not well which to offer at; lest, whilst I attempt to solve one, I should fall foul of the other. I know that mariners use to speak of spring tides at the new and full of the moon; though I have still had a suspicion that it might be some days after, as well in the open seas as in our narrower channels; of which suspicion you will find some intimations even in my first papers: but this, though I can admit, yet, because I was not sure of it, I durst not build upon it. The truth is, the flux and reflux of water in a vessel by reason of the jogging of it, though it follow thereupon, yet is for the most part discernible some time after. For there must upon that jog be some time for motion, before the accumulation can have made a tide. And so I do not know but that we must allow it in all the periods. For as the menstrual high tide is not till some days after the full and change; so is the diurnal high water about as many hours after the moon's coming to south; I mean at sea, for in channels it varies to all hours, according as they are nearer or further from the open sea: and the annual high tides of November and February somewhat later than (what I conjecture to be from the same causes) the greatest inequalities of the natural days happening in January and October. But this though I can admit, yet (till I am sure of the matter of fact) I do not build upon. And since it has hitherto been the custom to speak with that laxness of expression, assigning the times of new moon, full moon, and quadratures, with the moon's coming to south, for what is near those times; I did not think myself obliged in my conjectural hypothesis, to speak more nicely. If the hypothesis for the main of it be found rational, the niceties of it are to be adjusted in time from particular observation.

Having thus given you some answers to the objections you signify to have been made by several persons to my hypothesis, and that in the same order your paper presents them to me: I shall next give you some account of the two books which you advised me to consult; so far as seems necessary to this business; which upon your intimation I have since perused, though before I had not.

And first, as to that of Isaac Vossius, De motu Marium et Ventorum; though I do not concur with him in his hypothesis, that all the great motions of the seas, &c. should arise only from so small a warming of the water as to raise it (where most of all) not a foot in perpendicular, (as in his 12th chapter,) or that there is no other connexion between the moon's motion and the tide's menstrual period, than a casual synchronism, which seems to be the doctrine of his 16th and 18th chapters; beside many other things in his philosophy which I cannot allow; yet I am well enough pleased with what is historical in it, of the matter of fact; especially if I may be secure that he is therein accurate and candid, not wresting the phænomena to his own purpose. But I find nothing in it which induces me to vary from my hypothesis. For granting his historicals to be all true, the account of the constant current of the sea westward, and of the constant eastern blasts, &c. within the tropics, is much more plausibly, and I suppose truly rendered by Galilæo long since from the earth's diurnal motion; (which near the equator describing a greater circle than nearer the poles, makes the current to be there more conspicuous and swift, and consequently the eddy or re-current motion nearer the poles, where this is more remiss;) than can easily be rendered by so small a tumor as he supposes. Not to add that his account of the progressive motion, which he fancieth to follow upon this tumefaction, and by acceleration to grow to so great a height near the shore (as in chap. 13 and 14) is a notion which seems to me too extravagant to be solved by any laws of statics. And that of the moon's motion only synchronizing with the tides casually, without any physical connexion, I can very hardly assent to. For it can hardly be imagined that any such constant synchronism should be in nature, but where either the one is the cause of the other, or both depend upon some common cause. And where we see so fair a foundation for a physical connection, I am not prone to ascribe it to an independent synchronism. In sum, his history doth well enough agree with my hypothesis; and I think the phænomena are much better solved by mine than his.

And then as to Gassendus, in his discourse De Æstu Maris, I find him, after the relating of many other opinions concerning the cause of it, inclining to that of Galilæo, ascribing it to the acceleration and retardation of the earth's motion, compounded of the annual and diurnal; and moreover attempting to give an account of the menstrual periods from the earth's carrying the moon about itself as Jupiter doth his satellites, which together with them is carried about by the sun as one aggregate; and that the earth with its moon is to be supposed in like manner to be carried about by the sun as one aggregate, cannot be reasonably doubted, by those who entertain the Copernican hypothesis, and do allow the same of Jupiter and his satellites. But though he would thus have the earth and moon looked upon as two parts of the same moved aggregate, yet he does still suppose, as Galilæo had done before him, that the line of the mean motion of this aggregate, (or, as he calls, *motus equabilis et viluti medius*), is described by the centre of the earth, about which centre he supposes both its own revolution to be made, and an epicycle described by the moon's motion, not by another point, distinct from the centres of both, about which, as the common centre of gravity, as well that of the earth as that of the moon, are to describe several epicycles. And for that reason fails of giving any clear account of this menstrual period. And in like manner, he proposes the consideration as well of the earth's aphelium and perihelium as of the equinoctial and solstitial points, in order to find a reason of the annual vicissitudes; but does not fix upon any thing in which he can acquiesce: and therefore leaves it *in medio* as he found it.

It had been more agreeable to the laws of statics, if he had, as I do, so considered the earth and moon as two parts of the same moveable, (not so, as he doth, *alium in centro et sequentem præcise revolutionem axis, alium remotius ac velut in circumferentia,* but) so as to make neither of them the centre, but both out of it, describing epicycles about it: like as, when a long stick thrown in the air, whose one end is heavier than the other, is whirled about, so as that the end which did first fly foremost becomes hindmost; the proper line of motion of this whole body is not that which is described by either end, but that which is described by a middle point between them; about which point each end in whirling describes an epicycle. And indeed, in the present case, it is not the epicycle described by the moon, but that described by the earth, which gives the menstrual vicissitudes of motion to the water; which would, as to this, be the same if the earth so move, whether there were any moon to move or not; nor would the moon's motion, supposing the earth to hold on its own course, any whit concern the motion of the water.

But now, after all our physical or statical considerations, the clearest evidence for this hypothesis, if it can be had, will be from celestial observations, as for instance, (see fig. 5) supposing the sun at S, the earth's place in its annual orb at T, and Mars (in opposition to the sun or near it) at M, from whence Mars should appear in the zodiac at γ, and will at full moon be seen there to be;

the moon being at C, and the earth at c: and the like at the new moon. But if the moon be in the first quarter at A, and the earth at a; Mars will be seen, not at γ, but at α, too slow; and when the moon is at B, and the earth at b, Mars will be seen at β yet too slow; till at the full moon, the moon at C, the earth at c, Mars will be seen at γ, its true place, as if the earth were at T. But then, after the full, the moon at D, the earth at d, Mars will be seen, not at γ but at δ, too forward, and yet more, when the moon, at the last quarter, is at E, the earth at e, and Mars seen at ε. If therefore Mars, when in opposition to the sun, be found (all other allowances being made) somewhat too backward before the full moon, and somewhat too forward after the full moon, and most of all at the quadratures; it will be the best confirmation of the hypothesis. The like may be fitted to Mars in other positions, *mutatis mutandis*; and so for the other planets.

But this proof is of like nature as that of the parallax is of the earth's annual orb, to prove the Copernican hypothesis. If it can be observed, it proves the affirmative, but if it cannot be observed, it doth not convince the negative, but only proves that the semidiameter of the earth's epicycle is so small, as not to make any discernible parallax. And indeed I doubt that will be the issue. For the semidiameter of this epicycle being little more than the semidiameter of the earth itself, or about $1\frac{1}{3}$ thereof, (as is conjectured in the hypothesis, from the magnitudes and distances of the earth and moon compared), and there having not as yet been observed any discernible parallax of Mars, even in his nearest position to the earth; it is very suspicious, that here it may prove so too. And whether any of the other planets will be more favourable in this point, I cannot say.*

Animadversions of Dr. Wallis, upon Mr. Hobbes's† late Book‡, De Principiis et Ratiocinatione Geometrarum. Written to a Friend. N° 16, p. 289.

Since I saw you last I have read over Mr. Hobbes's book Contra Geometras, or De Principiis et Ratiocinatione Geometrarum, which you then showed me.

* Although Dr. Wallis did not strike out the true cause and theory of the tides; yet the numerous arguments and reflections here employed are so ingenious in other respects, as to render the paper on the whole a very important composition.

† Thomas Hobbes, the author of this book, was born at Malmsbury in 1588, and died in 1679, being 91 years of age. He studied at Oxford, and afterwards travelled through Europe several times, as governor to different young noblemen; on which occasions, holding a distinguished rank as a philosopher and a general scholar, he cultivated an intimate personal acquaintance with Descartes, Mersenne, Gassendi, Galileo, and other eminent philosophers; with whom, after his

A new book of old matter: containing but a repetition of what he had before told us more than once, and which hath been answered long ago.

In which, though there be faults enough to offer ample matter for a large confutation, yet I am scarce inclined to believe that any will bestow so much pains upon it. For, if that be true, which in his preface he saith of himself, *aut solus insanio ego, aut solus non insanio,* it would either be needless, or to no purpose. For, by his own confession, all others, if they be not mad themselves, ought to think him so: and therefore as to them a confutation would be needless; who, it is like, are well enough satisfied already, at least out of danger of being seduced. And, as to himself, it would be to no purpose. For, if he be the mad man, it is not to be hoped that he will be convinced by reason; or, if we all be so, we are in no capacity to attempt it.

But there is yet another reason why I think it not to need a confutation. Because what is in it has been sufficiently confuted already: and so effectually as that he professes himself not to hope that this age is like to give sentence for him, whatever *nondum imbuta posteritas* may do. Nor doth there appear any reason why he should again repeat it, unless he can hope that what was at first false may by oft repeating become true.

I shall therefore, instead of a large answer, only give you a brief account of what is in it, and where it has been already answered.

The chief of what he has to say in his first ten chapters against Euclid's definitions amounts but to this, that he thinks Euclid ought to have allowed his point some bigness, his line some breadth, and his surface some thickness.

But where in his dialogues he solemnly undertakes to demonstrate it, for it is there his 41st proposition, his demonstration amounts to no more than this; that unless a line be allowed some latitude it is not possible that his quadratures can be true. For finding himself reduced to these inconveniences; 1. That his geometrical construction would not consist with arithmetical calculations, nor with what Archimedes and others have long demonstrated. 2. That the arch of a circle must be allowed to be sometimes shorter than its chord, and sometimes longer than its tangent. 3. That the same straight line must be allowed

return, he corresponded on scientific subjects. On the breaking out of the civil wars he retired into France, where he became mathematical preceptor to the Prince of Wales, afterwards Charles the 2d, who had also fled to that country, and who, after the restoration, granted a pension to Mr. Hobbes for his life. He wrote much on a variety of subjects, philosophy, mathematics, poetry, law, polity, &c. in most of which his opinions have been accounted heterogeneal. In consequence he was almost always involved in warm and contentious disputes, particularly with the learned Dr. Wallis, who attacked Hobbes's pretended quadrature of the circle, &c.

‡ See a former notice of this book, at p. 85.

at one place only to touch, and at another place to cut the same circle, with others of like nature; he finds it necessary, that these things may not seem absurd, to allow his lines some breadth, (that so, as he speaks, while a straight line with its outside doth at one place touch the circle, it may with its inside at another place cut it, &c.) But I should sooner take this to be a confutation of his quadratures than a demonstration of the breadth of a mathematical line.

And what he now adds being to this purpose; that though Euclid's Σημεῖον, which we translate a point, be not indeed *nomen quanti*; yet cannot this be actually represented by any thing, but what will have some magnitude; nor can a painter, no not Apelles himself, draw a line so small, but that it will have some breadth; nor can thread be spun so fine, but that it will have some bigness, is nothing to the business, for Euclid does not speak either of such points, or of such lines.

He should rather have considered of his own expedient, that when one of his broad lines, passing through one of his great points, is supposed to cut another line proposed, into two equal parts; we are to understand the middle of the breadth of that line passing through the middle of that point, to distinguish the line given into two equal parts. And he should then have considered further, that Euclid by a line means no more than what Mr. Hobbes would call the middle of the breadth of his; and Euclid's point is but the middle of Mr. Hobbes's. And then, for the same reason that Mr. Hobbes's middle must be said to have no magnitude, (for else not the whole middle, but the middle of the middle will be in the middle, and the whole will not be equal to its two halves, but larger than both by so much as the middle comes to;) Euclid's lines must as well be said to have no breadth, and his points no bigness.

In like manner, when Euclid and others do make the term or end of a line, a point; if this point have parts or greatness, then not the point, but the outer half of this point ends the line, for that the inner half of that point is not at the end is manifest, because the outer half is beyond it: and again, if that outer half have parts also; not this, but the outer part of it, and again the outer part of that outer part, and so in *infinitum*. So that as long as any thing of line remains, we are not yet at the end; and consequently if we must have passed the whole length before we be at the end, then that end (or *punctum terminans)* has nothing of length; for when the whole length is past, there is nothing of it left. And if Mr. Hobbes tells us that this end is not *punctum*, but only *signum* (which he does allow *non esse nomen quanti)* even this will serve our turn well enough. Euclid's Σημεῖον, which some interpreters render by *signum*, others have thought fit, with Tully, to call *punctum*: but if Mr. Hobbes like not that name, we will not contend about it. Let it be *punctum*, or let it be *signum*, or if he please he may call it *vexillum*. But then he is to remember that this is only a controversy in grammar, not in mathematics; and his book should have been intituled Contra Grammaticos, not Contra Geometras. Nor is it Euclid, but Cicero, that is concerned, in rendering the Greek Σημεῖον, by the Latin *punctum*, not by Mr. Hobbes's *signum*. The mathematician is equally content with either word.

What he saith concerning the angle of contact amounts but to thus much, that by the angle of contact he does not mean either what Euclid calls an angle, or any thing of that kind; and therefore says nothing to the purpose of what was in controversy between Clavius and Peletarius, when he says, that an angle of contact hath some magnitude: but that by the angle of contact, he understands the crookedness of the arch; and in saying the angle of contact has some magnitude, his meaning is, that the arch of a circle has some crookedness, or is a crooked line: and that of equal arches, that is the more crooked whose chord is shortest; which I think none will deny, for who ever doubted but that a circular arch is crooked? or that of such arches equal in length, that is the more crooked whose ends by bowing are brought nearest together? But why the crookedness of an arch should be called an angle of contact, I know no other reason, but because Mr. Hobbes loves to call that chalk which others call cheese.

What he says here of ratios or proportions, and their *calculus* for eight chapters together, is but the same for substance, as he had formerly said in his 4th dialogue and elsewhere. To which you may see a full answer in my Hobbius Heauton-tim. from page 49 to p. 88, which I need not here repeat.

The quadrature of a circle, which here he gives us, is one of those twelve of his, which in my Hobbius Heauton-timorumenus, are already confuted; and is the ninth in order, as I there rank them. I call it one, because he takes it so to be; though it might as well be called two. For as there, so here, it consists of two branches, which are both false; and each overthrows the other.

His demonstration of chap. 23, where he would prove that the aggregate of the radius and of the tangent of 30 degrees, is equal to a line whose square is equal to 10 squares of the semiradius, is confuted not only by me in the place forecited, where this is proved to be impossible, but by himself also in this same chapter, where he proves sufficiently, and doth confess, that this demonstration and the 47th proposition of the first of Euclid cannot be both true. But, which is worst of all, whether Euclid's proposition be false or true, his demonstration must needs be false. For he is in this dilemma: if that proposition be true, his demonstration is false, for he grants that they cannot be both true. And again, if that proposition be false, his demonstration is so too; for this depends upon that, and therefore must fall with it.

His section of an angle *in ratione data*, has no other foundation than his supposed quadrature. And therefore that being false, this must fall with it.

His appendix, wherein he undertakes to show a method of finding any number of mean proportionals between two lines given, depends upon the supposed truth of his 22d chapter, about dividing an arch in any proportion given, as himself professes; and as is evident by the construction which supposes such a section. And therefore, that failing, this falls with it.

And so this whole structure falls to the ground. And withal, the proposition 47, El. 1, doth still stand fast, which he tells us must have fallen, if his demonstrations had stood: and so geometry and arithmetic do still agree, which he tells us had otherwise been at odds.

Observations, made in several Places, of the Eclipse of the Sun, June 22, 1666. N° 17, p. 295.

The observations that were made at London by Mr. Willughby, Dr. Pope, Mr. Hook, and Mr. Philips, are these:—

The Eclipse began at 5 h. 43 m.

			h.	m.			h.	m.
It was darkened.	$\frac{3}{11}$ diam.	at	6	00	5 dig.	at	7	6
	4 digits.......	at	6	7	4 dig.	at	7	13
	5 dig....... ..	at	6	13	3 dig.	at	7	20
	6 dig.........	at	6	21	2 dig.	at	7	26
	7 dig.	at	6	39$\frac{1}{2}$	1 dig.	at	7	32
	6 dig.........	at	6	57	0 dig.	at	7	37

Its duration hence appears to have been one hour and 54 m. Its greatest obscurity somewhat more than 7 digits. About the middle, between the perpendicular and westward horizontal radius of the sun, viewing it through Mr. Boyle's 60-foot telescope, there was perceived a little of the limb of the moon without the disk of the sun: which seemed to some of the observers to come from some shining atmosphere about the body either of the sun or moon.

They observed the figure of this eclipse, and measured the digits, by casting the figure through a five-foot telescope, on an extended paper fixed at a certain distance from the eye-glass, and having a round figure; all the diameters being divided by six concentric circles into 12 digits.

The Madrid Observations.

The observations made at Madrid by a noble member of the Royal Society, the Earl of Sandwich, as they were sent to the Lord Viscount Brounker, are these: At Madrid, the eclipse began about five o'clock in the morning: at 5h. 15m. the sun's altitude was 6° 55′.

The middle of it was at 6h. 2m. the sun's altitude 15 deg. 5′.

The end was exactly at 7h. 5m. the sun's altitude 25 deg. 24′.

The duration was 2h. 4m.

37 Parts of the sun's diameter remained light, and 63 parts dark.

The Paris Observations.

The observations made at Paris by Monsieur Payen, are these:

The eclipse began there at 5h. 44m. 52sec. mane; and ended at 7h. 43m. 6sec. So that its whole duration was 1h. 58m. 14sec. The greatest obscuration they assign to have been 7 dig. 50′; but they add, that it seemed to have been greater by 3 minutes; which M. Payen imputes to a particular motion of libration of the sun's globe, which kept that luminary in the same phasis for the space of 8 min. and some seconds, as if it had been stopped in the midst of its course; rather than to a tremulous motion of the atmosphere, as Scheiner would have it.

They conceive that the apparent diameters were almost equal; seeing that in the phasis of six digits, the circumference of the moon's disk passed through the centre of that of the sun, so as that two lines drawn through the two horns of the sun, made with the common semidiameter two equilateral triangles.

Then they observe, that the beginning and the middle of this eclipse happened to be in the north east hemisphere, and the end in the south east. The first contact of the two disks was observed in the superior limb of the sun's disk in respect to the vertical line, and in the inferior in respect to the ecliptic: But the middle and the end were seen in the superior limb, in respect both of the vertical and the ecliptic: And, what to this author seems extraordinary, both the beginning and the end of this eclipse happened to be in the oriental part of the sun's disk. Lastly, They take notice, that by their observations it appears that there is but little exactness in all the astronomical tables predicting the quantity, beginning and duration of this eclipse.

Inquiries and Directions concerning Tides. By Dr. Wallis. *N° 17, p. 297.*

Dr. Wallis having, in his Hypothesis of Tides, intimated, that he had reason to believe that the annual spring tides happen to be rather about the beginnings of February and November than the two equinoxes, desires that some intelligent persons at London or Greenwich, but rather nearer the sea or upon the sea-shore, would make particular observations of all the spring tides, (new moon and full moon) between this and the end of November; and noting the hour and the perpendicular height; that it may be seen whether those in September or November be highest: And also to observe the low waters; which may be easily done by a mark made upon any standing post in the water, by any waterman or other intelligent person who dwells by the water side.

Considerations and Inquiries concerning Tides. By Sir ROBERT MORAY. *N° 17, p.* 298.

It being observed, that tides increase and decrease regularly at several seasons according to the moon's age, so as about the new and full moon, or within two or three days after, in the western parts of Europe, the tides are at the highest, and about the quarter moons at the lowest, the former being called spring tides the other neap tides; and that according to the height and excesses of the tides, the ebbs in opposition are answerable to them, the highest tide having the lowest ebb, and the lowest ebb the highest tide; the tide from the quarter to the highest spring tide increasing in a certain proportion, and from the spring tide to the quarter tide decreasing in like proportion as is supposed; and also the ebb rising and falling constantly after the same manner: It is wished that it may be inquired, in what proportion these increases and decreases, risings and fallings, happen to be in regard of one another?

And it is supposed, from some observations made by the above-mentioned gentleman, that the increase of the tides is made in the proportion of sines, the first increase exceeding the lowest tide in a small proportion, the next in a greater, the third greater than that; and so on to the middlemost, where the excess is greatest, diminishing again from that to the highest spring tide; so as the proportions before and after the middle do greatly answer one another, or seem to do so. And likewise from the highest spring tide to the lowest neap tide, the decreases seem to keep the like proportions; the ebbs rising and falling in like manner and in like proportions. All which is supposed to take place when no wind or other accident causes an alteration.

And as it is observed, that upon the main sea-shore the current of the ebbings and flowings is sometimes swifter and sometimes slacker than at others, so as in the beginning of the flood the tide moves faster but in a small degree, increasing its swiftness constantly till towards the middle of the flood, and then decreasing in velocity again from the middle to the top of high water; it is supposed that in equal spaces of time, the increase and decrease of velocity, and consequently the degrees of the risings and fallings of the same in equal spaces of time, are performed according to the proportion of sines.

But it is conceived that the said proportion cannot hold exactly and precisely in regard of the inequalities that happen in the periods of the tides, which are commonly observed and believed to follow certain positions of the moon in respect of the equinox, which are known not to keep a precise and constant course; so that there not intervening equal portions of time between one new moon and another, the moon's return to the same meridian cannot be always performed in the same time; and consequently there must be a like variation of the tides in the velocity, and in the risings and fallings of the tides, as to equal spaces of time. And the tides from new moon to new moon being not always the same in number, as sometimes but 57, sometimes 58, and sometimes 59, without any certain order of succession, this is another evidence of the difficulty of reducing this to any great exactness. Yet it is very desirable that observations be constantly made of all these particulars for some months and even years together. And because such observations will be the more easily and exactly made where the tides rise highest, it is presumed, that a fit apparatus being made for the purpose, they may be made about Bristol or Chepstow best of any places in England, because the tides are said thereabouts to rise to 10 or 12 fathoms; as upon the coast of Brittany in France, they do to 13 and 14.

In order to which this following apparatus is proposed to be made use of. In some convenient place upon a wall, rock, or bridge, &c. let there be an observatory, standing as near as may be to the brink of the sea or upon some wall; and if it cannot be well placed just where the low water is, there may be a channel cut from the low water to the bottom of the wall, rock, &c. The observatory to be raised above the high water 18 or 20 feet; and a pump placed perpendicularly by the wall, reaching above the high water as high as conveniently may be. Upon the top of the pump a pulley to be fastened, for letting down into the pump a piece of floating wood, which, as the water comes in, may rise and fall with it. And because the rising and falling of the water amounts to 60 or 70 feet, the counterpoise of the weight that goes into the pump to hang upon as many pulleys as may serve to make it rise and fall within the space by which the height of the pump exceeds the height of the water; and because by this means the counterpoise will rise and fall slower, and consequently by less proportions than the weight itself, the first pulley may have upon it a wheel or two to turn indexes at any proportion required, so as to give the minute parts of the motion, and degrees of risings and fallings. All which is to be observed by pendulum watches that have minutes and seconds.

To prevent the water from rising and falling with an undulation, it will be proper that the hole by which the water enters be less than half the bore of the pump.

The particular observations to be made may be as follows:

1. The degrees of rising and falling of the water every quarter of an hour, or as often as may be, from the periods of the tides and ebbs; to be observed night and day for two or three months.

2. The degrees of the velocity of the motion of the water every quarter of an hour, for some whole tides together; to be observed by a second pendulum watch, and a log fastened to a line of about 50 fathoms wound about a wheel.

3. Exact measures of the heights of every highest tide and ebb, from one spring tide to another, for some months, or rather years.

4. The exact heights of spring tides and spring ebbs for some years together.

5. The direction of the wind at every observation of the tides; the times of its changes, and the degrees of its strength.

6. The state of the weather as to rain, hail, mist, haziness, &c. and the times of its changes.

7. At the times of observing the tides, the height of the thermometer, the baroscope, and the hygroscope; the age of the moon, and her azimuths and her place; also the sun's place.

An Account of several Books lately published. N° 17, p. 301, &c.

I. Johannis Hevelii Descriptio Cometæ, Anno Æræ Christianæ MDCLXV exorti; unà cum Mantissa Prodromi Cometici, Observationes omnes prioris Cometæ MDCLIV, ex iisque genuinum motum accurate deductum, cum Notis et Animadversionibus, exhibens.

This book undertakes to give an account of the second of the two late comets, which appeared when the other was scarce extinct; concerning which, the author assigns both its true place and its proper motion; adding a fair delineation of its course, with the genuine representations of its head and train, in each day of its appearing; and subjoining a general description of some of its more remarkable phænomena. It was observed at Dantzick by the author from April 6, half past one in the morning, till April 20, at three in the morning. During which time it went with a moderate velocity, making 46 deg. in its orbit, according to the order of the signs, moving from the breast of Pegasus towards the head of Andromeda, and the left horn of Aries; having, as it is presumed, taken its rise from above Sagittary, and run through the breast of Antinous, under Aquila and the Dolphin to the said Pegasus, and so on, as is already expressed.

The head of it is in the book described of a colour like that of Jupiter, all along much brighter than that of the former comet, though of a somewhat less magnitude; having in its middle only one round, but very bright and large, kernel or speck, resplendent like gold, and encompassed with another more dilute and seemingly uniform matter: its tail being at first about 17 deg. and afterwards 20, and sometimes 25 deg. long, and divaricated towards the end, which became narrower and more attenuated as it approached towards the sun.

He observes, that this star in progress of time became retrograde, whence it came to pass, that in the months of June and July it did not appear again before the rising of the sun, though the sun left it far behind; whereas, if it had proceeded toward the eye of Taurus, it would have appeared again in the morning.

He maintains that this comet was not the same with the former, which he thinks may be demonstrated only by a due delineation of both their courses upon the globe.

He concludes, 1st, With an intimation of his sense concerning two other comets pretended to have been lately seen; one at Rome, about the girdle of Andromeda, in the months of February and March, 1664; the other in Germany, in Capricorn, about Saturn in the head of Sagittary, during the months of September and October, 1665. 2dly, With an advertisement of what he has done in that important work for the advancement of astronomy, and the due restitution of the fixed stars.

The second part of this book endeavours to justify the author's observations touching the former comet, excepted against by M. Auzout in several particulars.

From all which he pretends to prove that he has not erred in his observation of February 18, nor been prepossessed by any hypothesis, nor deluded by any fixed star, as M. Auzout thinks; but that near the first star of Aries there then appeared a phenomenon most like to that comet that was seen some days before, if compared with the observations made thereof February 12, 13, 14; though he will not hitherto positively determine, whether that phenomenon which appeared to him February 18, was indeed that very comet which he saw with his naked eye and observed with his geometrical instruments, the said 12, 13, and 14th days of February; or whether it was another; and whether he had lost that comet, which moved towards the second star in Aries: but leaves to the learned world, and particularly to the Royal Society, to judge of this and the other particulars in controversy.

II. Isaacus Vossius * de Nili et Aliorum Fluminum Origine.

* Isaac Vossius, born at Leyden in 1618, was a man of considerable talents and learning, and spent a long life, of 71 years, in close study. He received several honours and emoluments from

In Number 14 of these Transactions was given an account of the cause of the inundation of the Nile, by M. de la Chambre: This book is to give another, not only of the inundation, but also of the origin of that and of other rivers, showing,

1. That those subterraneous channels, through which several philosophers teach that the sea discharges itself into the rivers, are not only imaginary, but useless; as it is impossible for the water to rise from the subterraneous places up to the mountains, where commonly the sources of rivers are.

2. He explains why, if a pipe be put into a basin full of water, the water is seen more raised in the pipe than in the basin, and rises higher according as the pipe is narrower. On the contrary, if the same pipe be put into a basin full of quicksilver, the quicksilver is lower in the pipe than in the basin. He adds, that this observation makes nothing for the explication of the origin of rivers; because, though it be true that the water by this means rises above its level, yet it does never run out at the top of the pipe. Having said this, he answers to the other arguments commonly alleged to maintain this opinion.

3. He pretends that all rivers proceed from collections of rain-waters; and that as the water that falls upon hills gathers more easily together than that which falls in plains, therefore it is that rivers usually take their source from hills. Thence also it happens that there are more rivers than torrents in the temperate zones; and, on the contrary, more torrents than rivers in the torrid zone; for, as in hot climates the mountains are far higher, the water that descends from them with impetuosity runs away in a short time, and forms such collections of water as soon dry up; but in cold climates the waters run off but slowly, and are renewed and recruited by rain before they are quite dried up; because the hills are there lower, and the bed of rivers has less declivity.

Having thus discoursed of rivers in general, he treats of the Nile in particular; and there, he proves by many recent relations, that the sources of the Nile are on this side of the equinoxial in Æthiopia, of which he gives

different sovereigns. He held a correspondence with Queen Christina of Sweden, who employed him in some literary commissions; and at the Queen's request he visited that country, and taught her the Greek language; though she afterwards discarded him, on hearing that he intended to write against Salmasius. In 1663 he received a present of money from Louis the 14th of France, with a complimentary letter from the minister Colbert. In 1670 he came to England, when King Charles the 2d made him canon of Windsor; though he knew his character well enough to say, there was nothing that Vossius refused to believe except the bible. Vossius was author of many writings; and died at Windsor in 1689; leaving, as supposed, the best private library in the world; which was afterwards purchased by the university of Leyden.

an accurate map, correcting many faults which geographers have committed in the description of Abyssinia, which they believe to be much greater than it really is.

This supposed, he easily shows why the Nile yearly overflows about the end of June: for, as at that time there falls much rain in Æthiopia, it must needs be that the Nile, whose source is in that country, should then overflow when those rains begin, and again subside when they cease.

There are besides in this book two other tracts. In the first, M. Vossius maintains that the soul of animals is nothing but fire; that there are no invisible atoms; nor so much as any pores even in the skin of man. Here he treats also of refractions, and alleges the examples of several persons, who have seen the sun by means of refraction when he was really under the horizon.

In the second, he discourses of some points of the mechanics; and relates, among other things, that the arrows and battering rams *(Aries)* of the ancients, did as much execution as our muskets and cannons.

III. Le Discernement du Corps et de l'Ame, par M. de Cordemoy.

This French treatise examines the different operations of the soul and body, and the secret of their union; pretending to discover to every one, what he is, and what is transacting within him.

Patterns of the Tables proposed to be made for observing Tides. By Sir Rob. Moray. N° 18, p. 311.

Of these patterns, one is for marking the precise time of high and low water during one month, that is between new and new moon, or full and full moon. The other for marking the degrees of the risings and fallings of the water in equal spaces of time, and the velocity of its motion at several heights; with the degrees of heat and cold, &c. these particulars having every one a ruled column allotted for each.

Other Inquiries concerning the Sea. By Mr. Boyle. N° 18, p. 315.

What is the proportion of salt in the water of different seas? Whether in the same sea it be always the same? If not, how does it differ?

What is the relative gravity of sea waters to fresh waters, and what to one another? Whether it varies not in summer and winter, and on other accounts? And whether in the same season its gravity proceeds only from the greater or less proportion of salt in it, and not sometimes from other causes? And what are the differing gravities of the sea water, according to the climates?

What are the odours, colours and tastes observable in sea water?

What is the depth of the sea in several places, and the order of its increase and decrease; and whether the bottom of the sea always rises towards the shore, unless accidentally interrupted?

Of the bottom of the sea, and how it differs from the surface of the earth, in reference to the soil, and evenness or roughness of the superficies; also the stones, minerals and vegetables to be found there?

What the figuration of the seas from north to south, and from east to west, and in the several hemispheres and climates?

What communication there is of seas by straits and subterraneous conveyances?

Of the motion of the sea by winds, and how far storms reach downwards?

Of the grand motions of the body of the sea, especially of the tides, their nature and differences?

What power the sea has to produce or hasten putrefaction in some bodies, and to preserve others; as wood, cables, &c.?

Of the power ascribed to the sea to eject dead bodies, succinum, ambergris?

Of the shining of the sea in the night?

What are the medical virtues of the sea, especially against hydrophobia?

What is its virtue to manure land? And what are the plants that thrive best with sea water?

Some Considerations concerning the parenchymatous Parts of the Body. By Dr. EDMUND KING. *N° 18, p.* 316.

The parenchymatous parts of the body are (this writer observes) generally supposed by anatomists to be in very many places wholly void of vessels, and to be designed chiefly to fill up cavities and interstices between the vessels, to bolster up the same, and to convey them through the parts; whereas on examination he has found the parenchyma of the liver, lungs, spleen, kidneys, &c. full of vessels.*

Observations on Petrifications. By Dr. BEALE. *N° 18, p.* 320.

A good history of petrifaction, and of the manner in which nature proceeds in that operation, would be very satisfactory. For if we could attain to the mode of causing petrifactions by art, directing the process at pleasure, such an art might be very useful, being applied to prevent the generation of the

* This is the substance of Dr. King's communication, rather prolix, and containing some observations not altogether relevant to the subject.

stone and gravel in human bodies, or to dissolve those concretions after they have been there formed.*

To such reflections an extraordinary case is added by Dr. Beale, being a narrative of a stone not long since taken out of the womb [or bladder ?] of a woman in his neighbourhood, near Trent in Somersetshire, by incision, and who afterwards was perfectly cured, though she had carried the stone with extreme torments for eight or nine years. The operation was performed at Easter 1666; after which time he weighed the stone and found it wanted somewhat of four ounces, though it had lost of the weight it formerly had, being very light for a stone of that bulk. He further describes it to be of a whitish colour, lighter than ash-colour. It had no deep asperities, and had somewhat of an oval figure, but less at one end than a hen's egg, and larger and blunter at the other than a goose's egg.

Of Worms that eat Stones and Mortar. By M. de la Voye. *N°* 18, *p.* 321.

In a large and very ancient wall of free-stone in the Benedictins Abbey at Caen in Normandy, facing southward, are found many stones so eaten by worms, that one may run one's hand into most of the cavities. In these cavities there is abundance of live worms with their excrement, and of the stone dust which they eat. Between many of the cavities there remain but leaves as it were of stone, very thin, which part them. I have taken some of these living worms, which I found in the eaten stone, and put them into a box with several bits of the stone, leaving them there together for the space of eight days; and then opening the box, the stone seemed to me so sensibly eaten, that I could no longer doubt of it.

These worms are inclosed in a shell which is grayish, and of the size of a barley-corn, sharper at one end than the other. By means of an excellent microscope I have observed, that this shell is all overspread with little stones and small greenish eggs; and that there is at the sharpest end a little hole, by which these creatures discharge their excrement; and at the other end a somewhat larger hole, through which they put out their heads and fasten themselves to the stones they gnaw. They are not so shut up but that sometimes they come out and walk abroad. They are all black, about two lines of an inch long, and three quarters of a line broad. Their body is distinguished into several plies or

* The mode in which petrifactions are formed is now very well understood; but the knowledge of this natural process throws no light either upon the origin of urinary calculi (whose chemical composition is different) or upon the means of preventing their formation.

folds, and near their head they have three feet on each side, which have but two joints, resembling those of a louse. When they move, their body is commonly upwards, with their mouth against the stone. They have a large head, somewhat flat and even, of the colour of a tortoise shell, with some small white hair. Their mouth is also large, where may be seen four kinds of jaw-bones, lying crosswise, which they move continually, opening and shutting them like a pair of compasses with four branches. The jaws, on both sides of the mouth, are all black; the nether-jaw has a point like the sting of a bee, but uniform. They draw threads out of their mouth with their fore-feet, using that point to range them, and to form their shells of them. They have ten eyes, very black and round, which appear to be larger than a pin's head. There are five of them on each side of the head.

Besides these worms, I have found that mortar is eaten by an immense number of small creatures, of the size of cheese mites. These have but two eyes, and are blackish. They have four feet on each side pretty long. The point of their muzzle is very sharp, like that of a spider.

You may observe more of them in walls exposed to the south than in others. The worms that eat the stone live longer than those that eat the mortar, which scarcely live above eight days. I have observed all their parts with a good microscope, without which, and a great deal of attention, it is difficult to see them well.

I have seen other very old walls altogether eaten, as those of the Temple at Paris, where I could find no worms, but the cavities were full of shells of various kinds, diversely figured and turned; all which I believe to be little animals petrified.

Some promiscuous Observations made in Somersetshire, and imparted by Dr Beale. N° 18, p. 323.

This paper contains only notice of a quantity of oak trees found lying a little under the surface of the earth near Bridgwater. Also of some water that cattle would not drink in the driest seasons. And of quantities of dead eels found in holes under the ice.

To find the Year of the Julian Period, by a new and very easy Method. By M. J. De Billy. N° 18, p. 324.*

Multiply the solar cycle by 4845, and the lunar by 4200, and that of the in-

* Father James de Billy was a learned French Jesuit, in the 17th century, born in Compeigne 1602, and died at Dijon 1679, at 77 years of age. He wrote several mathematical works; as,

diction by 6916. Then divide the sum of the products by 7980, which is the Julian period: the remainder of the division, without having regard to the quotient, will be the year required.

Ex. gr. Let the cycle of the sun be 3; of the moon 4; and of the indiction 5. Multiply 3 by 4845, and you have 14535; and 4 by 4200, which gives 16800; also 5 by 6916, which gives 34580. The sum of the products is 65915; which being divided by 7980, gives 8 for the quotient, and the number 2075, which remains, is the year of the Julian period.

An Account of some Books lately published. N° 18, *p.* 324.

I. Tentamina Physico-Theologica de Deo, sive Theologia Scolastica, ad Normam Novæ et Reformatæ Philosophiæ concinnata, et duobus libris comprehensa.

II. Honorati Fabri, Soc. Jesu Theologi, Tractatus duo; quorum Prior est de Plantis et de Generatione Animalium; Posterior, de Homine.

As the matter of this book is considerable, so is the order and dependence of all its parts excellent; since all the propositions are ranged according to a geometrical method, and so well disposed that the latter always suppose the former, and seem to depend all of them upon certain evident principles, whence they flow by a natural consequence.

III. Relation du Voyage de l'Eveque de Beryte, par la Turquie, la Perse, les Indes, &c. jusques au Royaume de Siam, et autres lieux; par M. de Bourges, Prestre, &c.

This author, employing his pen chiefly, according to his design, to give an account of the success the undertakers of this voyage had in propagating the Christian faith in the remoter parts of the world, and relating, on that occasion, what number of churches they have founded in Cochin China and the kingdom of Tonquin, in which latter alone he affirms that there are more than three hundred thousand Christians.

A Petrifaction. By Mr. PH. PACKER. N° 19, *p.* 329.

Near Wadley, a mile from Farringdon in Berks, there grows an elm containing near a tun of timber, which has now lost the top, and has grown hollow. From the butt of the tree one of the spreading limbs having been formerly cut off with an axe, that part of the butt, being about 1½ foot above ground, and inward with-

1. Opus Astronomicon; 2. Nova Geometriæ Clavis Algebra; 3. Tabulæ Lodovicæ; 4. Diophantus Redivivus; and perhaps some others.

A method of investigating such rules as that above, may be seen in Dr. Hutton's translation of Montucla's Philosophical Recreations, vol. iii. p. 231.

in the trunk of the tree, has contracted a petrified crust about the thickness of a shilling, all over the woody part within the bark; the marks of the axe also remaining very conspicuous with this petrified crust upon it. By what means it should thus happen cannot well be conceived, as there is no water near it, and as the part is above the ground, and the tree still growing: unless, being perhaps cut at a season when the sap was flowing, the oozing of the sap might become petrified by the air, and the tree grow rotten and hollow inward since that time.

Inquiries concerning Mines. By Mr. Boyle. *N° 19, p.* 330.

These queries are reduced by the author to six heads:

I. The neighbouring country about the mines. II. The soil where the mines are. III. The signs of mines. IV. The structure and other particulars belonging to the mines themselves. V. The nature and circumstances of the ore. VI. The reduction of the ore into metal.

I. Queries on the neighbouring Country.

Whether the country be mountainous, plain, or distinguished with vales? Whether high or low, &c. fruitful or barren, cold or temperate, rocky or not, hollow or solid? Whether they run in ridges or seem confusedly placed; and, if the former, what way the ridges run north and south, &c. &c.

Whether the country be barren or fruitful? What it produces, and what it most abounds in?

What cattle it nourishes, and what their nature and peculiarities?

The diseases and lives of the inhabitants longer or shorter than ordinary?

The rivers, brooks, springs, and other waters; and the nature of them?

Whether the air be dry or moist, hot or cold, clear or foggy, thick or thin, heavy or light; any subterraneous streams, and what they are?

II. The Soil.

Whether the soil be stony; and, if it be, what kind of stones it abounds with? Whether it be clayey, marly, chalky, &c.?

III. The Signs or Indications of Mines.

By what signs they know or guess that there is a mine in such a place?

Whether it be observed that trees and other greater plants seem to have their tops burned, or other leaves or outsides discoloured? Or whether there be any plants that affect to grow over such mines, or the contrary?

What colour the stones and pebbles take in the brooks, springs, or other waters?—Whether the waters, by their taste, smell, ponderousness, &c. indicate any minerals?—Whether snow will not lie, or frost continue so long, or dew be collected upon the ground, more or less than usual?—And whether the dew or the rain there will discolour white linen or woollen cloths, spread over night on the surface of the ground?—The state of the place as to thunder and lightning, storms or earthquakes, nocturnal lights and fiery meteors?—What indications from mists, or the *virgula divinatoria?*—What signs afford a probability of mines, or direction for following a vein over hills, valleys, lakes, &c.

The strata under the surface: their number, nature, depth, order, thickness, &c. —Indications by certain stones or marcasites, &c.—Peculiar kind of earths. Also heats or damps.

IV. The Structure, &c. of Mines.

The depth of the shaft or grove: what its width and extent? Whether the vein run horizontal or dip? what inclination it has? What its flexures; and what its directions?—What air-shaft belongs to the mine? Whether it be single, double, &c. and if several, what their distances, situation, &c.?—What waters spring in the mine; at what depths; and their quantity and nature?—What expedients and engines are employed to free the mines from water?—What are the conditions, number, &c. of the adits?—Whether the mine be troubled with damps, and of what kind they are?—What methods the mine men use in following the vein and tracing their passages under ground (which they call plumming and dyalling) according to the several exigencies? And whether they employ the instruments made with the help of the loadstone, the same way that is usual, or what other instruments?—What methods they take to secure themselves from the uncertainty incident to the guidance of magnetic needles from the iron-stone ore that they may meet with under ground?—How the miners deal with the rocks and spars they often meet with before they come at the ore? Whether they use fire to soften, calcine, or crack them? How they employ it, and with what measure of success?—What instruments they use to break the rock, &c.? And how the mine men work; whether naked or clothed? And what lights they use to work by?—How veins are followed, lost, and recovered? And how several miners work on the same vein? And what is the best way of getting all the ore in a vein and most conveniently?—How they convey out their ore and other things that are to be carried out of the mine?

V. The Nature of the Ore.

Whether the ore runs in the vein, or lies dispersed in scattered pieces; or be divided partly into a vein, and partly into loose masses; or like a wall between two rocks, as it were in a cleft; or be interspersed in the firm rock, like speckled marble? Or be found in grains like sand or gravel; as store of excellent tin is said to be found in some parts of Cornwall at the sides and in the channels of running waters; or whether the ore be of a softer consistence, like earth or lome, as there is lead-ore in Ireland holding store of silver; and iron ore in the north parts of Scotland and elsewhere? And what is observable in it as to weight, colour, mixture, &c.

Whether any part of the metal be found in the mine perfect and complete?

Whether the mine affords any parcels of metal that seem to grow like plants, as I have sometimes seen silver growing as it seemed out of stone or spar, almost like blades of grass; as also large grains of a metal, which appeared to me, and which those that tried some of it, affirmed to be gold, abounding in a stony lump, that seemed to consist chiefly of a peculiar kind of spar.

The depth of the vein. Its concomitants or coat.—What are the principal qualities of these extraneous substances: as that spar is white, but transparent, almost like coarse crystal, heavy, brittle, easily divisible into flakes, &c? Caulk of a different texture, white, opake, and like a stone, but much more ponderous. Mundick I have had of a fine golden colour.

Whether the vein be inclosed every way in its coats; or whether it only lie between them?—Whether the vein be uninterrupted, or in some places broken off; and whether it be abruptly or not; and whether it be by vales, brooks, gullets, &c.?—How wide the interruptions are; what signs whereby to find the vein again?

What proportion of metal it affords? as in our iron mines it is observed, that about three tuns of iron-stone will afford one tun of metal: and I have had lead ore, which an ingenious man, to whom I recommended such trials, affirmed to me to afford three parts in four of good lead.

Whether the ore be pure in its kind from other metals? and if not, of what metals it participates; and in what proportion? as I have known it observed, that lead ore, that is poor in its own metal, affords more silver than other; and I remember that the ore lately mentioned, being rich in lead, scarce afforded us upon the cuppel, an atom of silver. And Matthesius informs us, that a little gold is not unfrequently found in iron-ore. And I have by me some gold that never endured the fire, taken out of a lump of tin ore.

VI. Reduction of the Ore into Metal.

What are the mechanic and previous operations, as beating, grinding, washing, &c. that are used to separate the ore from the heterogeneous bodies, and prepare it for the fire?—Whether mercury be made use of, to extract the nobler from the baser metals?—Whether the burning and beating of the ore be used to prepare it for the furnace?—What flux-powders, and other ways they have to try and examine the goodness of the ore in small quantities?—Whether, when they work in great, they use to melt the ore with any flux or additaments, or only by the force of the fire, or in any way between both?—What kind of furnaces they use to melt the ore in?—What kinds of fuel, and what quantities of it, are wont to be employed in the furnace, within the compass of a day or week? How much is put in at a time? How often it is renewed? and how much ore in a determinate time, as a week or a day, is wont to be reduced to metal?—Whether the ore be melted by a wind excited by the fire itself, as in wind ovens? Or by the course of waters? Or actuated by the blast of bellows?—What contrivance they have to let or take out the metal that is in fusion, and cast it into bars, sows, pigs, &c.—What clay, sand, or mould they let it run or pour it through? And after what manner they refrigerate it?—What are the ways of distinguishing them, and estimating the goodness of the metals?—Whether they do any thing to the metal after it is once brought to fusion, and if need be, melt it over again, to give it a melioration? As when iron is refined and turned into steel; and what distinct furnaces and peculiar ways of ordering the metals are employed to effect this improvement?—Whether in those places where the metal is melted, there be not elevated some corpuscles, that stick to the upper parts of the furnace or building? And if there be, whether they be barely fuliginous and recrementitious exhalations or at least in part, metalline flowers?—Whether the metal, being brought to fusion, affords any recrements?—Whether, after the metal has been once melted, the remaining part of the ore being exposed to the air, will in time be impregnated, or ripened so as to afford more metal?

Answers to Queries. By M. Hevelius. N° 19, p. 346.

The inquiries you proposed to me, I imparted to several of my learned friends: but hitherto I have attained an answer but to few particulars. Among the rest you will find a letter of the learned Johannes Schefferus, professor in the Swedish university at Upsal, wherein he discourses handsomely of several things, being ready to entertain a literary commerce with you about such matters. Touching amber I am almost of the same mind with him, that it is a kind of fossil pitch or bitumen; seeing it is not only found on the shore of the Prussian sea, but also digged up in subterraneous places, some German miles distant from the sea, and that not only in sandy, but also in other hills of firmer earth; of which I myself have seen pretty big pieces.—Concerning swallows, I have frequently heard fishermen affirm, that they have here often fished them out of the lakes in the winter, but I never have seen it myself.—Whilst I am writing this, I receive letters out of Denmark, advertising me, that those two learned men, Thomas and Erasmus Bartholin, intend shortly to answer the same quæries. Next winter, if God vouchsafe me life and health, I purpose to make a journey to Konigsberg, where I hope to learn many things, especially about amber.

I am very glad to understand that you have so good telescopes, as to make such considerable observations in Jupiter and Mars, as you have lately done in England. I have no leisure now, by reason of the observations of the fixed stars, which I now almost constantly am employed about, to do any thing in the advancing of telescopes. I am obliged to finish the catalogue of the fixed stars; having meanwhile the contentment to find that many excellent persons labour about the improvement of optic glasses. Before I conclude, I must give notice to the lovers of astronomy, that on the 24th of September (N. S.) of this year, I have observed that new star in Pectore Cygni, which from the year 1662, until this time, has been almost altogether hid, not only with my naked eye, like a star of the 6th or 7th magnitude, but also with a very large sextant. It is still in the very same part of the heavens where it was formerly, from An. 1601, to almost 1662. For its distance from Scheat Pegasi I have found 35° 51′ 20″, and from Marcab, 43° 10′ 50″, which distances, as I have found in my journal, are equal to those which I observed An. 1658, the 1st of November. For the distance from Scheat at that time was 35° 51′ 20″, and from Marcab 43° 10′ 25″: It is therefore certain, that it is the self same star which Kepler first saw An. 1601, and continued until 1662. But whether in time it will grow larger and larger, or be lost again, time must show. He that will observe this star, must take care lest he mistake those three more southern ones of the sixth magnitude, and now in a manner somewhat brighter than the new star in Collo Cygni. The highest of those three is distant from Scheat Pegasi 36° 25′ 45″; the middlemost from the same 37° 25′ 20″, and the lowest 36° 4′ 30″.*

Answers to Queries. By M. Joh. Schefferus. N° 19, p. 349.

That he is confident the Royal Society of England will do much good for the advancement of useful knowledge.

That he conceives amber to be a kind of fossil pitch, whose veins lie at the bottom of the sea; believing that it is hardened in course of time, and by the motion of the sea cast on shore.

That it is most certain, that swallows sink themselves towards autumn into lakes, no otherwise than frogs; and that many have assured him of it, who

* For the figure of this constellation, see p. 137.

had seen them drawn out with a net together with fishes, and put to the fire and thereby revived.

That it is also very true that many animals there grow white in winter, and recover their own colour in summer. That himself has seen and had hares, which about the beginning of winter and spring were half white, and half of their native colour; that in the midst of winter he never saw any but all white. That foxes also are white in winter, and squirrels grayish, mixed of dark and white colour.

That fishes are killed by reason of the ice not being broken: but first, in ponds only or narrow lakes; next, in such lakes only where the ice is pretty thick; for, where it is thin, they die not so easily; lastly, that those fishes that lie in slimy or clayey ground die not so soon as others.

That neither oil, nor a strong brine of bay salt, is truly congealed into ice, in those parts, viz. at Upsal in Sweden.

That the frost pierces into the earth two cubits or Swedish ells; and what moisture is found in it is white like ice: That waters, if standing, freeze to a greater depth, even to three such ells or more; but those that have a current less: that rapid rivers freeze not at all; nor ever-bubbling springs; and that these latter seem even to be warmer in winter than in summer.

The Method observed in Transfusing the Blood out of one Animal into another. By the Hon. ROBERT BOYLE. *N° 20, p. 353.*

The method here described was first practised by Dr. Lower of Oxford. Take up the carotid artery of the dog or other animal, whose blood is to be transfused into another of the same or a different kind, and separate it from the nerve of the eighth pair, and lay it bare above an inch. Then make a strong ligature on the upper part of the artery not to be untied again: but an inch below, viz. towards the heart, make another ligature of a running knot, which may be loosened or fastened as there shall be occasion. Having made these two knots, draw two threads under the artery between the two ligatures; and then open the artery and put in a quill, and tie the artery upon the quill very fast by those two threads, and stop the quill with a stick. After this make bare the jugular vein in the other dog about an inch and half long; and at each end make a ligature with a running knot, and in the space betwixt the two running knots drawn under the vein two threads, as in the other: then make an incision in the vein, and put into it two quills, one into the descendent part of the vein, to receive the blood from the other dog, and carry it to the heart; and the other quill put into the other part of the jugular vein, which comes from the head (out of which the second dog's own blood must run into dishes.) These two quills being put in and tied fast, stop them with a stick till there be occasion to open them.

All things being thus prepared, tie the dogs on their sides towards one another so conveniently that the quill may go into each other, (for the dogs' necks cannot be brought so near, but that you must put two or three several quills more into the first two to convey the blood from one to another.) After that unstop the quill that goes down into the first dog's jugular vein, and the other quill coming out of the other dog's artery; and by the help of two or three other quills put into each other, according as there shall be occasion, insert them into one another. Then slip the running knots, and immediately the blood runs through the quills as through an artery very impetuously. And immediately as the blood runs into the dog unstop the other quill, coming out of the upper part of his jugular vein (a ligature being first made about his neck, or else his other jugular vein being compressed by one's finger;) and let his own blood run out at the same time into dishes, (yet not constantly, but according as you perceive him able to bear it) till the other dog begin to cry and faint, and fall into convulsions, and at last die by his side.

Then take out both the quills out of the dog's jugular vein, and tie the running knot fast, and cut the vein asunder, (which you may do without any harm to the dog, one jugular vein being sufficient to convey all the blood from the head and upper parts, by reason of a large *anastomosis*, whereby both the jugular veins meet about the *larynx*.) This done, sow up the skin and dismiss him, and the dog will leap from the table and shake himself and run away, as if nothing ailed him.

And this I have tried several times, before several in the universities, but never yet upon more than one dog at a time for want of leisure and convenient supplies of several dogs at once. But when I return I doubt not but to give you a fuller account, not only by bleeding several dogs into one, but several other creatures into one another, as you did propose to me before you left Oxford; which will be very easy to perform, and will afford many pleasant and perhaps not unuseful experiments.

But because there are many circumstances necessary to be observed in the performing of this experiment, and that you may better direct any one to do it without any danger of killing the other dog that is to receive the other's blood, I will mention two or three.

First, That you fasten the dogs at such a convenient distance, that the vein nor artery be not stretched; for then being contracted, they will not admit or convey so much blood.

Secondly, That you constantly observe the pulse beyond the quill in the dog's jugular vein (which it acquires from the impulse of the arterial blood:) For if that fails, then it is a sign the quill is stopped by some congealed blood, so that you must draw out the arterial quill from the other, and with a probe open the passage again in both of them, that the blood may have its free course again. For it must be expected when the dog that bleeds into the other has lost much blood, that his heart will beat very faintly, and then the impulse of blood being weaker, it will be apt to congeal the sooner, so that at the latter end of the work you must draw out the quill oftener and clear the passage; if the dog be faint hearted as many are, though some stout fierce dogs will bleed freely and uninterruptedly till they are convulsed and die. But to prevent this trouble, and make the experiment certain, you must bleed a great dog into a little one, or a mastiff into a cur, as I once tried, and the little dog bled out at least double the quantity of his own blood, and left the mastiff dead upon the table; and after he was untied he ran away and shaked himself, as if he had been only thrown into water. Or else you may get three or four several dogs prepared in the same manner; and when one begins to fail and leave off bleeding administer another, and I am confident one dog will receive all their blood, (and perhaps more) as long as it runs freely, till they are left almost dead by turns: provided that you let out the blood proportionably as you let it go into the dog that is to live.

Thirdly, I suppose the dog that is to bleed out into dishes will endure it the better if the dogs that are to be administered to supply his blood be of near an equal age, and fed alike the day before, that both their bloods may be of a near strength and temper.

There are many things I have observed upon bleeding dogs to death, which I have seen since your departure from Oxford, whereof I shall give you a relation hereafter; in the mean time, since you were pleased to mention it to the Royal Society, with a promise to give them an account of this experiment, I could not but take the first opportunity to clear you from that obligation, &c.

So far this letter: the directions whereof having been carefully observed by those who were employed to make the experiment, have hitherto been attended with good success; and that not only upon animals of the same species (as two dogs first, and then two sheep) but also upon some of very different species, as a sheep and a dog; the former emitting, the other receiving.

Note only, that instead of a quill a small crooked thin pipe of silver or brass, so slender that the one end may enter into a quill, and having at the other end that is to enter into the vein and artery a small knob, for the better fastening them to it with a thread, will be much fitter than a straight pipe or quill for this operation: for so they are much more easy to be managed.

It is intended that these trials shall be prosecuted to the utmost variety the subject will bear: As by exchanging the blood of old and young, sick and healthy, hot and cold, fierce and fearful, lame and wild animals, &c. and that not only of the same but also of different kinds. For which end, and to improve this noble experiment, either for knowledge or use, or both, some ingenious men have already proposed considerable trials and inquiries; of which perhaps an account will be given hereafter. For the present we shall only subjoin some

Considerations about Experiments of this Kind.

1. It may be considered in them, that the blood of the emittent animal may after a few minutes of time, by its circulation, mix and run out with that of the recipient. Wherefore to be assured in these trials that all the blood of the recipient is run out, and none left in him but the adventitious blood of the emittent, two or three or more animals (which was also hinted in the method above) may be prepared and administered to bleed them all out into one.

2. It seems not irrational to guess aforehand, that the exchange of blood will not alter the nature or disposition of the animals upon which it shall be practised; though it may be thought worth while, for satisfaction and certainty, to determine that point by experiments. The case of exchanging the blood of animals seems not like that of grafting, where the scion turns the sap of the stock grafted upon into its nature; the fibres of the scion so straining the juice which passes from the stem to it, as thereby to change it into that of the scion; whereas in this transfusion there seems to be no such percolation of the blood of animals, whereby that of the one should be changed into the nature of the other.

3. The most probable use of this experiment may be conjectured to be, that one animal may live with the blood of another; and consequently, that those animals that want blood, or have corrupt blood, may be supplied from others with a sufficient quantity, and of such as is good, provided the transfusion be often repeated, by reason of the quick expense that is made of the blood.*

An Account of some Sanative Waters in Herefordshire. By Dr. Beale. *N° 20, p.* 358.

There are two springs, says the writer of this communication, in Herefordshire,

* When these experiments were first projected there was a degree of enthusiasm with respect to the result of them, which is not uncommon on such occasions. Time, however, has shown the vanity of those expectations which philosophers then entertained as to the possibility of removing diseases, and lengthening the natural term of life, by such means.

whereof one is within about a bolt or bow-shot of the top of the near adjoining hill of Malvern,* and has had a long and old fame for healing the eyes. When he was for some years molested with tetters on the back of one and sometimes of both his hands, notwithstanding all endeavours of his friends and skilful physicians, he was speedily healed by a neighbouring spring of far less fame. Moreover this spring healed very old ulcers on the legs of a poor fellow, after other applications had been useless. And by many trials upon his hands and the tetters, Dr. B. was persuaded that in long droughts and lasting dry frosts these waters were more effectually and more speedily healing than at other times.

Of Vitriolate Water, &c. By. Dr. BEALE. *N° 20, p.* 359.

I wish we had a full account of our salt springs at Droitwich near Worcester, and at Nantwich in Cheshire, and any other salt springs in England. It should be inquired at what distance they are from the sea, or from salt fluxes, from hills, and how deep in the vales? What the weight? Whether in droughts or long frosts the proportion of salt or weight increases? Whether the earth near the springs, or in their passage, has any peculiar ferment, or produces a brackishness, if it rests after it is well drained?

Inquiries for Turkey. By Mr. H. N° 20, p. 360.

1. In what part of Turkey the Rusma is to be found; and in what quantity? Whether the Turks employ it to any other uses besides that of the taking away of hair? Whether there be different kinds of it? How it is used to take off hair, and how to get store of it?

2. Whether the Turks do not only take opium themselves for strength and courage, but also give it to their horses, camels and dromedaries, for the same purpose, when they find them tired and faint in their travelling? What is the greatest dose any men are known to have taken of opium? and how prepared?

3. What effects are observed from their use, not only of opium, but also of coffee, bathing, shaving their heads, using rice; and why they prefer that which grows not unless watered, before wheat, &c.?

4. How their Damasco-steel is made and tempered?

5. What is their way of dressing and making leather, which though thin and supple, will hold out water?

* An excellent account of the Malvern spring, by Dr. Wall, is to be found in the 49th and 50th vols. of the Transactions.

6. What method they observe in breeding those excellent horses, they are so much famed for?

7. Whether they be so skilful in poisoning as is said; and how their poisons are curable?

8. How the Armenians keep meat fresh and sweet so long?

9. What arts or trades have they worth learning?

10. Whether there be a tree about Damascus, called mouslac, which every year about the month of December is cut down close by the root, and within four or five months shoots up again, bringing forth leaves, flowers and fruit, and bearing but one apple at a time?

11. Whether about Reame, in the southern part of Arabia Felix, there be grapes without any grains? And whether the people in that country live, many of them, to 120 years in good health?

12. Whether in Candia there be no poisonous creatures; and whether the serpents there are without poison?

13. Whether all fruits, herbs, earth, fountains, are naturally saltish in the isle of Cyprus? And whether those parts of this isle, which abound in Cyprus trees, are more or less healthful?

14. What store of amianthus in Cyprus; and how it is worked?

15. Whether mummies be found in the sands of Arabia, that are the dried flesh of men buried in those sandy deserts in travelling? And in what respects they differ from the embalmed ones?

16. Whether the parts about the city of Constantinople or Asia Minor be as subject to earthquakes now as formerly? And whether the eastern winds do not plague the said city with mists, and cause that inconstancy of weather it is subject to?

17. Whether the earthquakes in Zant and Cephalonia be so frequent, as now and then to happen nine or ten times a month? And whether these isles be not very cavernous?

18. What is the height of mount Caucasus, its position, temperament, &c.?

19. With what declivity the water runs out of the Euxine Sea into the Propontis? With what depth? And if the many tides and eddies called Euripi have any certain periods?

20. Whether the Caspian empties itself into the Euxine sea by any passage under ground?

21. By what inland passages they go to China?

22. Whether in the aqueducts they line the inside with as good plaster as the ancients did? and how it is made?

23. To inquire after those excellent works of antiquity with which that

country abounds. And particularly the size and structure of the aqueducts made about Constantinople by Solyman the Magnificent, &c.

Optic Glasses made of Rock Crystal. By EUSTACHIO DIVINI. *N° 20, p. 362.*

Though it be commonly believed that rock crystal is not fit for optic glasses, because there are many veins in it; yet Eustachio Divini made one of it, which he says proved an excellent one, though full of veins.*

An Account of the Use of the Grain of Kermes for Coloration. By M. VERNY, *Apothecary at Montpellier. N° 20, p. 362.*

The grain of kermes is here described to be an excrescence† growing upon the wood, and often upon the leaves of a shrub,‡ plentiful in Languedoc, and gathered in the end of May and the beginning of June, full of red juice. Two uses are mentioned of this grain, the one for medicine, the other for the dyeing of wool; of which last alone notice is here taken.

They take the kermes when ripe and spread it upon linen, turning it at first (whilst it abounds in moisture) twice or thrice a day, to prevent its heating. When there appears red powder among it, they separate it, passing it through a sieve; and then again spread out the grain upon the linen, until there be perceived the same red powder: and at the end, this red powder appears about and on the surface of the grain, which is still to be passed through a sieve till it render no more.

In the beginning, when the small red grains are seen to move, as they will

* It is a question whether those were true veins or only superficial strictures and slight scratches.

† These *grains* or *excrescences*, as they are called, are produced by an insect termed by naturalists *coccus ilicis*. The female insect punctures the bark and leaves of the shrub on which it is found; deposits its eggs in a sort of nidus thus formed, loses its original shape, and dies. Its reliquiæ, with the contained eggs, acquire the appearance of grains or excrescences (analogous to gall-nuts) from which issue, after a certain time (if no measures are taken to destroy them) a number of young insects. The red-powder, mentioned in the subsequent part of this paper, consists of larvæ destroyed by the process of sprinkling the grains with vinegar and afterwards drying them.

‡ The tree or shrub on which the kermes insect is found is the *quercus coccifera*, Linn. In the South of France, and in some of the provinces of Spain, numbers of people, chiefly women, (who for this purpose let their nails grow) are employed in picking these insects from the leaves and branches of this species of oak. They are used for dyeing woollen cloth of a scarlet colour, and constitute an article of commerce of no small consideration to both the above-mentioned countries. The colour they give is more durable than that of cochineal, but not so bright; hence a more frequent use of the latter in modern times.

do, they are sprinkled over with strong vinegar, and rubbed between one's hands; afterwards, little balls are formed of them, which are exposed to the sun to dry.

If this red powder should be let alone, without pouring vinegar or some other acid liquor upon it, out of every grain there would be formed a little fly, which would skip and fly up and down for a day or two, and at last changing its colour, fall down quite dead, deprived of all the bitterness which the grains whence they are generated had possessed.

The grain being altogether emptied of its pulp or red powder, is washed in wine, and then exposed to the sun. Being well dried, it is rubbed in a sack to render it bright; and then put up in small sacks, putting in the midst, according to the quantity the grain has afforded, 10 or 12 pounds (for a quintal) of the dust, which is the red powder that came out of it. And accordingly as the grain affords more or less of the said powder, dyers buy more or less of it.

It is to be noted, that the first red powder which appears issues out of the hole of the grain, that is, on the side where the grain adhered to the plant; and that which about the end appears sticking on the grain has been alive in the husk, having pierced its cover, though the hole whence it commonly issues remains close as to the eye.

Books lately Published. N° 20, p. 364.

I. Pinax Rerum Naturalium Brittaniarum continens Vegetabilia, Animalia et Fossilia, in hoc insula reperta, Inchoatus. Auth. Christophoro Merret, Med. D. et utriusque Societatis Rigiæ socio.

The learned author of this book has, by his laudable example of collecting together what natural things are to be found here in England, invited the curious in all parts of the world to attempt the like, and thereby to establish the much desired and highly useful commerce among naturalists, and to contribute every where to the composing of a genuine and full history of nature.

II. Placita Philosophica Guarini. The chief subject of this treatise is natural philosophy; upon many important questions whereof it enlarges, as those of the motion of the celestial bodies, of light, of meteors, and of the vital and animal functions; leaving sometimes the common opinions, and delighting in the defence of paradoxes.

III. Gustus Organum, per Laurentium Bellini, novissimè deprehensum. A Treatise on the Organ of Taste. By Lawrence Bellini.*

* Bellini was born at Florence in 1643, and studied at Pisa, where the expences of his education were defrayed by Ferdinand II. through whose interest he successively obtained the professorships of

This author proposing to himself to discover both the principal organ of taste and the nature of its object, begins with the latter, and examines first, what is taste? He judges that it is caused by nothing but salts, which being variously figured, affect the tongue variously; alleging this for his chief reason, that the salt which is extracted by chemists out of any mixt body whatever carries away with it all its taste. He adds that the teeth in grinding the food serve much to extract this salt; and that the teeth are so necessary for preparing the aliment, that certain animals which seem to have none have them in their stomach; that nature has put at the entry of the palate of those that are altogether destitute of them certain moveable inequalities, which are to them instead of teeth.†

But then, secondly, concerning the organ of taste he is of opinion, that it is neither the flesh, nor the tongue, nor the glandules called amygdalinæ, but those little eminences (papillæ) that are found upon the tongue of all animals. He observes,

1. That from the middle of the tongue to the root, as also towards the tip, there are found innumerable little eminences called papillæ, but from the tip of the tongue unto the string there are observed none at all.

2. He hath found by experiment that if you put sal ammoniac upon the places of the tongue, where those eminences are not, you will have no taste; but that you will perceive a taste as soon as you put any of that salt upon those parts of the tongue, where those eminences or papillæ are met with. He therefore infers that those eminences are the principal organ of taste.

logic, philosophy, physic, and anatomy, in that celebrated university. He was afterwards appointed physician to Cosmo III. but through the intrigues of his rivals he lost the favour and confidence of that prince; and being stigmatized as an enemy to religion, he experienced much uneasiness during his latter days, living in constant dread of popular fury. He published several medical and anatomical treatises, viz. De Urinis et Pulsibus; De Sanguinis Missione; De Febribus; De Morbis Capitis et Pectoris, &c. These have been collected into one 4to. volume, which has often been reprinted. Bellini was a man of great learning and very considerable talents, but was too fond of resorting to mechanical and mathematical principles for explaining the natural and morbid phænomena of the human body. He died at Florence in 1703, being 60 years of age.

† The teeth serve two purposes, viz. for seizing the food, as in animals of prey, and for mastication, in which operation the saliva becomes mixed with the food. In fishes the teeth are merely instruments of prehension or seizure, and are not at all subservient to the preparation of their food, the solvent power of the gastric juice not requiring in them any such aid. The assertion that certain animals have teeth in their stomach is absolutely false. The mechanical explanation here given of the sense of tasting is such as might be expected from one of the founders of the iatromathematical sect. It was long received, but is now justly discarded. Salts make the greatest impression upon the papillæ of the tongue, i. e. are the most sapid of all substances, not in consequence of their peculiar configuration, but of their ready and perfect solubility.

3. He assures that with a microscope many small holes may be seen in those risings or papillæ, at the bottom whereof there are small nerves, terminating there. But he directs to observe this in live and healthy, not in dead or diseased animals.

Having laid down these observations, he concludes, that taste is performed in the following manner, viz. that the particles of salt passing through those pores which pierce the papillary eminences, and penetrating as far as to the nerves, meet them there, and by means of their small points prick them ; which pricking constitutes taste. In the meantime he acknowledges, that before him Malpighi, professor at Messina, had made some of these discoveries.

Calculation of the late Solar Eclipse. By M. Hevelius. N° 21, p. 369.

This calculation is now useless.

The Figure of the Stars in the Constellation of Cygnus; together with the New Star in it, very lately seen by M. Hevelius again. N° 21, p. 372.*

The figure of that constellation with the new star in it was thus, hastily drawn, sent over by that observer.

* See No. 19, p. 127.

To Measure the Diameters of the Planets, and the Parallax of the Moon. By M. Auzout. N° 21, p. 373.

I applied myself last summer to measuring the diameters of the sun, moon, and the other planets, by a method which M. Picard and myself have esteemed the best of all that have been practised hitherto; since we can take the diameters to second minutes, being able to divide one foot into 24,000 or 30,000 parts, scarce failing as much as in one part only, or to three or four seconds. I can well assure you, that the diameter of the sun, taken in his apogee, has not been much less than 31 m. 37 or 40 sec. and certainly not less than 31 m. 35 sec. and that at present in his perigee it does not exceed 32 m. 45 sec. but may be less by a second or two. What is at present troublesome is, that the vertical diameter, which is the most easy to take, is diminished, even at noon, by eight or nine seconds, because of the refractions, which are much greater in winter than summer at the same height; and that the horizontal diameter is difficult, because of the swift motion of the heavens.

As for the moon, I never yet found her diameter less than 29 m. 44 or 45 sec. and I have not seen it exceed 33 m. or if it has, it was only by a few seconds. But I have not yet taken her in all situations of the apogees and perigees which happen, with the conjunctions and quadratures. I have found a way to know the parallax of the moon, by the means of her diameter: viz. if on a day when she is to be in her apogee or perigee, and in the most northerly signs, you take her diameter towards the horizon, and then towards the south, with her altitudes above the horizon. For if the observation of the diameters be exact, as in these situations the moon changes not considerably her distance from the earth in six or seven hours, the difference of the diameters will show the proportion there is of her distance, with respect to the semidiameter of the earth. The same would yet be practised better in the places where the moon passes through the zenith than here; for the greater the difference is of the heights, the greater is that of the diameters. If one were under the same meridian, or the same azimuth in two very distant places, and took at the same time the diameter of the moon, it would effect the same thing, though not so exactly.

From what has been said may be collected the reason of the observation which M. Hevelius has made in an eclipse of the sun, touching the increase of the moon's diameter near the end. For the moon's diameter must change in the eclipses of the sun according to the places where they happen, and according to the hour and height of the moon. And had the eclipse been in the evening the contrary would have happened; for the moon in that eclipse, beginning in the morning, was higher about the end than at the beginning, was nearer us, and consequently must appear larger; but if the eclipse should happen in the evening, she would be lower at the end, and therefore more distant from us, and consequently appear less. So also in two different places, one having the eclipse in the morning, and the other at noon, the moon must appear larger to him that hath it at noon: and she must likewise appear larger to those who shall have a less elevation of the pole under the same meridian, because the moon will be nearer them.

A Relation of the [supposed] Loss of the Method of preparing the Bononian Stone for shining. Anonymous. N° 21, p. 375.

In this short notice an apprehension is expressed lest the art of preparing and calcining the Bononian stone, so as to possess the property of absorbing and emitting light, should be lost.*

Description of a Swedish Stone, which affords Sulphur, Vitriol, Alum, and Minium. N° 21, p. 375.

There is a stone in Sweden of a yellow colour, intermixed with streaks of white, as if composed of gold and silver, and very heavy. It is found in firm rocks, and runs in veins, upon which they lay wood, and set it on fire. When the stone is thus heated they cast water upon it to make it split, and then dig it up with mattocks. This done, they break it into smaller pieces, and put it into iron pots, of the shape represented by fig. 1, pl. 4; the mouth of the one going into the other. These they place, the one in the oven upon an iron fork sloping, so that, the stone being melted, it may run into the other, which stands at the mouth of the oven, supported on an iron. The first running of the stone is sulphur.

The remainder of the burned stone is carried out and laid upon a high hill, where it lies exposed to the sun and air for the space of two years; it then takes

* The Bolognian stone is a barytic spar (sulphate of barytes). The art of preparing it so as to become phosphorescent (the accidental discovery of an Italian shoemaker) has never been lost, though many have made a great secret of it. The process is very simple. The stone being calcined and pulverized is made into thin cakes with mucilage, which, after being dried, are put upon charcoal and subjected to the heat of a reverberating furnace. In this state, if it be exposed to the sun's rays for a few minutes and immediately afterwards brought into the dark, it will appear luminous. The prepared cakes must be carefully preserved from damp. See Malpighi's observations on the Bolognian stone in the xiith vol. of the Transactions, and an account of Mr. Canton's method of making a phosphorus that will imbibe and emit light like the Bolognian stone in the lviiith vol. of the Transactions.

fire of itself, yielding a thin blue flame, scarce discernible in the day time, and leaving a blue dust behind it; which the workmen observe and mark with wooden pins. This they dig up and carry into the work-house, putting it into large tubs of water to infuse about 24 hours. The water they afterwards boil in kettles as we do saltpetre, and put it into cooling tubs, in which cross sticks are placed, and on them the vitriol fastens like sugar-candy.

The water that remains after the extraction of the vitriol is mixed with an eighth part of urine and the lyes of wood-ashes, which is again boiled very strong, and being set to cool in tubs with cross sticks, the alum fastens on these.

In the water which remains after the alum, is found a sediment, which, being separated from the water, is put into an oven, and wood laid upon it, and fired till it become red, which makes the minium, wherewith they paint their houses, and make plaster.

There is a kind of stone in the north of England yielding the same substances, except minium.*

A Shower of Ashes in the Archipelago. By Capt. WM. BADILY. *N° 21, p. 377.*

December 6, 1631, riding at anchor in the Gulf of Volo, about ten o'clock that night, it began to rain sand or ashes, and continued till two o'clock the next morning. It was about two inches thick on the deck, so that we threw it overboard with shovels, as we did snow the day before. The quantity of a bushel we brought home, and presented to several friends, especially to the masters of the Trinity House. There was no wind stirring when these ashes fell; and they not only fell in the places where we were, but likewise in other parts, as ships were coming from St. John d'Acre to our port; though at that time a hundred leagues from us. We compared the ashes together, and found them both alike.

Concerning Salamanders living in Fire. By M. STENO. *N° 21, p. 377.*

M. Steno states, that a knight called Corvini had assured him, that having cast a salamander, brought by him out of the Indies, into the fire, the animal thereupon swelled presently, and then vomited a quantity of thick slimy matter,

* The mineral above described is a pyritical aluminous ore, and occurs in many other countries as well as in Sweden. What the author terms *minium* was doubtless an oxyd of iron.

which put out the coals that were in contact with it, to which the salamander retired immediately, putting them out again in the same manner as soon as they rekindled, and by this means saving himself from the force of the fire for the space of two hours. The gentleman was then unwilling to hazard the creature any further: That afterwards it lived nine months: That he had kept it eleven months without any other food besides what it took by licking the earth on which it moved, and on which it had been brought out of the Indies; which at first was covered with a thick moisture, but being dried afterward, the urine of the animal served to moisten the same. After the eleven months, the owner having a mind to try how the animal would do upon Italian earth, it died three days after the earth had been changed.*

A Relation of an uncommon Accident in two aged Persons. By Mr. Colpresse. *N° 21, p.* 380.

The first of these relations sets forth that the Rev. Mr. Jos. Shute of Devonshire, when at the age of 81, cut a new tooth, viz. the third grinder in the upper jaw. The second relation states, that Maria Stert of the same county, aged 75, about the 40th year of her age lost three of her upper incisores or cutters, the other drawn out, and so remained toothless as to them for about 25 years; when she perceived that a new tooth came forth (without any pain) next the canini of the left cheek. And about two years after, another tooth grew out likewise without pain, close by the former: The first whereof never came to above half the length of her former cutters, the latter scarce breaking the skin. They both however proved serviceable till about six weeks since, when, as she was eating no hard, crusty or solid food, that tooth which came out first fell into her mouth, though she had not perceived it to be loose before, nor had suffered any pain from it. It had not a fang like other cutting teeth (incisores) but much less and shorter. The other abides firm and serviceable.

An Account of two Books. N° 21, p. 381.

I. Ismaelis Bullialdi† ad Astronomos Monita duo: Primum, de Stella Nova,

* The salamander (*Lacerta Salamandra.* Lin.) is a native of many parts of Europe, and is a moderately large lizard, of a black colour, with deep yellow spots and patches: it delights in damp, shady places, and is naturally provided with a whitish fluid, exsuding through the numerous and large pores of its skin; and which it sometimes even discharges to some little distance around it: in consequence of this it is not so immediately affected by the force of fire as an animal of a drier nature would be, but remains like the snail and many other creatures, unscorched for a small space of time. Whether the Indian salamander here mentioned was the same species or not, it is impossible to determine.

† Ismael Bulliald, the author of this book was born at Laon, in the Isle of France, in 1605, and

quæ in Collo Ceti ante annos aliquot visa est. Alterum, de Nebulosa in Andromedæ Cinguli parte Borea, ante biennium iterum orta.

The chief end of the author in publishing this tract seems to be to excite astronomers to a diligent observation, both of that new star in the neck of the Whale to be seen in February and March next; and of that other in the northern part of Andromeda's girdle, to be seen at this time.

As to the former of these stars he affirms, that, as it has appeared for many years in the said place, so it will in the beginning of March next appear equal to the stars of the third magnitude, or perhaps larger; and that about the end of the same month, if the crepuscle do not hinder, the greatest phasis of it will appear, if it keep the same analogy of motions and periods, which it observed from An. 1638 to An. 1664. Where he takes notice of the causes, why its two greatest appearances could not be seen An. 1664, 1665, 1666; and how he comes to know that in the beginning of March next it will equal, or even exceed the stars of the third magnitude; noting, that from the observations hitherto made of this star, it is manifest that the greatest phases every year anticipate by 32 or 33 days; forasmuch as An. 1660 its greatest appearance was about the end of October and the beginning of November; An. 1661, about the end of September or the beginning of October; An. 1662, about the end of August, &c. so that this year it must be in March, if the former analogy hold.

He collects also from the observations, that one period from the greatest phasis to the next consists of about 333 days: but that the interval of the time between the times of its beginning to appear equal to the stars of the sixth magnitude, and of its ending to do so, consists of about 120 days: And that its greatest appearance lasts about 15 days: All which yet he would have understood with some latitude.

This done, he proceeds to the investigation of the causes of the vicissitudes in the emersion and disappearance of this star, and having remarked, that the apparent increase and decrease of every lucid body, proceeds either from its changed distance from the eye of the observer, or from its various site and position in respect of him, by which the angle of vision is changed; or from the increase or diminution of the bulk of the lucid body itself: and having also de-

became a celebrated astronomer and mathematician, and the author of several ingenious books on those sciences; particularly, 1. Philolaus, or a Treatise on the true System of the World; 2. Astronomia Philolaica; 3. A Treatise on Spiral Lines; 4. The two Admonitions noticed in the above Article; 5. His Arithmetic of Infinites, published in 1682, being a diffuse amplification of Dr. Wallis's treatise on the same subject. Bulliald was the inventor of a useful correction of Bishop Ward's approximating hypothesis.

monstrated it impossible that this star should move in a circle, or in an ellipsis; and proved it improbable that it should move in a straight line; he concludes, that there can be no other genuine, or at least no other more probable cause of its emersion and occultation than this, that the larger part of that round body is obscure and inconspicuous to us, and its lesser part lucid, the whole body turning about its own centre and one axe, whereby for one determinate space of time it exhibits its lucid part to the earth, for another, subducts it: it not being likely that fires should be kindled in the body of that star, and that the matter thereof should at certain times take fire and shine, at other times be extinguished on the consumption of that matter.

As to the other star, in the girdle of Andromeda, seen about the beginning of 1665; he relates, that when in the end of 1664 the world beheld the then appearing comet, astronomers observed also this new phenomenon, which was called by them Nebulosa in Cingulo Andromedæ. Concerning which he notes, that the same had been already seen many years before by Simon Marius, viz. An. 1612, when with a telescope he searched for the satellites of Jupiter, and observed their motions. He farther shows that it has formerly appeared, about 150 years ago, and been taken notice of by an expert, though anonymous astronomer; whose words he cites out of a manuscript, brought out of Holland by the excellent Jacobus Augustus Thuanus, returning from his embassy to Paris; wherein also was marked the figure of that phenomenon. From all this he collects, that whereas this star has been seen formerly, and that 150 years since, but yet neither observed by Hipparchus, nor any other of the ancients that we can find; nor also in the former age by Tycho Brahe, nor in our age by Bayerus; and appeared also in the month of November last much lessened and obscure, after it had two years ago shone very bright; that therefore it must needs appear and disappear by turns, like those in the necks of the Whale and Swan.

II. Entriens sur les Vies et sur les Ouvrages des plus excellens Peintres, Anciens et Modernes, par M. Felibien.

Trials proposed by Mr. Boyle to Dr. Lower for the Improvement of transfusing Blood out of one live Animal into another. N° 22, p. 385.

1. Whether by this way of transfusing blood, the disposition of individual animals of the same kind may not be much altered? As whether a fierce dog, by being often quite new stocked with the blood of a cowardly dog, may not become more tame, *et vice versa?*

2. Whether immediately upon unbinding a dog, replenished with adventitious blood, he will know and fawn upon his master, and do the like customary

things as before? And whether he will do such things better or worse at some time after the operation?

3. Whether those dogs that have peculiarities will have them either abolished, or at least much impaired by transfusion of blood? (As whether the blood of a mastiff, being frequently transfused into a blood-hound or a spaniel, will not prejudice them in point of scent?)

4. Whether acquired habits will be destroyed or impaired by this experiment? (As whether a dog taught to fetch and carry, or to dive after ducks, or to set, will after frequent and full recruits of the blood of dogs unfit for those exercises, be as good at them as before?)

5. Whether any considerable change is to be observed in the pulse, urine, and other excrements of the recipient animal, by this operation, or the quantity of his insensible transpiration?

6. Whether the emittent dog being full fed at such a distance of time before the operation, that the mass of blood may be supposed to abound with chyle, the recipient dog being before hungry, will lose his appetite more than if the emittent dog's blood had not been so chylous: and how long, upon a vein opened on a dog, the admitted blood will be found to retain chyle?

7. Whether a dog may be kept alive without eating by the frequent injection of the chyle of another, taken freshly from the receptacle, into the veins of the recipient dog?

8. Whether a dog, that is sick of some disease chiefly imputable to the mass of blood, may be cured by exchanging it for that of a sound dog? and whether a sound dog may receive such diseases from the blood of a sick one, as are not otherwise of an infectious nature?

9. What will be the operation of frequently stocking (which is feasible enough) an old and feeble dog with the blood of young ones, as to liveliness, dulness, drowsiness, squeamishness, &c. *et vice versa?*

10. Whether a small young dog, by being often fresh stocked with the blood of a young dog of a larger kind, will grow bigger than the ordinary size of his own kind?

11. Whether any medicated liquors may be injected together with the blood into the recipient dog? and in case they may, whether there will be any considerable difference found between the separations made on this occasion, and those which would be made, in case such medicated liquors had been injected with some other vehicle, or alone, or taken in at the mouth?

12. Whether a purging medicine being given to the emittent dog a while before the operation, the recipient dog will be thereby purged, and how? (which experiment may be greatly varied.)

13. Whether the operation may be successfully practised, in case the injected blood be that of an animal of another species, as of a calf into a dog, &c. and of a cold animal, as of a fish, or frog, or tortoise, into the vessels of a hot animal, and *vice versa?*

14. Whether the colour of the hair or feathers of the recipient animal, by the frequent repeating of this operation, will be changed into that of the emittent?

15. Whether, by frequently transfusing into the same dog the blood of some animal of another species, something further, and more tending to some degrees of a change of species, may be effected, at least in animals near of kin? (as spaniels and setting dogs, Irish grey-hounds and ordinary grey-hounds, &c.)

16. Whether the transfusion may be practised upon pregnant bitches, at least at certain times of their pregnancy? and what effect it will have upon the whelps? *

A Method of observing Eclipses of the Moon. By Mr. Rook, late Gresham Professor of Geometry. N 22, p. 388.

Eclipses of the moon are observed for two principal ends: one astronomical, that by comparing observations with calculations, the theory of the moon's motion may be perfected, and its tables reformed; the other geographical, that by comparing among themselves the observations of the same ecliptic phases, made in divers places, the differences of meridians or longitudes of those places may be discovered.

The knowledge of the eclipse's quantity and duration, the shadows, curvity, and inclination, &c. conduce only to the former of these ends. The exact time of the beginning, middle, and end of eclipses, as also in total ones the beginning and end of total darkness, is useful for both of them.

But because in observations made by the naked eye, these times considerably differ from those with a telescope; and because the beginning of eclipses and the end of total darkness are scarce to be observed exactly, even with glasses (none being able clearly to distinguish between the true shadow and penumbra, unless he has seen, for some time before, the line separating them pass along upon the surface of the moon): and lastly, because in small partial eclipses, the beginning and end, and in total ones of short continuance in the shadow, the beginning and end of total darkness are unfit for nice observations, by reason of the slow change of appearances, occasioned by the oblique motion of the shadow. For these reasons I shall propound a method peculiarly designed for the accomplishment of the geographical end in observing lunar eclipses, free, as far as is possible, from all such inconveniences.

* To most of these questions a negative answer may be given.

For, first, It will not be practicable without a telescope. Secondly, The observer will always have opportunity, before his principal observation, to note the distinction between the true shadow and the penumbra. And, thirdly, It will be applicable to those seasons of the eclipse, when the alteration in the appearances is most sudden. For which purpose, let a select number of the most eminent spots, dispersed over the moon's surface, be pitched on to be constantly used in all parts of the world; as, for example, those which M. Hevelius calls Mons Sinai, Ætna, Porphyrites, Scrorum; Insula Besbicus, Creta; Palus Mæotis, Maræotis, and Lacus Niger Major.

In each eclipse let, for instance, three of these spots, which then lie nearest the ecliptic, be exactly observed, when they are first touched by the true shadow, and again when they are just completely entered into it; and also in the decrease of the eclipse, when they are first fully clear from the true shadow; for the accurate determinations of which moments of time, let there be taken altitudes of remarkable fixed stars, on this side of the line, of such as lie between the equator and tropic of Cancer, but beyond the line, of such as are situated towards the other tropic; and in all places of such as at the time of observation are about four hours distant from the meridian.

Halos about the Moon. By the Earl of SANDWICH. *N° 22, p.* 390.

December 25, old style, 1666, in the evening there appeared at Madrid a great halo about the moon, the semidiameter whereof was about 23 deg. 30m. Aldebaran was exactly in the north-east part of the circle, and the two horns of Aries were enclosed by the south-west of it, the moon being in the centre. About five or six years ago, viz. November 21, 1661, an hour after sun-set, I saw a great halo about the moon of the same semidiameter, at Tangier, the moon being very near the same place where she was at the other observation.

Toads and Spiders innoxious. By Dr. N. FAIRFAX. *N° 22, p.* 391.

The ingenious author of this letter having taken notice of what was published in Number 9, viz. that creatures reputed venomous are indeed no poisons when swallowed, though they may prove so when put into wounds; he, for confirmation thereof, alleges examples of several persons well known to him, who have frequently swallowed spiders,* even of the rankest kind, without any more harm than happens to hens, red-breasts, and other birds, who make spi-

* It is certain that the generality of common spiders may be swallowed with perfect impunity, the extremely small quantity of the poisonous fluid with which their fangs are provided, being incapable of injuring the larger animals. Several persons have been in the habit of making this idle experiment: among others the famous Anna Maria Schurman is said to have had a propensity of this

ders their daily food. And he mentions some men who ate even toads without receiving any hurt.

The same gentleman relates, that once seeing a spider bruised into a small glass of water, it tinged it somewhat of a sky colour; and he was told that a dozen of them being put in they would dye it almost a full azure. Indeed it seems not more incredible that this creature should yield a sky colour when put in water, than that cochineal, which also is but an insect, should give a fine red when steeped in the same liquor.

He also gives an instance of a boy, who, by bruising a toad, and receiving some of the noxious juice upon his lips, had them swollen to an enormous size, which swelling continued during his life.*

An Account of some Books. N° 22, p. 392.

I. Le Tome troisieme et dernier des Lettres de M. Descartes.†

As the first two tomes of M. Descartes's letters contain questions for the most part of a moral and physiological nature, proposed to and answered by him; so this consists of the contests he had upon several subjects with divers men eminent in his time.

Besides other particulars treated of in this tome, there occur many pretty questions concerning numbers, the cycloid, the manner of working glasses for telescopes, the way of weighing air, and many other curiosities, mathematical and physical.

nature; jocularly excusing herself by saying she was born under the sign Scorpio. Several other instances of a similar nature may be found in the work of Rosel. Mouffet, in his history of insects, relates a story of a profligate quack, who was employed by a rich London matron to cure her of a tympany, which he had the audacity to attempt by giving her several spiders to swallow in the disguise of pills; stipulating that half the proposed reward should be immediately paid, and the remainder when the cure was completed. He then absconded; not doubting that he had poisoned her: but hearing some time afterwards that the lady was perfectly recovered, he immediately waited upon his rich patient, and apologizing for his long absence, received the remainder of his reward, with many praises for the efficacy of his medicine.

* With respect to the noxious quality of the fluid here said to have been received from the toad on the boy's lips, the account seems by far too exaggerated to deserve any credit; a slight temporary swelling being the utmost that can be supposed to have happened in such a case, even from those species of toad which secrete the most acrimonious fluid from their skin, as the *Rana alliacea, mephitica, &c.*

† Rene Descartes, a very celebrated French philosopher and mathematician, was born at la Haye in Turenne, in the year 1596, and died at Stockholm in Sweden (whither he had been invited by queen Christina), in 1650, in the 54th year of his age. Descartes was a man of a fine genius, which he had cultivated by a life spent in intense application to study. The result was a great number of ingenious and learned works, on geometry, dioptrics, philosophy, music, &c. &c. the two

II. Astronomia Reformata, Auctore Johanne Bapt. Riccioli,* Soc. Jesu.

The design of Riccioli in this work, is to examine the different hypotheses of several astronomers, as to the system of the world; in which he finds great difficulty to conclude any thing certain. He expected to reform what he esteemed the errors which some astronomers had fallen into; but unfortunately takes a wrong direction, and becomes a strenuous advocate for the immobility of the earth.

III. Anatome Medullæ Spinalis, et Nervorum inde proventium, Gerardi Blasii, M. D. The Anatomy of the Spinal Marrow, and of the Nerves that arise therefrom. By Dr. Gerard Blasius.†

The author shows in this little tract a way of taking the entire medulla spinalis or marrow of the back out of its theca or boney receptacle, without laceration, which else happens frequently both to the nerves proceeding from it and to the coats investing it; not to name other parts of the same. This he affirms to have been put in practice by himself, by a fine saw and wedge; which are to be dexterously used: and he produces accordingly in excellent cuts the representations of the structure of the said medulla thus taken out, and the nerves thence proceeding; and that of several animals, dogs, swine, sheep.

He intermixes several observations, touching the singleness of this medulla against Lindanus and others; its original, viz. Whether it be the root of the brain, or the brain the root of it: its difference of softness and hardness in several animals; where he notes, that in swine it is much softer than in dogs, &c.

former of these being the most useful and truly scientific of the whole. His taste in the mathematics was genuine and correct, making considerable improvements in algebra, geometry, dioptrics, and in mechanics. But in philosophy he failed, his genius taking a wrong turn through the brilliancy of his imagination, which led him to invent systems of nature, instead of investigating her laws by means of judicious experiments, painfully and patiently pursued.

Descartes's chief work in pure mathematics, was his application of algebra to the geometry of curve lines; making those two sciences mutually subservient to each other, to the improvement of both.

He conducted, during the greatest part of his life, considerable, not to say violent controversies with his contemporary philosophers; most of which are extant in the several volumes of letters which have been published with the collections of his works.

* John Baptist Riccioli, a learned Jesuit, was born at Ferrara in 1598, and died in 1671, at 73 years of age. He was author of several considerable works, as, 1. Almagestum Novum; in which, after the manner of Ptolemy, he collected every thing relating to astronomy, ancient and modern. 2. Astronomia Reformata, above-mentioned. 3. Chronologia Reformata. Riccioli took great pains in collecting information, and employing it to the best purpose. But it would seem that the prejudices of his order had induced him to adopt an erroneous hypothesis as to the system of the world.

† Gerard Blasius was professor of physic at Amsterdam in the latter part of the 17th century. He was author of several Latin tracts on medical, chemical and anatomical subjects; among the most celebrated of which may be numbered his Anatome Animalium.

He exhibits also the arteries, nerves, and veins, dispersed through this medulla, and inquires, Whether the nerves proceed from the medulla itself or its meninx? and discourses also of the principle and distribution of the nerves; referring for ampler information, in this and the other particulars, to that excellent book of the learned Dr. Willis, De Anatome Cerebri.

END OF VOLUME FIRST OF THE ORIGINAL.

Of a considerable Load-stone dug out of the Ground in Devonshire. By Dr. EDW. COTTON. *N° 23, p.* 423. *VOL. II.*

This stone weighs 60 pounds; and though it takes up no great weight, yet it moves a needle about nine feet distant. A part which had been broken off he has sent up also, because when put in its proper place, it adds much strength to it, but without that addition it moves not much more than at seven feet distance.

Remarks on Load-stones and Sea Compasses. By Mr. OLDENBURG. *N° 23, p.* 423.

A noble person, on a late occasion, affirmed that a needle of a sea compass, put in a good iron mine, which yielded 23 pounds of metal out of 120 pounds of ore, was not sensibly moved by it.

Another honourable person desired it might be observed whether touched needles move otherwise, when the veins of iron do not lie north and south, than when they do?

It being inquired from abroad, whether sea compasses in England were brought to greater perfection than in other countries? Answer was made by intelligent persons here, that all the perfection of our sea compasses as yet consisted in this, that the needles be touched by good load-stones and well balanced, and that the variation be truly placed: though it was suggested, that for the greater perfection of such sea compasses, a way was contriving to show the variation to minutes and seconds.

It was also proposed, that it might be inquired into,

1. Whether a needle may be so touched on any magnet, as not to point to the true north and south, to be tried in such places where there is no variation known?

2. Whether different load-stones will give different directions? And whether fainter or stronger touches on one and the same magnet will cause any variation in the directions? For which purpose as many load-stones should be pro-

cured as could be had, and a good number of needles exactly made, of the same metal, largeness, and figure?

Proposals to try the Effects of the Pneumatick Engine [Air-pump] exhausted, on Plants, Seeds, and Eggs of Silkworms. By Dr. BEALE. *N° 23, p.* 424.

Dr. Beale formerly suggested as follows:—

It would be very well worth the trial, to see what effects would be produced on plants put into the pneumatic engine of Mr. Boyle, with the earth about their roots and flourishing; whether they would not suddenly wither if the air were totally taken from them. And particularly to try in the season cherry-blossoms when partly opened, partly not opened upon a branch; to wit, whether the air may be so attenuated as to blast them.

Mr. Boyle suggests, that it may be tried,

1. Whether seeds (especially such as are of a hasty growth, viz. orpin, lettice, garden cress seeds, &c.) will germinate and thrive in the exhausted receiver of the said engine?

2. Whether the exclusion of air from the sensitive plant would be hurtful to it?

3. Whether the grafting of pears upon *spina cervina* will produce the effect of communicating to the fruit its purging quality or not?

4. Whether silkworms' eggs will be hatched in such an exhausted receiver, in the season proper for hatching?

The experiment heretofore made of this kind, was, that some lettice-seed being sown upon some earth in the open air, and some of the same seed at the same time upon other earth in a glass receiver of the above-mentioned engine, afterwards exhausted of air; the seed exposed to the air was grown up an inch and a half high within eight days; but that in the exhausted receiver not at all. And air being again admitted into the said emptied receiver, to see whether any of the seed would then come up; it was found that in the space of one week it was grown up to the height of two or three inches.

Observations on Ants. By Dr. EDMUND KING. *N° 23, p.* 425.

1. There have occurred to my observation but three sorts of ants, commonly without wings; viz. very black, dark brown, and the third sort of nearly the colour usually called feuillemort.

2. Each kind have distinct habitations in their several banks, two sorts seldom or never being found together; and if either of the other two sorts be put into the black ants' bank, it is worth observing what enmity there is

between these little creatures, and with what violence the black ones will seize on the red, pinching them on the head with forceps or claws, till they have killed them, which done, they will carry them out of the field from their bank. But if you put black ants into a bank of the red, the black seem to be so sensible of the strangeness of the place they are in, that there they will not meddle with the red, but as if they were frightened, and concerned for nothing but self-preservation, run away.

3. Upon opening these banks, I observe first a white substance, which to the bare eye looks like the scatterings of fine white sugar or salt, but very soft and tender; and if you take a bit of it, as big perhaps as a mustard seed, and lay it on the object plate of a good microscope, you may, by opening it with the point of a needle, discern many pure white and clear appearances in distinct membranes, all figured like the lesser sort of birds' eggs, and as clear as a fish's bladder. This same substance I find in the ants themselves, which I take to be the true ants' eggs; it being obvious that wherever this is uncovered, they make it their business to carry it away in their mouths to secure it, and will, after you have scattered it, lay it on a heap again with what speed they can.

4. I observe they lie in multitudes upon this spawn; and after a little time, every one of these small adherents is turned into a little vermicle, as small as a mite, hardly discerned to stir; but after a few days more you may perceive a feeble motion of flexion and extension, and they begin to look yellowish and hairy, shaped very like a small maggot; and so keeping that shape grow almost as large as an ant, and have every one a black spot on them.

5. Then they get a film over them, whitish and of an oval shape, for which reason I suppose they are commonly called ants' eggs, which yet, properly speaking, they are not. [These are the chrysalids.]

6. I have, to prevent mistakes, opened many of these vulgarly called ants' eggs, I mean the lesser sort (for there are some as big as a wheat corn, others less than a rye corn) and in some I find only a maggot, to appearance just such as was described before: in others I find a maggot beginning to put on the shape of an ant about the head, with two little yellowish specks where the eyes are designed: in others a further progress, and furnished with every thing to complete the shape of an ant, but wholly transparent, the eyes only excepted, which are then as black as black bugles.

7. But when they have newly put on this shape, I could never discern the least motion in any part of the little creature, the reason of which may perhaps be the weakness of their fibres; for after a little more time, when they begin to be brownish, they have strength to stir all their parts.

8. At last I met with some of these reputed eggs, which having carefully

opened, I took out of several of them every way perfect and complete ants, which immediately crept about among the rest, no way differing from many other ants, but by a more feeble motion of their limbs. And this I took for a clear demonstration of what I wished to know, that the film covers the maggot only while she is transforming into an ant, and till fit to shift for herself.

9. The black speck that is at one end of every such reputed ant's egg, I suppose to be cast out of the maggot in her transformation; since after it puts on the shape of an ant the speck is quite gone, and the whole body of the ant clear; since also this speck at the end of the said egg lies always close to the anus of the inclosed ant.

10. As to their care for their young (by which I mean all the sorts and degrees aforesaid, from the spawn to the vulgarly called eggs, in every one of which you will find a young ant) it is observable, how upon the breaking up of their banks they make it their business immediately to carry their young out of sight again, laying the several sorts of them in several places and heaps; which if you mingle again or scatter, you shall, laying but some bits of slate or the like in any place they may come to and get under, after a few hours see all the vermicles and vulgarly called eggs laid in their several and distinst parcels under such pieces of slate, &c. provided the place be not so cold as to chill their limbs; which if it be, by being brought to the fire they will soon recover their strength, and fall to their business again of securing their little ones.

11. I have observed in summer, that in the morning they bring up those of their young (which are vulgarly called ants' eggs) towards the top of the bank; so that you may from ten in the morning until five or six in the afternoon find them near the top; especially about one, two, or three o'clock and later, if the weather be hot, when for the most part they are found on the south side of the bank, so that towards seven or eight at night, if it be cool or likely to rain, you may dig a foot deep before you can find them.

They know all the sorts of their young so well, that you cannot deceive them; though you may with fine sugar, salt, or the crums of very white stale bread scattered in the mould where their first true eggs are (as I call them) be mistaken yourself, yet the ants will not, nor touch a bit of what is not their own offspring.

An Account of a French Book, entitled, Histoire des Joyaux, et des Principales Richesses de l'Orient et de l'Occident, par le Sig. CHAPUZEAU. *N° 23, p. 429.*

This history treats of diamonds, rubies, emeralds, pearls, coral, bezoar, yellow amber, ambergris, indigo, &c.

The Directions for Seamen explained. *N° 24, p.* 435.

The particulars themselves follow:

1. To observe the variation of the compass or needle.

At land, where by the help of good fixed dials and other fit instruments, the precise meridian of the place may be known, it is easy to find the variation of the needle divers ways: As by applying the needle, &c. to the shadow of a thread hanging perpendicular, when the sun is in the meridian; or to the meridian line or to the side of a fixed horizontal dial, &c.

But at sea, as the meridian is not so easy to be found to any tolerable exactness, to know the variation of the needle is much more laborious and difficult. The height of the pole and the sun's declination being known, a large ring-dial, having a compass or needle fixed to its meridian below, may go near to show the variation required. For when it is set to the exact hour and minute of the day, its meridian stands just in its due place; and so shows how far the needle varies from it. But because these dials are rarely just, they are not to be relied on. The following method may therefore be employed. Find the sun's azimuthal distance from the meridian, some hours before or after noon; and then its magnetical azimuth or distance from the meridian pointed at by the needle there, the difference of these two distances is the variation of the needle.

To find the sun's true azimuth, or by how many degrees, &c. of the horizon it is distant from the meridian: its declination, its altitude, and the elevation of the pole must all three be known.

To do which accurately, constitute a spherical oblique angled triangle of the three complements of the sun's declination of his altitude, and of the height of the pole; the measures of all the sides whereof are known; one from the zenith to the pole; another from the pole to the point of the sun's altitude; and the third from that point to the zenith. From these find the angle at the zenith, which subtract from 180, and the remainder is the sun's true azimuth or distance from the meridian of the place.

The true azimuth of the sun being thus found, and the magnetical azimuth, according to your needle observed, subtract the less number from the greater, and the remainder is the variation of the needle. If the magnetical azimuth be less than the other, then the variation is on the same side of the meridian with the sun; if greater, on the other side.

To find this variation by the stars is no more requisite than to find out the true north, that is the meridian, and compare the needle's position with it. By this means the variation may be had well enough to degrees, half degrees, and some smaller parts; and if carefully and curiously prosecuted, even to minutes too. But it will not be amiss to do it both by the sun and stars, for the greater certainty.

2. The dipping needle is to be used at least as often as the former experiment is made. All that need be said of the manner is, that when the dipping of the needle is to be examined, the circle in which it moves is to be hung perpendicular, and turned till it be just in the magnetical meridian, where it dips the most, and the degree of its depression under the horizon is to be noted in a table. See fig. 2, pl. 4.

3. The chief particulars of the tides to be regarded are, the precise times of the beginnings of the flood and ebb: which way currents run in all places, with their times, changes, &c. The perpendicular heights and depths of the tide, and lowest of the ebb: what day of the moon's age, and what times of the year, the highest and lowest tides fall out.

4. To sound the depth of the sea without a line.

To perform this take a globe of fir or maple, or other light wood, as fig. 3, pl. 4, let it be well secured by varnish, pitch, or otherwise, from imbibing water; then take a piece of lead or stone D, considerably heavier than will sink the globe: Let there be a long wire staple B, in the ball A, and a springing wire C, with a bended end F, and into the said staple press, with your fingers, the springing wire on the bended end, and on it hang the weight D, by its hook E; and so let globe and all sink gently into the water, in the posture represented in the said figure, to the bottom, where the weight D touching first is thereby stopped; but the ball, by the impetus it acquired in descending, being carried downwards a little after the weight is stopped, suffers the springing wire to fly back, and thereby sets itself at liberty to re-ascend. Then, by observing the time of the ball's stay under water, either by a watch having minutes and seconds, or by a good minute-glass, or best of all by a pendulum vibrating seconds, with the help of some tables, the depth of the sea will be known.

In some of the trials already made with this instrument, the globe being of maple wood, well covered with pitch to prevent its saturation, was $5\frac{13}{16}$ inches in diameter, and weighed $2\frac{1}{2}$ pounds; the lead of $4\frac{1}{2}$ pounds weight, was of a conical, but is now used of a globular figure, 11 inches long, with the sharper end downwards, $1\frac{9}{16}$ in diameter at the bottom. And in those experiments made in the Thames, in the depth of 19 feet water, there passed between the immersion and emersion of the globe six seconds; and in the depth of ten feet water, there passed $3\frac{1}{2}$ seconds. From many of such experiments it will not be difficult to find out a method to calculate what depth is to be concluded from any time of the globe's stay under water: As for instance, if in the depth of 20 fathoms measured by the line, the globe stay under water 15 seconds; then if the ball stay 700 seconds, the depth of the sea is 933 fathoms and two feet, if the ball be found to move equal spaces in equal time.

In the same trials made with this instrument in the Thames, it has been found that there was no difference in time between the submersions of the ball at the greatest depth, when it rose several yards from the place where it was let fall, being carried by the current of the tide, and when it rose only a yard or so from the same place where it was let down.

And if it be alleged that it must be known, when a light body ascends from the bottom of the water to the top, in what proportion of time it rises; it may be considered that in this experiment the times of the descent and ascent are both taken and computed together; so that for this purpose there needs not the nicety which is alleged.

Of other experiments of this way of sounding without a line, made by the noble Lord Viscount Brounker, Sir Robert Moray, Knight, and Mr. Hook, in the channel at Sheerness, the following account was given, viz.

	Weighed Oz.	Gr.
A wooden ball (A)	$32\frac{9}{16}$	0
Another wooden ball (B)........	30	22
A lead (A)	30	0
Another lead (B)..............	$30\frac{3}{4}$	0

The ball (B) and the lead (B) were let down at 16 fathoms; and the ball returned in 48 single strokes of a pendulum, held in the hand, vibrating 58 single strokes in a minute.

A second time repeated with the same success; therefore the motion was four feet every second.

Again the ball (A) and the lead (B), the nail of which was bent into a sharper angle, the ball returned in 39 strokes. A second time repeated with the same success, at the same depth.

Ball (B), lead (B), in which trial the line, not being clear, stopped a little the motion, the ball returned in 47 at the same depth.

Ball (A) lead (A) at eight fathoms and one foot, returned at 20.

Repeated at eight fathoms, returned at 19.

Tried the third time at ten fathoms and four feet, returned at 28.

A fourth trial, at the same depth, just the same.

A fifth at ten fathoms five feet, returned in 27.

A sixth trial, just the same.

A seventh at twelve fathoms five feet, in 37.

An eighth trial, just the same.

Another day, near the same place, when the pendulum was adjusted, and made a little shorter, there having been but 58 vibrations in a minute the former day.

Ball (A) lead (B) at 14 fathoms, returned in $32\frac{1}{2}$.

A second trial a little after in the same place returned in 33. In making of which trial the vibrations were told aloud, and the lead having been let down by a line, was found to touch the bottom in just half the time, the ball staid under water. By a second trial the ascending and descending were found to be in equal times. And by a third trial with another lead, the very same was found, viz. $16\frac{1}{2}$ descending, and $16\frac{1}{2}$ ascending. This lead and ball let down without a line, the ball returned in 13 vibrations; a sign it went not to the bottom.

A trial made with a lead, the iron crook being fastened at the top of it, like that in fig. 4, succeeded very well, and the ball returned in $34\frac{1}{2}$. But by reason of the current, the experimenters could not perceive when the lead touched the bottom. This lead being let down without a line, the ball returned in $32\frac{1}{2}$. The depth of the water was now found by the ship's lead to be 14 fathoms.

Another trial was made with a line, bowing the point of the lead, like that in fig. 5, and the ball returned in 34. The same let down without a line, the ball returned in 6 or 7 vibrations; a sign again, it went not to the bottom.

In a trial with another lead, the ball returned in 34.

Repeated again with the same success.

In a trial with a lead having the nail set awry, like that of fig. 6, the ball returned in 34. After which trial, the depth was found to be just 14 fathoms.

The last lead and ball being let down without a line, the ball returned at 35.

In another trial with a lead that never failed, the ball returned in 34, and the lead touched the bottom at 17.

By a trial with another lead, the same time was found exactly.

By a third trial with this last, the very same.

These trials were made near about high-water, at the depth of 14 fathoms exactly: and in them the motions seem to be 5 feet every second.

In all these trials, the greatest difficulty was in the use of conical figures, with iron crooks, to bend the iron that it might be sure to carry down the ball with it to the bottom, and when come thither to let it go: for almost every one of these leads failed in one of these requisites, till by several trials they had been adjusted.

It is not to be omitted, that the last trials being made near high water, the ball was found to rise (by the boat being permitted to drive) far off upon one side out of the way, that any light thing suffered to swim on the water would be carried; which seemed to argue a motion of the under parts of the water, differing from that of the upper; a thing which is said to be at certain times of the tides, both at the mouth of the Sound and of the Straits; which deserves to be further inquired into. The angle made by these different motions seemed to be about 40 degrees.

5. The strength of the winds is measured by an instrument, such as is represented by fig. 7; which being exposed to the wind so as the flat side may be right against it, the number of degrees on the limb A B, to which the wind blows up, or raises that flat side C D, shows the force or strength of the wind, in proportion to the resistance of the flat side of the instrument.

6. The glass phial for measuring the different gravities of salt water is to be made with a very narrow neck, and when it is almost full water is to be dropped into it, drop by drop, till it can hold no more, drying well the phial before it be weighed, having taken first the weight of the empty phial. Then by evaporating gently the water till the salt be left dry on the bottom, the proportion the salt of each water holds to its weight may be known.

Mr. Boyle describes a glass tube, as is represented by fig. 8, blown at a lamp, and poised in good common water, by putting quicksilver into it, till it sink so low that nothing but the top appears above the water; which done, it is to be sealed up and graduated on its side into what parts you please; which may be done with a diamond. And then being put into any water to be weighed, it will by its sinking more or less show the difference of the water's gravity.

7. To fetch up water from any depth of the sea.

Let there be made a square wooden bucket, fig. 9, whose bottoms E E are to be so contrived, that as the weight A sinks the iron B, (to which the bucket C is fastened by two handles D D, on the ends of which are the moveable bottoms or valves E E,) and thereby draws down the bucket, the resistance of the water keeps up the bucket in the position C, by which the water has a clear thorough passage all the while it is descending; but as soon as the bucket is pulled upwards by the cord F, the resistance of the water to that motion beats the bucket downward, and keeps it in the position G; whereby the included water is kept from going out, and the ambient water from getting in.

By the vessel we may know the degrees of saltness of sea water, according to its nearness to the top or bottom; or rather the nature of the sea water in several depths of several climates: And whether in some places of the sea there be any sweet water at the bottom; as is affirmed in the East Indian voyages of Van Linschoten, viz. that in the Persian gulph, about the isle of Baharem, sweet water is fetched up from the depth of four or five fathoms.

An Account of an easier and safer Way of Transfusing Blood out of one Animal into another, viz. by the Veins, without opening any Artery of either. By Dr. EDMUND KING. *N° 25, p.* 449.

1. I took a calf and a sheep, both of the larger sort, and having prepared a jugular vein in each, I planted my pipes and quills as is usual, both in the jugular vein of the calf designed to be the emittent, and in that of the sheep intended for the recipient. Then I took out of the sheep 49 ounces avoirdupois of blood, before any other blood was let in; about which time the company concluding the sheep to be very faint, and finding the blood to run very slowly, I stopped the vein of the sheep, and unstopped the pipe in the calf, letting run out 10 ounces into a porringer, which was done in about 40 seconds. Then I conveyed pipes from the emittent calf's vein into the recipient sheep's vein, and there ran a good free stream of blood for the space of five minutes, though perhaps less swift than the first ten ounces. And not to be deceived in the running, I often struck with my finger the upper part of the emitting vein, and thereby easily felt every stroke answered on the recipient vein, just like a pulse. And now supposing that by this time (the lapse of five minutes) the sheep had received as much if not more blood than it had lost, we stopped the current of blood from the calf, and closed also the vein of the sheep; and then having untied her and set her down in the room, she went about, and appeared to have as much strength as she had before the loss of her own blood. Then resolving to bleed the sheep to death, we bound her the second time, and opened the emittent part of the vein again; whereupon having bled about 60 ounces, she fell into convulsions; and after the loss of about five ounces more, she died upon the spot: and being dressed by the butcher, there did not in all the usual places appear above three ounces of blood; and the whole sheep looked of a lovely white; and the meat of it (to the taste of those that eat of it) was very sweet.

The sheep being dead, we resolved likewise to see the calf bleed to death; but he having bled ten ounces, and then for the space of five minutes more into the sheep, and rested a good while, the blood by that time began to coagulate in the vein; which made me open the carotid artery, letting thence run out about 25 ounces of blood of a very vivid colour, vastly excelling therein the blood of the vein. The calf when dressed had by the information of the butcher as little blood as the sheep; and we saw him look whiter than they usually do in the ordinary way of killing.

2. I took more than 45 ounces of blood out of the jugular vein of a sheep,

of a less size than the former; by which time the spectators as well as myself found her exceeding faint, and some thought her past recovery, without a supply of blood. Then I conveyed blood from the jugular vein of a calf into that of the sheep, for the space of seven minutes, when we did believe by the continuance of a good stream from the calf, that the sheep had already received more blood than she had lost. Whereupon we set her free, and she had no sooner got her liberty, but seeing a dog near her, a spaniel that had formerly suffered the transmission of sheep's blood into him, she butted with great violence at him three or four times, not appearing at all concerned at what she had endured in the experiment. We keep this sheep alive, she being sent to grass again, and seeming hitherto very strong and lusty.

The calf was much larger than the sheep. We bled the calf to death, and received from him six porringers full of blood after the sheep had been supplied, each porringer containing $11\frac{1}{4}$ ounces of water. The sheep lost four of the same measures full of blood; which being supplied by that of the calf, we reckon that the calf lost ten such measures in all.

An Account of another Experiment of Transfusion, in Bleeding a Mangy into a Sound Dog. By Mr. Thomas Coxe. *N° 25, p.* 451.

The object of this experiment was to ascertain whether the sound dog would become infected by having the blood of the mangy dog transfused into his veins. The result was, that no alteration whatever was produced in the sound dog by this operation; but in about ten days or a fortnight the mangy dog was perfectly cured, in consequence, it is supposed, of the quantity of blood which he lost on this occasion.

Extract of a Letter from M. Denis, *Professor of Philosophy and Mathematics, touching the Transfusion of Blood, dated April 2,* 1667. *N° 25, p.* 453.

Since the experiments, of which I wrote to you the 9th of March, we have transfused the blood of three calves into three dogs, to assure ourselves what the mixture of two such different sorts of blood might produce. I shall hereafter acquaint you at large with the particulars; at present I shall only inform you, that the animals into which the blood had been transmitted do all of them eat as well as before, and that one of these three dogs, from whom the day before so much blood had been drawn that he could hardly stir any more, having been supplied the next morning with the blood of a calf, recovered instantly his strength, and showed a surprising vigour.

We have found new ways of making this transfusion with so much facility, that M. Emmerez undertakes to perform it without any ligature, only by puncture, like that which is used in letting of blood.

The re-uniting of the separated Bark of Trees. By Dr. MERRET. *N° 25, p.* 453.

In the middle of March I made a section of the rinds of ash, and of the tree falsely called sycamore. The first section of each of the rinds was square, whereof three sides were cut, the fourth uncut. The success was, that the whole bark united by binding it with pack-thread, leaving a scar in each of the cut sides.

Then I cut off, and separated entirely from the tree, several parts of the bark, some shallower, leaving part of the bark on; others to the very wood itself, both in the trunk and branches; from an inch square to less dimensions; and some of them I bound close with pack-thread: all which were separated, a new rind succeeding in their place. Some I covered over beyond the place of incision with diachylon plaster, and tied them fast with pack-thread. All which, thus bound and plastered, in the space of three weeks firmly united to the tree.

But trying the same about Michaelmas and in the winter season, at neither of these times could any union be made of the bark to the tree. I suppose it was because the sap mounted not so vigorously and in such plenty as in the spring season.

To recover Cherries almost withered. By Dr. MERRET. *N° 25, p.* 455.

Anno 1665, I made the following experiment with three May cherry-trees, planted in a rich mould, which lay to a south wall, shaded from the sun by a high building, till the beginning of March, when being high, and shining strongly on them, the fruit constantly withered for some years before. But this year, the season being very hot and dry, I bared the roots of one of them by making a hole about it, and watered it every morning and evening with about a gallon of water, for about a fortnight before the cherries came to redness; and the fruit was full and good. The other two trees, left without this ordering, had most of their fruit withered, having only skin and stones. Now to try this experiment farther, I made a hole round about one of the other trees, and fed it with water daily, as the former; in a week's time those that were quite withered fell off, and the rest that were not so, grew and increased exceedingly: the other tree, that was not used after this manner, had not any of its fruit come to perfection.

The American Aloe, with indented Leaves. By Dr. Merret. *N° 25, p.* 455.

August 4, 1656, this aloe weighed 21 ounces 6 drams 2 grains. Its colour was of a pale green, consisting of 11 leaves; it was bound about with a red dry cloth, and was hung up without oil, as is usual, in the kitchen.

In a year it lost 2 ounces 3 drams 24 grains. The succeeding year, being drier and hotter, it lost 3 ounces $2\frac{1}{2}$ scruples; and more than double in the six colder than the six hotter months. I kept it about five years, and it decreased in nearly the same proportion. In the year 1660, hanging it in a colder garret, it perished.

I observed that every year two of the greater leaves first changed colour then withered, and in the spring there succeeded two fresh and green ones, but never amounting to the size of any of the preceding; so that I had always the same number of leaves. These new leaves were more fresh and green, and not serrated, and were thicker also in proportion to their other dimensions. From the growth of these latter leaves perhaps it may be inferred that there is a circulation of the nutritious juice in this plant. For how is it possible that the roots, continuing as firm and solid as at first, should supply so much nourishment as to procreate new leaves, unless it were from the return of the said juice from the old and decaying leaves into the root, and there protruded for the production of new ones?

On Mr. Gascoigne's *Micrometer. By Mr.* Rd. Townley. *N° 25, p.* 457.

Observing in No. 21 of the Phil. Trans. how much M. Auzout esteems his invention of dividing a foot into near 30,000 parts, and thence taking angles to very great exactness; I think it right to inform the world that I have found, by some scattered papers of one Mr. Gascoigne, that, before the late civil wars, he had not only devised an instrument of as great a power as M. Auzout's, but had also for some years made use of it, not only for taking the diameters of the planets and distances upon land, but had farther endeavoured to gather many certainties in the heavens; amongst which I shall only mention one, viz. The finding the moon's distance, from two observations of her horizontal and meridional diameters: which I the rather mention, because the French astronomer esteems himself the first who in that way undertook to settle the moon's parallax. The very instrument he first made I have now by me, and two others more perfected by him; which doubtless he would have greatly improved, had he not been unfortunately slain in his late majesty's service. He had a treatise on optics ready for the press, but though I have used my utmost endeavour to retrieve it, it has not been with success. The instrument is small, not exceeding in weight, nor much in bigness, an ordinary pocket watch, exactly marking above 40,000 divisions in a foot, by the help of two indexes; the one showing hundreds of divisions, the other divisions of the hundred; every last division in my small one containing $\frac{1}{10}$ of an inch; and that so precisely, that as I use it there goes above $2\frac{1}{2}$ divisions to a second. Yet I have taken land angles several times to one division, though it be very hard to come to that exactness in the heavens, because of the swift motion of the planets. Yet to remedy that, I have devised a rest, easy to be made and used.

Mr. Hook also announces (p. 459), that he has two or three ways of measuring the diameters of the planets to the exactness of a second, by the help of a telescope; and of taking the position and distances of the fixed stars from each other, when this does not exceed two or three degrees.

Of the Nebulosa in the Girdle of Andromeda; and the extraordinary Star in the Neck of the Whale. By M. Bulliald. N° 25, p. 459.

January 1667, the nebulosa in Andromeda's girdle, which may be seen by the bare eye, appeared much more obscure than the year before. In the months of February and March I did not see it.

January 20, 1667, at night, 6 h. 30 m. the sky being pretty serene, the star in the neck of the Whale approached to the size of a star of the sixth magnitude, and grew afterwards larger. For, Feb. 12, 6 h. 30 m. it equalled a star of the fourth magnitude at least. And Feb. 24, 7 h. was equal to one of the third magnitude, shining very bright.

Feb. 26 and 27 it appeared still to increase.

M. Hevelius also observes concerning this star; viz. Jan. 23, I found a little star of the sixth or seventh magnitude, about the same place where the said new star used to appear. But it then seemed to me not the genuine new star, but another preceding the new; the longitude of which has been defined by me in Mercurius in Sole visus, Aries 25° 43′ 3″, and the latitude 14° 41′ 32″, south. Anno 1660, Feb. 2, it appeared very bright, and when the moon shone, of the size of that in the mouth of the Whale; from which time I always observed it to grow larger. March 13, I still found it extremely bright, but could not accurately determine its magnitude by my naked eye, because of the vivid crepuscle, and the lowness of the star.

Extract of a Letter from M. Pecquet, concerning a new Discovery of the Communication of the Ductus Thoracicus with the Emulgent Vein. N° 25, p. 461.*

This letter gives an account of a supposed discovery of a communication between the thoracic duct and the emulgent vein of the left kidney. The subject was a woman who had died some days after she had been brought to bed. M. Gayant (who assisted M. Pecquet in this dissection) having discovered the ductus thoracicus upon the 7th and 8th of the vertebræ descending from the back, inserted a quill into the said duct, and having tied it upon the quill, he blew into it: whereupon the duct was filled with wind from the quill unto the subclavian vein. This wind issued at the ascending cava, which had been cut, when he, to whom the corpse belonged, had lifted up the heart to make the demonstration of it. M. Gayant would tie this cava, but it was cut so short, that the ligature could not hinder the wind from issuing out of it; which was the cause that it could not be thrust as far as the breasts. I would supply this defect by compressing with my finger that place of the vein at which the wind came out (which was at about the third vertebra descending from the back) and M. Gayant having blown afresh into it, I compressed with my fingers the vena cava and the ductus thoracicus together; but the wind that was thrust into this channel showed us that it had another way to escape. And indeed we saw as often as we did blow, that the emulgent vein on the left side was filled with wind, and that thereupon the body of the vena cava also filled itself from the emulgent unto the iliacs. This wind seemed to us to come from the left kidney, and to insinuate itself into the emulgent vein, and thence into the cava. The right kidney had been removed, so that we could say nothing of its communication with the said duct.

The question was made, whether the wind that seemed to enter into the emulgent and the cava did enter there indeed; or, whether it did not slide between the proper coat of this vein and that common one which comes to it

* This celebrated French anatomist was born at Dieppe, and flourished about the middle of the 17th century. In 1651 he discovered the duct which conveys the chyle to the subclavian vein, and which has been called, after his name, Receptaculum Chyli Pecquetianum. This duct he traced and described not only in the human body but in brute subjects also. See his Experimenta nova Anatomica, of which the best edition is that which was printed at Paris, 4to. 1654. He died in 1674, having shortened his days by the abuse of spirituous liquors, which, by a strange infatuation, he believed to be extremely nourishing. His discoveries involved him in a controversy with Riolan and other contemporary anatomists, whom, however, he very successfully refuted in his Dissertatio de Thoracicis Lacteis, published in 1661.

from the peritoneum? This question obliged us to slit the cava at the place of the emulgent; and then blowing into the ductus thoracicus, we saw that the wind which had swelled the emulgent did escape at the opening just now made in the cava.

This experiment made us judge there was a communication of the ductus thoracicus with the left kidney, or at least with the emulgent vein, in the body of this woman.*

A Description of several Kinds of Granaries, as those of London, of Dantzick, and in Muscovy. N° 25, p. 464.

These granaries were at the Bridgehouse, Southwark, where by frequent turning, airing and drying, corn has sometimes been preserved for 32 years.

Dr. Pell mentioned, at a meeting of the Royal Society, that they keep corn at Zurich in Helvetia 80 years.

As for the granaries of Dantzick and Moscovy, some observing merchants and travellers give this short account of them.

First, That those of Dantzick are generally seven stories high, some nine stories; having each of them a funnel, to let the corn run down from one floor to another; thereby chiefly saving the labour and charges of carrying it down. And then that those in that town are quite surrounded with water, whereby the ships have the conveniency of lying close to them, to take in their lading. No houses suffered to be built near them, to be thereby secured from the casualties of fire.

Secondly, That those of Muscovy are made under ground, by digging a deep pit of almost the figure of a sugar loaf, broad below and narrow at the top; the sides well plastered round about, and the top very closely covered with stone. The people of that country are so very careful to have the corn well dried, before they put it into those subterraneous granaries, that when the weather of that northern climate serves not to dry it sufficiently, they heat their barns by means of great ovens, and thereby very well drying their corn, supply the deficiency of their short summer.

Inquiries for Hungary, Transylvania, Egypt, and Guinea. N° 25, p. 467, &c.

These inquiries are too long and not sufficiently interesting to be inserted.

* It is unnecessary to notice the other experiments made by M. Pecquet upon the body of this woman, for the purpose of proving the above stated communication of the thoracic duct with the emulgent vein; since, as Dr. Needham has remarked in a subsequent volume of the Transactions, (Vol. vii. No. 85) this supposed discovery was merely a *lusus naturæ*.

Experiments for Improving the Art of Gunnery. By Sir ROBERT MORAY. *N° 26, p. 473.*

I. To know how far a gun shoots point blank, that is, near the level of the cylinder of the piece. On a fit platform, place and point the gun at a mark, as large as the bullet, at 50, 60, or more yards distant, so as the under side of the mark may be in the same level or line with the under side of the cylinder of the piece. Then between the gun and the mark, at several distances, place pieces of canvas, sheets of paper pasted together, or the like, on stakes fixed in the ground, so that the under side being level with the horizon, may just touch the visual line passing from the eye to the upper side of the mark, when the eye is in the line drawn from it to the upper side of the cylinder of the gun; the canvas being so broad and long that if the bullet pass through it two or three feet higher than the level of the mark or of either hand, the hole it makes may show how much it flies higher than the level of that place. If the bullet falls lower than the mark and touch not the canvas, the gun may be next time raised a little, and so on till the bullet hit the mark, or as high as it: And if at first it fall as high as the mark and cut the canvas, the mark and canvas may be brought nearer the gun: Afterwards the mark may be removed to greater and greater distances, till, to hit the mark, it fly higher than some or all the interposed canvases: And thus the experiment is to be repeated and varied at pleasure.

II. To know what quantity of powder is the just charge for any piece, so as it makes the farthest shot, and the powder all fires.

1. Elevate the gun to a mean random, as of 20 or 25 degrees, and fire it with the ordinary charge of powder, in some convenient ground where the fall of the bullet may be easily seen, and measure the distance to the hole made by the bullet. 2. Then, instead of a full charge of powder used in the first shot, take $\frac{1}{10}$ part less, or some such proportion, for the next trial. 3. For a third, fourth, or more trials, diminish still the quantity of powder by $\frac{1}{10}$ at a time, till the range be considerably shorter than at first. 4. Then take $\frac{1}{10}$ more than the first charge, and do all things else as before, and so continue more trials, increasing always the quantity of powder in the same proportion every new trial, till you find the increase of the charge does not make the piece carry further. 5. The right charge being found, the greatest random is to be sought by trying all randoms, elevating the gun more and more, by degrees at a time.

III. To know what gun shoots farthest:

1, A gun to be prepared of much more than the ordinary length, and to be placed as in the former experiments, and charged with the ordinary charge of powder, or rather with that quantity which by the former experiments shall be found the best; and being discharged, the fall of the bullet is to be marked, and the distance measured as has been before suggested.

2. Then try her with less and more powder as before.

3. Then cut off two inches of the muzzle with a saw, and try as before, doing every thing in the same manner: And so cut off still for new trials, till the shot begin to fall shorter than before.

4. The same may done with guns of different bores. And every experiment to be repeated three or four times, in order always to take a medium among them.*

Magnetical Experiments. By Mr. SELLERS. *N° 26, p.* 478.

Mr. Sellers states, that he had often made trial with many needles, touching them on each hemisphere of the stone, in all variety of ways he could imagine, to find if it were possible, by that means, to cause any of these needles to vary in its direction: but that he always found the contrary, all of them conforming to the magnetical meridian, and standing north and south, as other needles that were touched on the very pole of the stone. He adds, that some of these experiments he tried in London, when there was no variation known.

That on frequent trials of touching needles with different load-stones of several magnitudes, as also of different virtue; the needles touched gave all of them the same directions. This he thinks is confirmed by all the needles and sea compasses made in several parts of the world, and consequently touched on several stones of different countries, yet all agreeing in this magnetical harmony, that they all give the same directions. That having sometimes drawn a needle only over the pole of the stone, within the sphere of its virtue, without touching the stone, it has received the same directive quality from the stone as if it had been really touched on the stone itself, though not altogether so strong as if it had touched the stone. Again, that having touched needles on the stone with faint strokes, and other needles with stronger, all these needles received the same effect from the stone, both for strength and direction; he conceiving that it is not the fainter or stronger touches on the stone, nor the multiplicity of strokes, that varies the needle's strength or direction, but that the nature of the steel whereof the needle is made, and the temper that is given thereunto,

* A much better way of making all such gunnery experiments, is by means of the ballistic pendulum, invented by Mr. Robins; of which an account will be given in the proper place in these Transactions.

cause different effects as to the strength it receives from the stone; himself having tried all sorts of steel that he could possibly procure, and all the different tempers he could imagine, for the most powerful receiving and retaining the virtue from the load-stone; he also affirms that he has fully satisfied himself that he can infuse such virtue into a piece of steel, that it shall take up a piece of iron of two ounces weight or more; and give also to a needle the virtue of conforming to the magnetical meridian, without the help of a load-stone or any thing else that has received virtue therefrom.*

Extract of a Letter from Paris, containing an Account of some Effects of the Transfusion of Blood; and of two Monstrous Births. Anonymous. N° 26, p. 479.

The blood of a young dog being transfused into the veins of one that was almost blind from age, and could scarcely stir, the latter was observed two hours after the operation to leap and frisk about.

Of the two monsters mentioned in this letter, one resembled an ape, having all over its shoulders, almost to its middle, a mass of flesh, that came from the hind part of its head, and hung down in the form of a little cloak. The report was, that the woman that brought it forth had seen on a stage when she was five months gone with child, an ape so cloathed. The aforesaid mass of flesh was divided into four parts, corresponding to the coat the ape had on. This phenomenon was ascribed to the power of imagination. The other monster was a fœtus come to maturity, having, instead of a head and brain, a mass of flesh like a liver. It lived four days. There came a letter from Florence, written by Steno, stating that a tortoise was found to move its foot three days after its head had been cut off.

An Account of two Monstrous Births, not long since produced in Devonshire. By M. Colepresse. N° 26, p. 480.

One of these monsters was a lamb with one head, but two distinct bodies, and eight legs; the bodies were joined in the neck. It had two eyes and as many ears, in the usual places, and one extraordinary eye in the niddock, with a single ear about an inch behind the eye. The other monster was a lamb with two distinct heads and necks joined at the shoulders, but only one body, and that well formed, yet having double entrails throughout.

* This seems to be the first notice of making artificial magnets, viz. by this Mr. Sellers, who was probably the same person as the author of the Practical Navigation, first printed in 1669.

Some Observations made both in Mines and at Sea. N° 26, p. 481.

Mr. Colepress relates, that discoursing with one John Gill, a man well experienced in mineral affairs, he affirmed, that if in digging deep under ground, the work-men meet with water, they never want air or wind; but if they miss water, as sometimes it happens, even at 12 or 16 fathoms depth, they are destitute of convenient air, either to breathe in or to make their candles burn. And that when there happens to be a great quantity of a winter's standing water in a deep mine, they commonly bring or drive up an adit for drawing away such water; but as soon as that part of the level is made, that any of the standing water begins to run off, the men must secure themselves from danger of being dashed in pieces against the sides of the adit; for the inclosed air or wind in the standing water breaks forth with such a terrible noise, like that of a piece of ordnance, and with such violence as to carry all before it, loosening the very rocks, though at some distance in the work or adit.

He observed also on several occasions, in sailing between London and Plymouth, that in a calm, the way in which the sea began to loom or move, the next day the wind was sure to blow from that point of the compass.

Hail Stones of an unusual Size. By Dr. Nath. Fairfax. N° 26, p. 481.

July 17, 1666, about 10 in the forenoon, there fell a violent storm of hail about the coast towns of Suffolk. The hail was small near Yarmouth; but at Seckford-Hall, one hail-stone was found by measure to be 9 inches about; one at Melton 8 inches about; at Snape-bridge 12 inches round. A lady of Friston Hall, putting one of them into a balance, found it weigh 12s. 6d. Several persons of good credit in Aldborough affirmed that some hail-stones were full as large as turkeys' eggs. A carter had his head broken by them through a stiff country felt; in some places his head bled, in others bumps arose; the horses were so pelted that they hurried away his cart beyond all command. The hail-stones seemed all white, smooth without, shining within.

Account of a great Number of Stones found in one Bladder. By Dr. Fairfax. N° 26, p. 482.

Mr. Goodrick, surgeon at Bury St. Edmunds, affirmed to me, that himself cutting a lad of the stone (for which he has a great name) took out thence at one time 96 small stones, all of them of unlike shape, size, corners, sides;

some of which were so bestowed as to slide upon others, and had thereby worn their flats to a wonderful sleekness. He assured me also, that in the same place, another when dead had a stone taken from him, almost as large as a new-born child's head, and much of that shape.

A Well and Earth in Lancashire taking Fire at a Candle. By THO. SHIRLEY, *Esq. N° 26, p.* 482.

About a mile from Wigan in Lancashire is a spring, the water of which is supposed to burn like oil. It is true that when we came to the spring, and applied a lighted candle to the surface of the water, there was suddenly a large flame produced, which burned vigorously. Having taken up a dishful of water at the flaming place, and held the lighted candle to it, the flame went out. Yet I observed that the water at the burning place boiled and rose up like water in a pot upon the fire, though my hand put into it felt no warmth.

This boiling I conceived to proceed from the eruption of some bituminous or sulphurous fumes; considering this place was not above 30 or 40 yards distant from the mouth of a coal pit there. And indeed Wigan, Ashton, and the whole country for many miles compass, is underlaid with coal. Then applying my hand to the surface of the burning place of the water, I found a strong breath like a wind bear against my hand. Upon making a dam, and hindering the recourse of fresh water to the burning place, I caused that which was already there to be drained away, and then applying the burning candle to the surface of the dry earth at the same point, where the water before burned; the fumes took fire and burned very bright and vigorous. The cone of the flame ascended a foot and a half from the surface of the earth. The basis of it was of the compass of a man's hat about the brim. I then caused a bucket full of water to be poured on the fire, by which it was presently quenched. I did not perceive the flame to be discoloured like that of sulphurous bodies, nor to have any manifest smell with it. The fumes when they broke out of the earth and pressed against my hand, were not to my best remembrance at all hot.*

Account of ATHANASII KIRCHERI *China Illustrata. N° 26, p.* 484.

This book is esteemed one of the most curious of the many productions of the industrious, but credulous Kircher. It contains a vast mass of extraordinary particulars relating to the population, revenue, mountains, isles, lakes,

* The fumes here mentioned were inflammable air or hydrogen gas, of which the rapid ascent through the water gave it the appearance of boiling.

rivers, canals, plants, animals, fossils, architecture, towns, walls, turrets, bells, printing, artillery, gunpowder, &c. &c.—Of their bells, one at Pekin is said to weigh 120 thousand pounds.—Of their stupendous bridges, one is 360 perches long, and a perch and half broad: it is without any arch, standing on 300 pillars, supporting very long and large stones laid over the tops of them. Another is built from one mountain to another, of one vast arch only; being 400 cubits long, and 500 cubits high from the surface of the river running under it. The description and figure of this bridge are given by Perhault, as well as in this place, as exhibited in our plate 5, fig. 1.—The Chinese wall, said to be built 215 years before Christ, is described as of 300 German miles in length, 30 cubits high, and from 12 to 15 cubits in breadth; so that several horses could go abreast upon it. A part of it is exhibited pl. 5, fig. 2.

An Advertisement concerning the Invention of the Transfusion of Blood. By Mr. Oldenburg. N° 27, p. 489.

The purport of this advertisement is to show that the transfusion of blood originated in the suggestions of some ingenious members of the Royal Society, several years before it was tried in foreign countries; and that in England it was first put in practice by Dr. Lower, and afterwards by Dr. King.

An Account of some Experiments of injecting Liquors into the Veins of Animals. By Signior Fracassati, Professor of Anatomy at Pisa. N° 27, p. 490.

1. Having infused into the jugular and crural vein of a dog some aquafortis diluted, the animal died presently; and being opened, all the blood in the vessels was coagulated. The great vessels were burst.

2. Into the veins of another dog some spirit of vitriol was infused, which had not so immediate an effect, for the animal suffered a great while, and foamed like epileptics, having its respiration very thick. After death, his blood was found coagulated and grumous, resembling soot.

3. Into the veins of another dog was injected some oil of sulphur; but he did not die, though this infusion was several times tried upon him. The wound being closed and the dog let go, he fell to gnawing some bones which he found, with great avidity, as if this liquor had caused in him a great appetite.

4. Another dog, into whose veins some oil of tartar was injected, did not escape so well: for he suffered much, and after being greatly swoln died. Being opened, the spectators were surprised to find his blood not curdled; but on the contrary more thin and fluid than ordinary.

An Account of some Discoveries concerning the Brain and the Tongue, made by Signior MALPIGHI,* *Professor of Physic in Sicily.* *N°27, p. 491.*

1. This anatomist states that he has discovered, that the exterior and softer part of the brain does not cover only the corpus callosum, as has been believed hitherto, but is also inserted into it in many places. He has also observed, that the corpus callosum is nothing but a contexture of small fibres, issuing from the medulla spinalis, and terminating in the said exterior part of the brain. And these fibres, he says, are so manifest in the ventricles of fishes' brains, that when they are looked through, they represent the figure of an ivory comb.

2. The use which he ascribes to the brain is different, he says, from what has been assigned to it hitherto. He pretends that as half, or at least a third of the blood of an animal is conveyed into the brain, where yet it cannot be consumed, the finest serum of this blood is filtrated through the exterior part, and then entering into the fibres of the brain, is thence conveyed into the nerves,† which he affirms to be the reason that the head is so often found full

* Marcellus Malpighi was born in the year 1628, in the neighbourhood of Bologna, where he studied and took his degree of doctor of physic in 1653. He was elected to the professorship of the theory of medicine in that university in 1656, but soon afterwards accepted of a similar appointment at Pisa, which situation he resigned at the end of three years, as the air of that place was prejudicial to his health. In 1662 he succeeded Castelli in the professorship of physic at Messina, where he remained four years, and then returned again to Bologna. Here he continued as a teacher of medicine in the highest repute from 1666 to 1691, when he was invited to Rome and appointed chief physician to Pope Innocent XII. He died at Rome of an apoplexy, in 1694. Malpighi's labours have thrown great light upon the structure and physiology of the human, brute, and vegetable creation; as may be seen by consulting his Anatome Plantarum, his Epistolæ Anatomicæ, his Exercitationes Anatomicæ, his Dissertationes de Utero, de Formatione pulli in ovo, de bombyce, &c. &c. These tracts were collected into two folio vols. printed in London in 1686, under the title of Malpighii Opera Physica et Medica. And in 1697 a third folio volume appeared, containing his Opera Posthuma. The first of these collections of his works was re-printed at Amsterdam, and the second at Leyden, each in one volume 4to. He wrote memoirs of his own life, dedicated to the Royal Society of London, of which he was a member. In his anatomical investigations he resorted to what in those days were new methods; viz. to maceration of the parts, injection of the vessels with coloured liquors, and the employment of magnifying glasses. By such means he was very successful in developing the intricate structure of some of the viscera in man and quadrupeds, as well as the minute fabric of insects and vegetables. He appears to have been the first who used the microscope for examining the circulation of the blood.

† This idea of the nerves being filled with serum, and thence producing hydrocephalus, is extremely erroneous.

of water when the brain has received a wound or an alteration by some distemper.

3. He has taken a particular care of examining the optic nerve in divers animals, it being one of the most admirable productions in the brain. Having therefore, among other fishes, dissected the head of a xiphias or swordfish, which has a very large eye, he has not observed any considerable cavity in the optic nerve, nor any nervous fibres; but found that the middle of this nerve is nothing else but a large membrane folded according to its length in many doubles almost like a fan, and invested by the dura mater. Eustachius, a celebrated anatomist, had written something of this before, but obscurely, and without mentioning the animal wherein he had made this observation.

4. He thought he should have met with the same thing in terrestrial animals, but he found that fishes alone have such a structure of the optic nerve: for that of an ox, pig, and other such animals is nothing but a heap of many small fibres of the same substance with the brain, wrapped about with the dura mater, and accompanied with many little blood-vessels. Hence he decides that great question among anatomists, whether the optic nerve be hollow or not? For, says he, it cannot be otherwise but there must be many cavities in this nerve, forasmuch as the small filaments of which it is composed cannot be so closely joined that there should not be some void space between them.

5. Concerning the tongue, the same author has discovered in it many little eminences, which he calls papillary, and believes to be the principal organ of taste.

[See Number 20 of the Transactions, page 135, where a full account is given of this last discovery, from the treatise of Bellini de Organo Gustûs.]

An Experiment of Sig. Fracassati upon Blood grown Cold. N° 27, p. 493.

When blood is suffered to become cold in a dish, that part which is beneath the superficies appears much blacker than that on the top; and it is vulgarly said, that this black part of the blood is melancholy blood, and men are wont to make use of this example to show, that the melancholy humour, as it is called, enters with the three others into the composition of the blood. But Signior Fracassati maintains that this blackish colour comes from hence, that the blood which is underneath is not exposed to the air, and not from a mixture of melancholy: to prove which he assures, that upon its being exposed to the air it changes colour and becomes of a florid red.*

* This observation, made so many years ago by Fracassati, contains the germ of the modern theory of the oxygenation of the blood in the lungs.

Quicksilver found at the Roots of Plants, and Shells on Inland Mountains. By Sig. MANFREDI SEPTALI. *N° 27, p.* 493.

In the valley of Lancy, which runs between the mountains of Turin, grows a plant like the doronicum, near the roots whereof is found pure quicksilver running in small grains like pearls. The juice of the plant being expressed and exposed to the air of a clear night, there is to be found as much mercury as it lost of juice.*

In a voyage he made a few years since to Genoa, when passing over some mountains he found great store of different shells, as the turbinets, echini, and some pearl shells, whereof one had a fair pearl in it, which he says he put into his repository.

Observations made in a Voyage from England to the Caribbee Islands. N° 27, p. 494.

This observer having noticed at Deal the great difference in the rusting of iron in such houses as front the sea, in comparison of that effect in the street immediately placed behind the former, he was told that it rusted more at high floods than at neap tides; the height of the beach hindering the saline exhalations. This remark reminded him of the vanity of the argument of M. Ligons and others, viz. That the air of the West Indies was hot and moist, because of the rusting of the iron; whereas it proceeds from some other principle in the air; for at the point of Cagua in Jamaica, where it scarcely rains 40 showers in a year, iron rusts as much or more than any where else: in Jamaica it rusts least in rainy weather.

The steams of the sea are of such a nature, that the sweet meats rotted; sugar of roses and other lozenges grew moist, though there was no rainy weather. And those pyes and gammons of bacon which had kept well before, after they had been once exposed to the open air, spoiled more in a day or two than in six weeks before.

At the point Cagua the iron guns of the fort were so corroded that some were become almost useless, being perforated like honeycombs; and some pounds of rusty iron broken off with a hammer. But the guns which lay in the salt water were not much damaged by rust.

Many other things receive damage by the air: not only iron rusts, but even linen rots, and silks once exposed to the air rot, without losing their colour. If a lancet be once exposed to the air it will rust, though you presently put it up again; but if it be never exposed to the air it will hardly rust.

* This assertion is erroneous; if this plant had not grown in that place, quicksilver would still have been found there. No attraction could subsist between them.

To preserve ale, he was directed to put to every rundlet of five gallons, after it is placed in the ship, two new laid eggs whole, and to remain in it: that in a fortnight's time, or little more, the whole egg shells would be dissolved, and the eggs become like wind eggs, inclosed only in a thin skin; that after this the whole white would disappear, but the yolk would remain unaltered. By this means the ale kept all the way to Jamaica, and it was much better than at Deal. That if eggs be thus put into March beer after it has done working, they preserve it from ever growing harsh.

Concerning the Thames water, it is not only observable, that in eight months time it acquires a spirituous quality, so as to burn like spirit of wine; and some East India ships it is said, have been in danger of firing, by holding a candle near the bung-hole at the first opening of the cask. If you take the bung out of any cask that stinks, it will in 24 hours become sweet again; and if you take a broom stick and stir it about well, it will become sweet in four or five hours, depositing a black lee to the bottom, which re-mixes with it, and so occasions a third or fourth fermentation and stench, after which it stinks no more. But though Thames water on stinking do not putrify, yet other waters become irrecoverable upon stinking, and dangerous to drink.

I observed at sea the fallacy of Glaüber's opinion, that the water as it grows salter becomes greener. For after we were out of the Narrow, the sea grew darkish, and after perfect azure, yet was it much more salt the further we went, as I found by a waterpoise of glass, with quicksilver at the one end. It rose about half an inch above the sea water in the Downs; and at 24 degrees more, two inches; but after that I never observed any difference all the way to Jamaica; which is contrary to another observation, that the nearer the tropics and the line the salter the sea.

As to the burning of the sea, he could never observe so great a light as to perceive fishes in it, though the light was sometimes great. At Deal it shone more the night before we set sail than ever after in the voyage: all the water ran off the oars almost like liquid fire; the wind was then south-east; and it is observed that at east and south winds it shines most.

It sometimes happens that two contrary winds poise each other, and make a calm in the midst, ships at a distance sailing with contrary gales at the same time.

It is observable, that in the Indies such places as have any high mountains have every night a wind that blows from the land. And in Jamaica every night it blows off the island every way at once, so that no ship can any where come in by night, nor go out but early in the morning before the sea breeze come on. As the sun declines, the clouds gather, and shape according to the mountains, so that old seamen will tell you each island in the afternoon towards evening by the shape of the cloud over it. And not only mountains, but other high parts, such as trees, will also cause a collection of the clouds; so that if you destroy the woods, you abate or prevent the rains. So Barbadoes has not now half the rains that it had when more wooded. In Jamaica likewise the rain is diminished as the plantations are extended. In the harbour of Jamaica there are many rocks, shaped like bucks' and stags' horns: there grow also several sea plants, whose roots are stony; of which some are insipid, but others perfectly nitrous. On these plants there gathers a limestone, which fixes not upon other sea fans growing near them. It is observable also, that a Monchinel apple falling into the sea, and lying in the water, will contract a lanugo or down of saltpetre.

It is commonly affirmed, that the seasons of the year between the tropics are divided by the rains and fair weather, allotting six months to each season. But this observation holds not generally true: For at the Point in Jamaica it hardly rains forty showers in a year, as before observed, beginning in August to October inclusively. From the Point you may look towards Port Morant, and so along to Ligonee, six miles from the Point, and you will scarcely see for eight or nine months, beginning from April, an afternoon in which it rains not. At the Spanish Town it rains only for three months in the year, and then not much. At the time it rains at Mevis, it rains not at Barbadoes. And at Cignateo, otherwise called Eleutheria, in the Gulph of Bahama, it rains not sometimes in two or three years, so that the island has been twice deserted for want of rain to plant in.

At the Point of Jamaica, wherever you dig five or six feet deep, water will appear, which ebbs and flows with the tide. It is not salt but brackish, is unwholesome for men, but good for hogs. At the Caymans there is no water but what is brackish also; yet is that wholesome for men, insomuch that many are recovered there, by feeding on tortoises, and yet drink no other water.

The blood of tortoises is colder than any water there; yet is the beating of the heart as vigorous as that of any animal, and their arteries as firm. Their lungs lie in their belly, below the diaphragm, extending to the end of their shell. Their spleen is triangular, and of a firm flesh, and floridly red. Their liver is of a dark green, inclining to black. In the œsophagus or gullet are a sort of teeth, with which they chew the grass they eat in the meadows growing at the bottom of the sea.

All the tortoises from the Caribbees to the Bay of Mexico and Honduras, repair in summer to the Cayman Islands, to lay their eggs and to hatch there. They coot for fourteen days together, then lay in one night about three hundred eggs, with white and yolk, but no shells: then they coot again, and lay in the sand, and so on thrice: after which the male is reduced to a kind of gelly, becomes blind, and is so carried home by the female. Their fat is green, but not offensive to the stomach. The urine of those that eat it is oily, and looks of a yellowish green colour.

There is no manner of earth but sand at the Point; yet melons, musk, and water melons thrive well there. A great many trees also grow there, especially mangranes and prickle pears. In other parts one may ride through woods that are full of very large timber, and yet have nothing of earth, only firm rock to grow in.

In some ground that is full of saltpetre, and where tobacco grows wild, it flashes in smoaking.

The fruit trees there of the same kind ripen not all at one time, some are observed in flower, others with ripe, others with green fruit, and others done bearing at the same time. The sower-sop, a pleasant fruit there, has a flower with three leaves; when these open they give so great a crack, that the observer has more than once run from under the tree, thinking it was tumbling down.

There is a bird called a pelican, but is a kind of cormorant, of a fishy taste, but when buried in the ground for two hours, it loses that taste.

In analysing some bodies, by letting ants eat them, he found that they would eat brown sugar till it became white, and at last reduce it to an insipid powder. So they reduced a pound of sallet oil to two drams of powder.

At first coming to Jamaica, people sweat continually in great drops for three quarters of a year, and then it ceases. During that space they are not more dry, more costive, or make less urine than in England. Neither does all that sweat make one faintish. If one be dry, it is a thirst generally arising from the heat of the lungs, and affecting the mouth, which is best cooled by a little brandy.

Most animals drink little or nothing there, as hogs, even horses in Guanaboa never drink, nor cows in some places of the island for six months; goats drink but once perhaps in a week; parrots never drink, nor parrokets, nor civet-cats, but once a month.

The hottest time of the day is about eight in the morning, when there is no breeze. On placing a weather-glass in the window, it did not rise considerably at that time, but by two o'clock it rose two inches.

Venice treacle became so dry in a gally-pot as to be friable; and then it produced a fly called a weevil, and a sort of white worm. The pilulæ de tribus also produce a weevil.

There is in the midst of the island a plain, called Magotti Savanna, where when it rains, the drops, as they settle on the seams of any garment, turn in half an hour to maggots.

Magnetical Experiments; also, an excellent Liquor. By Mr. COLEPRESS. *N° 27, p. 502.*

I took an unpolished loadstone, which attracted but weakly; and heated a lath nail glowing hot, nimbly applying the north-pole of the said magnet to it, which quickly took it up and held it suspended a great while. I took the same stone and cast it into the fire, letting it remain there till it was thorough hot; I applied the north-pole to another lath nail cold and untouched before, which it took up, but faintly, yet held it suspended for some time. Two or three days after, I took the same loadstone, and found that it attracted then as strongly as before it was cast into the fire. Whence I inferred, that the fire somewhat lessened its attractive faculty, but did not deprive the stone of it.

The liquor announced is a composition of the juices of good cyder apples and mulberries, producing the best tasted and most curiously coloured liquor.

An Account of some Books. N° 27, p. 503.

I. The History of the Royal Society of London, for the advancement of Experimental Philosophy, by Tho. Sprat.

II. Disquisitio Anatomica de Formato Fœtu; authore G. Needham, M. D. London, 8vo. An Anatomical Inquiry into the Formation of the Embryo or Fœtus; by Walther Needham,* M. D.

This disquisition consists of seven chapters.

In the first he inquires into the passages by which the nourishing juice is conveyed to the womb of the animal; where he examines the assertion of Everhard, importing that some of the lacteal vessels carry the said juice to the uterus; which vessels are pretended to have been seen by himself in the dissection of rabbits. Which engaged our author to take up again the anatomical knife, and to dissect with all possible accuracy not only some of the larger animals, as cows and mares, but some of the smaller kind also, such as rabbits, which are instanced by Everhard.

But having spent all his labour and care herein in vain, and besides evinced by ligatures, that the pretended vessels are neither those that are described by Bartholin, under the name of lymphatic, nor others presumed to be known

* W. Needham was an excellent English anatomist, and distinguished himself greatly by the work of which an account is here given. Further particulars respecting this author's anatomical discoveries may be seen in the 3d vol. of Birch's History of the Royal Society.

by Everhard alone, as immediately carrying the chyle out of its receptacle to the womb and breasts; he imputes the cause of this mistake to the trunk of the lymphatics running over the vena cava into the receptacle near the emulgents, which duct he affirms to have often found filled with chyle from the intestinum rectum, or the ileum, or cæcum (a dog having no colon;) but maintains withal, that by ligatures it is manifest that that duct goes to the receptacle, and there deposits its liquor; which he proves to be alike true of all the milky vessels, so that they carry nothing back, and consequently are unfit to convey any thing to the womb. This he illustrates by a noble experiment of that learned and expert anatomist Dr. Lower, using to open sometimes the right side of the thorax, and with his fingers to break the receptacle; and sometimes on the left side, the ductus thoracicus a little under the subclavian; whereby it has come to pass that dogs, well fed all the while, have thrown out all the chyle into the opened part of the thorax, and though plentifully fed, were starved within three days: there appearing mean time in the veins opened a crass blood, destitute of serum, but not any mixture of transmitted chyle.

Having rejected the lacteal and lymphatic vessels from this office, he declares, that we must rest in the ancient doctrine, which lays the task of conveying the succus nutritius to the breasts and womb upon the arteries; unless the nerves be called in for aid, for conveying some of the spirituous juice, to be mixed with the nutritious to give life and vigour; and having proved this, he takes notice of the multitude of anastomoses, remarkable in the womb of pregnant creatures; and subjoins a discussion of the way how the alimentary juice is in the womb severed from the mass of the blood: whether by mere percolation, or by some ferment working upon the blood, and thence precipitating what is proper for the use of that part.

In the second chapter he treats of the placentas and glandules, and shows how many ways the juice is derived from the womb to the fœtus: First, simply from the membrane of the uterus to the membrane of the fœtus; as in all oviparous creatures; and among viviparous, in a sow all the time of her bearing; in a mare for half the time; and in a woman the first month only. Secondly, By a mass of flesh filtering the juice; as in all cake-bearing (called in Latin placentifera) and in all kernel-bearing (called glandulifera) or ruminating animals. Where he gives a particular account of the double placenta or cake to be found in rabbits, hares, mice, moles, &c. and examines the learned Dr. Wharton's doctrine, assigning a double placenta to at least all viviparous animals, so as one half of it belongs to the uterus, the other to the chorion; showing how far this is true, and declaring the variety of these phænomena, together with a very ingenious assignation of the cause of that variety. Where do occur many uncommon observations concerning the difference of milk in ruminating and other animals; the various degrees of thickness of the uterine liquor in oviparous and viviparous creatures; the property of the humour, turning into eggs, with a hint of the cause of their being excluded, and not quickened and formed within; as also, of the cause of moles in the womb, and of many kernelly and fleshy substances in other parts of the body; where he takes notice of a concretion seen by himself grown to the cone of the heart, of nine ounces weight, in a healthy body, that died of a violent death; and of the like adhering to the spleen, kidneys, and liver, without any perceived trouble to the animal; yea of some found within the heart itself.

He adds the number, shape and use of these placentas; and first observes, that those that are kernel-bearing (glandulifera) animals, or chewing the cud, have many; and those that are cake-bearing (placentifera) have for the most part one cake for each fœtus; but a woman commonly but one, though she happen to have many embryos.

He annexes a particular description of the placenta of a woman, as the most considerable, and teaches how it may be most conveniently severed from the vessels to render them conspicuous, which are a numerous offspring of arteries, veins and fibres; of the last whereof he inquires whether they be the capillaries of the arteries and veins, or nervous.

The shape of that in a woman is orbicular, about a foot large, and two inches thick; one of its superficies convex, but uneven, the other concave, and every where sticking close to the chorion.

The use of the placentas is known to be, to serve for conveying the aliment to the fœtus. The difficulty is only about the manner. Here are examined three opinions of Curvey, Everhard and Harvey. The two former do hold, that the fœtus is nourished only from the amnion by the mouth; yet with this difference, that Curvey will have it fed by the mouth when it is perfect, but whilst it is yet imperfect by filtration only through the pores of the body, and by a kind of juxta-position; but Everhard, supposing a simultaneous formation of all the instruments of nutrition together at first, and esteeming the mass of blood by reason of its asperity and eagerness unfit for nutrition, and rather apt to prey upon, than feed the parts, maintains that the liquor is sucked out of the amnion by the mouth, concocted in the stomach, and thence passed into the milky vessels, even from the beginning. Meantime they both agree in this, that the embryo doth breathe, but not feed through the umbilical vessels.

This our author undertakes to disprove; and having asserted the mildness of at least many parts of the blood, and consequently their fitness for nutrition, he defends the Harveian doctrine of the colliquation of the nourishing juice by the arteries, and its conveyance to the fœtus by the veins.

In the third chapter, the membranes and humours of embryos are considered. The membranes are in some three, in others four, in an egg six. All placentiferous animals (if I may assume this word) he affirms to have three membranes, and sows, mares, and women also; but only two humours. Again, bitches, cats, and conies four membranes, and three humours; so that the number of the membranes has been hitherto observed always to exceed that of the humours.

Giving the history of both, he begins from sheep, cows, and other ruminating animals, describing first the chorion, assigning its use, and comparing it with that in deer, sows, mares, women, rabbits, bitches and cats, when with young. Then he proceeds to the description of the allantoides (the membrane immediately encompassing that skin wherein the fœtus is wrapped) and thence to that of the amnion, wherein the embryo itself lies swimming in its alimental liquor; and lastly to that which is observed to be in bitches, cats and rabbits, and contains a very good and nourishing juice; which how it comes thither, is a difficult inquiry, as well as that other, how the liquor gets into the amnion. To resolve both which our author, having disproved the filtration of the liquor held by Curvey and Everhard out of the chorion into the amnion, and evinced that the liquor in the allantoides, interjected between those two, is urinous, he concludes, that the alimentary juice passes through the umbilical vessels, by a proper artery, depositing it in those membranes we speak of, and reserving it there for the use of the fœtus.

Concerning the humours he affirms, that all of them in all animals are nutritive, except that in the allantoides. He observes also, that most oviparous fishes have eggs or spawn, as to sense of one only colour, and but one humour; yet that the spawn of a skate has a white and a yolk. Birds have mostly three nutritious substances that are visible, viz. a yolk and a double white; to which upon incubation comes a fourth, colliquated out of the former; the tender embryo feeding upon the two whites, till they being consumed, the yolk of the chick now to be hatched is shut up in the abdomen, and thence by a peculiar duct conveyed into the guts; and so serves the young bird for breasts it is fed by until the twentieth day.

In viviparous creatures are found sometimes two, sometimes three humours, and in bitches, cats, and rabbits four; which perplexes the author as to the giving a reason for it. These humours, he says, he has examined, by concreting, distilling, and coagulating them; where he furnishes the reader with no vulgar observations. He concludes this chapter by observing, that there is also air in the said membranes; which, besides other arguments, he proves from the crying of infants in the womb (of which he alleges a memorable and well attested example in a child of an English lady in Cheshire, the child being yet alive and in good health;) and from chickens often heard to pip in the egg, both before the breaking of the shells, and after the membranes being yet entire; ascribing the production of this air to the spirituous liquor in the membrane, apt to ferment, and thereby causing store of exhalations.

The fourth chapter discourses of the umbilical vessels; and observes first, that they differ in different animals, and hold proportion to the membranes and liquors, so as those that have two liquors have four membranes, and three liquors have six: the oviparous also being furnished with a duct, passing to the guts, because they want breasts, and their yolk is shut up in the belly.

The umbilical arteries belonging to the placenta, and commonly said to be derived from the crurals, are by him affirmed to proceed from the end of the aorta. They are here described, and their several portions distributed for the chorion and amnion. Then an account is given of the hepatic vein, corresponding to the arteries. It is in viviparous animals inserted into the vena porta, passing again with the remaining blood through the canalis venosus into the cava without percolation made in the liver. In birds it enters not into the liver, but passes over its convexity into the cava. A description also is made of the urachus, found in all viviparous creatures, though by many writers denied to be in man, who notwithstanding has need as well as other such animals somewhere to lodge his urine. The oviparous want this umbilical cord, but yet are furnished with fit sanguineous vessels, which here also are explained; especially the ductus intestinalis, said to be omitted by Dr. Harvey, and to have been known to the author long before Mr. Steno claimed the discovery of it; for which he appeals to the testimony of Mr. Boyle, and three worthy physicians, Willis, Millington and Lower; as also to that of two ingenious Frenchmen, Guison and Fiard, to whom our author affirms to have showed, An. 1659, when they were going over into Holland, not only this duct, but also the ductus salivales, and the passages of the nostrils, published afterwards by the said Steno.

The use of this ductus intestinalis is esteemed to be the conveying of the yolk into the guts for a second coction, there made by the pancreatic juice, acknowledged to be excellently handled by the learned Sylvius, and his ingenious scholar De Graeff, from the former of whom our author yet dissents about the mixture of the gall with the said juice in the heart, refuting it by several experiments.

The fifth explains the communion of vessels in embryos; in whom he says, three anastomoses are usually observed, which as soon as the fœtus is born are closed. They are called foramen ovale, canalis arteriosus, and venosus. The two former to be met with about the heart; the last in the liver. All three here described by the author, who also compares, as Harvey does, the fœtus yet in the womb with the manner of operation of those animals that are provided but with one cavity in the heart and with no lungs; the blood of the fœtus, as long as it is unborn, passing neither through the parenchyma of the lungs, nor that of the liver. Lastly, The necessity of respiration is explicated, and how the defect of lungs, and of one of the ventricles of the heart, is supplied in fishes, viz. by comminuting and mixing the blood in the gills. To which is annexed the manner of respiration in amphibia which are furnished with lungs and two ventricles of the heart, and yet, if Bartholin misinforms us not, keep the foramen ovale all their lifetime open; which yet our author calls in question, alleging to have seen no diving animals which had not the said foramen closed after their being born.

The sixth makes a digression, to discourse of the biolychnium, and the ingress of the air into the blood, for the generation of spirits, and the pretended kindling of a vital flame. But our author can see nothing that may prove either the existence or the necessity of such a flame. He does not question whether air is received into the mass of blood, but only doubts whether through the lungs there be a high way for the air to the blood.

After this our author gives his thoughts both of the true use of the lungs and of sanguification.

The lungs, he says, serve chiefly, by their constant agitation, to comminute the blood, and so to render it fit for a due circulation; which office he thinks to be performed in fishes by the continual motion of their gills, a succedaneum to lungs.

Sanguification, according to him, is chiefly performed and perfected by the frequent pulsions of the heart, and the repeated contractions of its left ventricle at the passing of the sanguineous liquor from thence into the aorta.

The seventh and last chapter contains a direction for the younger anatomists, of what is to be observed in the dissection of divers animals with young: and first, of what is common to all the viviparous; then, what is peculiar to several of them, as a sow, mare, cow, ewe, she-goat, doe, rabbit, bitch, and a woman: lastly, what is observable in an eel, skate, salmon, frog, &c. The whole is illustrated by many accurate engravings.

An Account of more Trials of Transfusion, with some Considerations thereon, chiefly in reference to its circumspect Practice on Man; together with a farther Vindication of this Invention from Usurpers. By Mr. OLDENBURG. *N° 28, p.* 517.

In this paper Mr. Oldenburg states, that as the experiment of transfusion has caused disputes among the curious both in England and other countries, so it has put some upon suggesting such measures and cautions as may render it safe and beneficial. Of the latter number is Mons. Gasper de Gurie de Montpoly, who, while he admits the invention to be ingenious, and such a one as promises to be useful, is nevertheless of opinion that it requires to be practised with much circumspection, otherwise it may be productive of mischief. No considerate person, he observes, will venture upon a total transfusion, though he thinks a partial one may be serviceable in some cases. To this purpose he remarks, that he was pleased to learn that a moderate intromission of blood had (according to his conjecture) succeeded well in the human subject, as appears from a letter of Mons. Denis. The success of a larger transfusion in another instance he ascribes to the healthy and robust constitution of the individual, a labouring man. But to these instances he subjoins two others, where the event was unfavourable. One of these instances occurred in Baron Bond, son to the first minister of state of the King of Sweden, who underwent the operation twice, and appeared to be strengthened by it the first time; but died soon after the second operation. His disease was a mortification of the intestines, and he is thought to have been an unfit subject for the experiment. The other instance was that of a dog, in a trial made by Mons. Gayen. He drew three great dishes of blood from the dog that was to receive, and weighed the other dog that was to furnish. When the operation was over he weighed the latter again, and found his weight less by two pounds: of which, after abating an ounce more or less for the urine the dog made, and an ounce or two more for the blood spilt in the operation, there remained at least $1\frac{1}{2}$lb. of blood that was transfused. But the recipient dog, though well dressed and fed, died five days after, the emittent dog being then alive. Lest an inference prejudicial to the cause of transfusion should be drawn from the preceding observations, Mr. Oldenburg remarks, that although considerable caution may be requisite, yet several large transfusions have been successfully made in London, of which a very remarkable instance was afforded in a bitch, which lost in the operation near 30 ounces of blood, and was recruited accordingly. The animal not only survived to the date hereof, but afterwards underwent a more severe experiment, in which her spleen was cut out, without tying up the vessels whence that viscus was separated. Since which time (even before the wound was healed up) she took dog, was with puppy, brought forth whelps, and remains well and jocund. Some reasons are then assigned why philosophers in England were backward in performing the experiment of transfusion upon men, upon whom Mons. Denis of Paris was the first to make the trial; a circumstance of great exultation to the French. Mr. Oldenburg observes that the English are more tender in hazarding the life of a fellow creature, and that the law also among us is more strict and jealous in cases of this nature. Yet, he adds, it was a matter that had been in preparation several months since, and that the following method of operating was agreed upon, as suggested by Dr. Edmund King, and described by him in the following letter.

SIR,

The method of transfusing blood you have seen practised, with facility enough, from beast to beast; and we have things in readiness to transfuse blood from the artery of a lamb, kid, or what other animal may be thought proper, into the vein of a man. We have been ready for this experiment these six months, and wait for nothing but good opportunities, and the removal of some considerations of a moral nature. I gave you a view, you may remember, a good while ago, of the instruments which I think very proper for the experiment, which are only a silver tube, with a silver stopper somewhat blunted at one end, and flatted at the other, for conveniency of handling, used already upon beasts with good success. The way in short is this: After the artery is prepared in the lamb, kid, &c. let a ligature be made upon the arm, &c. of a man (hard enough to render the vein turgid) in the place you intend to insert the lesser end of the silver pipe, which is so fitted, that the silver stopper thrust into the tube reaches somewhat by its blunt end beyond one of the ends of that tube. This done, divide the skin of the part in the same manner that is used in cutting an issue, just over the vein to be opened. Then with a fine lance open the vein; or, if you please, in case the vein lie fair and high (especially if the skin be fine) you may open both together, according to the usual way of letting blood. Which done, let an assistant clap his finger, or a little bolster prepared beforehand, or the like, upon the vein, a little below the orifice, to hinder the blood from ascending. Keeping that position, insert the blunt-ended tube upwards into the vein; when it is in, hold it and the skin close together between your finger and thumb. Then pull out of the tube the stopper, and insert the pipe by which the arterial blood is to be infused from the emittent animal; managing the remainder according to the known method of this experiment.

Mr. Oldenburg concludes this account with references to facts and observations given in former Numbers of these Transactions, in proof that the transfusion of blood originated in England; though some of the French journals (he complains) have assigned the merit of this invention to their countrymen.*

* In a note at p. 131, we hinted that the notions which philosophers once entertained respecting the possibility of removing disease and prolonging life by the transfusion of blood, were extravagant and ill-founded. Nor will the justness of this remark be disputed, as applied to the generality of those morbid affections against which it was at first proposed as a remedy, and as applied also to the project of procuring indefinite longevity. It must be confessed, however, that the accurate and well-contrived experiments of Professor Harwood place it beyond a doubt, that in cases of sudden and profuse evacuations of blood, the fatal consequences, which would otherwise ensue, may in the brute creation be prevented by the immediate introduction of a proportionate quantity of fresh blood from another animal in a healthy state; a fact which seems convertible to medical use in certain cases of hæmorrhage occurring in the human subject. On this occasion we are happy to have it in our power to lay before the public an account of the above-mentioned professor's interesting experiments and observations hereon.

Dr. Harwood, the present professor of anatomy in the university of Cambridge, having perused the accounts of Lower and others concerning the transfusion of the blood, and being equally dissatisfied with the mode in which former experiments had been conducted, and the superstition and prejudice which occasioned the subsequent relinquishment of the practice; was induced in the year 1785 to make it the subject of his Thesis, preparatory to taking his M. B. degree in that year. Upon this occasion the professor made a number of very interesting experiments, which he has since repeated in a great variety of forms, some of them having been privately conducted, and others publicly exhibited at his lectures on comparative anatomy in the schools of the university. From a great number of very curious experiments, the following have been selected, and we have the professor's permission to lay them before the public.

Experiment 1. A dog of middling size, from whose *jugular vein* eight ounces of blood had been previously evacuated, was supplied with an equal quantity from the *carotid artery* of a sheep. During the operation the dog showed evident marks of uneasiness, but was little affected in any other way, till about twenty-four hours after the operation, when he had a shivering fit, succeeded by a considerable degree of heat, thirst, and the usual symptoms of fever, all of which disappeared in the course of the next day, and the dog remained afterwards in perfect health. This experiment being several times repeated, and the quantity of transfused blood being occasionally increased or diminished, the feverish symptoms were observed to be more or less violent in proportion to the quantity of *arterial blood* introduced into the *vein* of the recipient animal. It now occurred to Dr. H. that the uneasiness of the animal during the operation, and the febrile disease with which he was attacked some hours afterwards, might probably arise from the preternatural degree of stimulus occasioned by the introduction of the highly oxygenated arterial blood into the right side of the heart.

Experiment 2. The experiment was therefore repeated, with this difference, that the blood was conducted through the tube from the *jugular vein* of the sheep, instead of an artery. The result was exactly what the operator expected it to be, the animal was perfectly composed during the operation, and did not suffer the smallest inconvenience at any time afterwards.

On the Mendip Lead Mines. By Mr. JOSEPH GLANVIL. *N° 28, p. 525.*

In answer to former queries concerning mines, this gentleman replies, That Mendip is all mountainous. That it is barren and cold, and in some places rocky. That the ridges run confusedly, but mostly east and west. Its surface is heathy, ferny and furzy.

Experiment 3. Two pounds of blood were taken from a large pointer who had been previously weighed, and three pounds of sheep's blood from the *jugular vein* introduced. Dr. Harwood had been in the habit of letting out the superabundant blood when any of the animals had received too much, but in this experiment, it was suggested by the Rev. Mr. Midcalf, then fellow of Christ's College, that it would be curious to observe the effect which would follow such an extraordinary degree of artificial plethora as the introduction of an additional pound of blood (into the circulating system of an animal who had naturally not more than five) must necessarily occasion. The vein was accordingly secured in the usual way, and the dog being released ran into the court, and immediately attempted to relieve himself from this unusual plenitude by every possible evacuation. His efforts to accomplish this purpose not proving successful, he became still more uneasy and restless, and afterwards drowsy and stupid. These symptoms were succeeded or rather accompanied by a considerable degree of fever, which terminated in a copious evacuation of blood by stool, by urine, and by vomiting. He took no nourishment for three days after the operation, except pure water, and was more reduced than Dr. H. ever saw an animal in that space of time. He now drank a little milk, and afterwards some broth, from which time he gradually recovered his appetite and strength, and remained in good health for several years.

Experiment 4. All the blood of a pointer was let out (as far as it was possible to evacuate it) till the animal was in convulsions on the table, and apparently expiring. The blood was then transfused from the jugular of a sheep into the correspondent vein of the dog, and in less than half a minute after the introduction of the tube, he began to respire, and as soon as he had received a quantity of sheep's blood equal to what he had lost of his own, he leaped from the table and walked home, without experiencing any apparent inconvenience either then or at any subsequent period. This experiment was performed before a very crouded meeting at the public schools in the Botanic garden of the university. It has been frequently repeated since, and a variety of other animals have been subjected to the same experiments, and with equal success.

As the nature of our design will not admit of our relating a greater number of Professor Harwood's experiments, we shall close this article with a few pathological remarks from that gentleman.

From all the numerous experiments which have been lately made upon this subject, one important fact seems to be fully established, that the blood of an *herbivorous animal* may be substituted for that of a *carnivorous animal,* and *vice versa,* without danger, or even inconvenience to the animal who receives it. In cases therefore of such copious evacuations of blood as to threaten the death of the patient, would not transfusion be expedient? and if death should be inevitable without it, does it not become a duty to make the trial?

Professor Harwood is at present engaged in a course of experiments to ascertain whether diseases may be communicated, or medicines conveyed into the system, by the transfusion of diseased or medicated blood from one animal to another; which he will probably publish at some future time, together with a description of the easiest mode of performing the operation, and answers to all the objections which have hitherto been urged against the application of it on the human subject.

That the natives and inhabitants are healthy, and live to the ordinary age, except such as are employed in melting the lead at the mines; who, if they work in the smoak, are subject to a disease that kills them, and the cattle likewise that feed thereabouts. From the bottom of the hills there are many springs both to the north, south, and west; and those waters are very wholesome, and produce rivers after they have run to some distance from thence. The air is moist, cold, foggy, thick, and heavy. The soil near the surface is red and stony; and the stones are either fire-stones or lime-stones; but there is no clay, marl or chalk.

That the trees have their tops burnt, and their leaves and outsides discoloured and scorched with the wind, and grow to no size or height. The stones and pebbles that are washed by the brooks and springs are of a reddish colour and ponderous. Snow, frost, and dew remain longer on Mendip than on any of the neighbouring grounds.

That Mendip is unusually subject to thunder and lightning, storms, nocturnal lights and fiery meteors. There are no certain signs above ground that afford any probability of a mine to my knowledge.

The ore lies in veins as a wall; and is perfect lead, only on the outside covered with reddish earth.

The ore is beaten small; then washed clean in a running stream; then sifted in iron rudders; the hearth or furnace is made of clay or fire-stone, set in the ground, on which is built the fire, and lighted with charcoal, and continued with young oaken branches, blown with bellows by men's treading on them.—After the fire is lighted and the fire place hot, they throw the lead ore upon the wood, which melts down into the furnace; then with an iron ladle they take it out, cast it upon sand, and into what form they please.

Magnetical Variations. By M. Petit. N° 28, p. 527.

Nothing can be more agreeable to me than to discourse on this subject, especially with the philosophers of England, whence the philosophy of the magnet had its rise, and whence also the principal observations of the change of its declination are come to us; so that it is just that the observations made elsewhere concerning the same should return thither as to its source.

After I had made the experiments that are in Gilbert* and others, I made that of the needle's declination on three different meridian lines, which I traced,

* An English physician, who published a learned work, Anno 1600, on the load-stone. He was born at Colchester 1540; became a physician in ordinary to queen Elizabeth, and died in 1663, at 63 years of age.

Anno 1630, in several places of Paris, and found that the needle declined 4½ degr. north-east: this I made known here to the curious and to artists, some of whom counted nine or 10 degrees, according to the tradition and writings of Orontius Fineus and Castelfranc; others 11½ degrees, following Sennertus and Offusius.

You know that Gilbert, though the first who has written rationally on the magnet, asserts towards the end of his book, that if a magnet altogether round were placed on a meridian, and its poles so placed as to answer to the poles of the world, and consequently its axis to the axis of the world, the stone would continually of itself turn round in 24 hours. Whence he infers that the whole earth, as a great magnet, turns also round about its axis in the same space of time.

To try the truth of this proposition, I caused a magnet to be turned with the powder of emery of a spherical form, with all possible exactness, of 1½ inch in diameter; which, on account of its compact and uniform composition, had its three centres of magnitude, gravity, and magnetism all the same, with so much justness, that after I had exactly found the two poles of this stone, I caused two small holes to be made therein, to support it by two points of needles, as by two pivots: which having put in a meridian of brass, and suspended the ball between them like a little globe, it was so easily moveable that I made it turn every way with a blast only of my mouth, and it stopped indifferently, now in one, then in another place, not any side of it prevailing by its gravity, nor descending as it would have done, if any of them had been heavier than another.

This stone thus prepared, without any defect in virtue or figure, uniform, homogeneous, equilibrated, being adjusted on its meridian, and a horizon so placed on its meridian line, that the poles answered to the poles of the heavens, the result was that it had not any motion, and a small white mark I had made upon this stone remained still in the same place where I had put it without turning at all: whence I thought the proposition of Gilbert sufficiently refuted.

This stone, together with a greater one, served also to find out whether the needles touched in different places, nearer to or further from the poles, had different declinations. Which having tried frequently with these, and with other stones, I found no difference at all in the declination of the needles, which in all of them was 4½ degrees from the north-eastward. And as I did not suspect that this declination would have changed, having found it to be the same in many places, from Brest in Brittany to the Valtaline among the Alps, I believed the ancients had ill observed, and that the want of their exactness in respect either of the meridian line or the fabric of their needles, or the division of their circles, was the cause of this defect. But I was soon undeceived of my own error, when I learned a little while after, by letters from England, that Mr. Burrows,* Anno 1580, had near London observed the declination of the needle to be 11° 11 m. as well as Offusius and Sennertus: And that, An. 1612, Mr. Gunter,† professor of the mathematics, had in the same place found the declination much diminished, having then found it but six degrees: And lastly, that, Anno 1633, Mr. Gellibrand‡ had found it but four degrees north-east, conformable to my observations. Which did assure me, that those declinations were not constant but had varied.

And that I might be convinced by myself, I made from time to time experiments in divers places, and found still more and more diminution; so that, Anno 1660, in June, after I had very exactly traced a meridian by many azimuths, before and after noon, with a brass quadrant of six feet diameter, and applied good needles upon it, the one of seven, the other of ten inches long, I found that they declined but one degree or thereabout: And the last year, 1665, I found no more than ten minutes on the same meridian. Upon which having lately applied, since the receipt of the letter, the same two needles, it seems the declination is yet less than the last year. But this I can assure you, that the declination is yet some minutes towards the east, at least at Paris. So that you may upon my word doubt || of the observation of your friend, whom perhaps the meridian or the needle, or the construction and division of his compass, may have deceived to a degree and a half north-west, which he at the present assigns to the declination. But I doubt not but in 12 or 15 years it will be found true what he affirms, as I have prognosticated by my hypothesis, which makes the declination to vary a degree every seven or eight years.§

* Wm. Burrowes published, 1614, "The New Attractive, and the Variation of the Compass."

† Edmund Gunter, an excellent mathematician, was born 1581. He was author of many ingenious books and instruments. The land-surveying chain, and the logarithmic lines placed on rules and scales, are called by his name to this day. He became professor of astronomy in Gresham College, London, where he died in 1626, being only 45 years of age.

‡ Mr. Henry Gellibrand succeeded Mr. Gunter as professor of astronomy in Gresham College, where he died in 1636, at 39 years of age. He was strongly attached to the old or Ptolemaic astronomy. He wrote, however, some useful books on navigation; and after the death of Mr. Briggs he had the care of publishing the Trigonometria Britannica of that author.

|| By the favour of the author, it is not conclusive, that because the declination is yet somewhat towards the east at Paris, it must therefore be so at London; since it is known here, that even the variation of Whitehall differs from that of Limehouse; which two places are but about four English miles distant from each other.

§ According to Henry Bond, the variation "was first found to decrease by Mr. John Mair; secondly, by Mr. Edmund Gunter; thirdly, by Mr. Henry Gellibrand; fourthly, by himself in 1640; and lastly, by Mr. Robert Hooke, and others, in 1665." This change in the variation is continual and universal, but different in all different places and at all times. We shall hereafter have occasion

An Account of some Books. N° 28, p. 532.

I. Free Considerations about Subordinate Forms; by the Honourable Robert Boyle.

This tract is an appendix to the noble author's Examen of Substantial Forms.

II. Joh. Swammerdam, M. D. de Respiratione et Usu Pulmonum. John Swammerdam,* M. D. on Respiration and the Use of the Lungs.

This author is of opinion, that all those philosophers who have hitherto inquired into the nature and use of respiration have only caught the shadow of it, nothing of the substance. And he gives this for the chief reason, because they have been too negligent in considering the first manifest motion of the breast and lungs in a fœtus; which particular being understood, he thinks it very easy to judge of the respiration of born animals. He scruples not to reprehend the immortal Doctor Harvey, for having excluded from the office of

to relate in this work several other writings on this curious subject by Dr. Hook, Dr. Halley, and other persons. And it is to be lamented that all philosophical societies, in all countries, do not observe and publish in their Transactions, at least annually, the state of the magnetic direction, in their respective situations.

* This celebrated anatomist and natural historian was born at Amsterdam in 1637. His father was an apothecary in that city, and possessed a small cabinet of natural curiosities, by the frequent survey of which his son acquired a taste for those pursuits, by which he afterwards rendered himself so conspicuous. He studied at Leyden, where he took his degree of doctor in medicine in 1667, but never engaged in the practice of physic, devoting himself wholly to anatomical and physiological inquiries, and to collecting and examining insects. Of this class of animated beings he investigated the generation, structure, and metamorphoses, with astonishing patience and assiduity, and described and elucidated the same in his admirable work entitled, A General History of Insects, first published in the Dutch language in 1669, and afterwards translated into Latin. His Historia Ephemeræ appeared in 1675. These and other observations, relative to the natural history of insects, were collected into a folio vol. (Dutch and Latin) printed at Leyden in 1737, under the title of Biblia Naturæ sive Historia Insectorum. This edition was superintended by Boerhaave, who wrote the biographical memoirs which are prefixed to it; but the Latin translation was by Gaubius. Besides the tract on respiration, of which an account is given in the present Number of the Transactions, Swammerdam wrote another anatomical work, entitled, Miraculum Naturæ seu Uteri Muliebris Fabrica, published in 1672. He appears to have been the first who practised the art of injecting the blood-vessels with wax; for his countryman and contemporary Ruysch learned this method of him. Having neglected to improve his finances by the exercise of his profession as a physician, he was much straitened in his circumstances; and some years before his death, which happened in 1680, he became a prey to melancholy and superstition. His collection of insects and other objects belonging to natural history, for which the Grand Duke of Florence once offered him 12,000 florins, was sold for a very inconsiderable sum. We shall have occasion to notice one or two communications of his in the subsequent volumes of the Transactions.

the lungs the use of refrigeration; which he pretends to have asserted himself by most evident experiments and uncontrolable reasons.

To represent distinctly what he undertakes to make out in this tract, we may take notice of these particulars:

1. He takes pains to refute the doctrine of attraction, and to substitute in its place the doctrine of pulsion or intrusion of air into the lungs.

2. He endeavours to assert that the lungs do not fall down, but are by the breast contracted.

3. He affirms to have clearly shown what is the proper function and work of the diaphragm and other muscles serving for respiration.

4. He pretends to have experimentally evinced the genuine use of respiration, and the benefit thence resulting to the animal life.

In short, he makes respiration to be a motion of the thorax and lungs, whereby the air is sometimes impelled by the nose, mouth, and wind-pipe into the lungs, and thence again expelled, farther to elaborate the blood, by refrigerating it and by separating its fuliginous steams, and to raise it to its ultimate and highest perfection for the conservation of the life of animals.*

III. Observations faites sur un Grand Poisson et un Lion, dissequés dans la Bibliotheque du Roy à Paris, Juin 1667. Observations made upon a large Fish and a Lion, dissected in the King's Library at Paris, in June 1667.

The large fish dissected at Paris as above-mentioned, was a vulpecula marina or sea-fox;† concerning which it was observed:

1. The length of his tail equalled very near the whole length of the rest of his body, (the whole fish being 8½ feet long) and fashioned after the manner of a scythe, bowed and turned up toward the belly.

2. That his mouth was armed with two sorts of teeth; one sort in the upper jaw, being pointed, hard and firm, and of one only bone in the manner of a saw: the other sort found in the rest of the upper and in the whole under jaw were moveable, and fastened by fleshy membranes.

3. That his tongue did altogether adhere to the lower jaw, and its skin was hard and covered with little shining points, which rendered it very rough and scabrous one way. The points, viewed with a microscope, appeared transparent like crystal.

4. That his throat was very large, and the œsophagus as large as his maw; concerning which authors say, that he has the dexterity of disengaging himself

* The process of respiration is better understood in these days, as we shall have an opportunity of noticing in a future volume.

† Chimæra monstrosa, Linn.

from the swallowed hook, by casting it up together with his maw, the inside of it turned out. They found in his maw the sea-herb varec, 5 inches long, and a fish of the like length without head, scales, skin and guts, all being wasted but the musculous flesh, which remained entire.

5. That the superior part of his great gut had this peculiar, that instead of the usual circumvolutions of guts, the cavity of this was divided transversely by many partitions, consisting of the membranes of the gut turned inwards, and in the figure of a vice, like snail-shells or winding stairs.

6. That his spleen was double; his liver divided into two lobes; the gall found to have more of bitter than sour; the heart without a pericardium, as large as a hen's egg; the head almost nothing but a mass of flesh, very little brains in it, and those having very few meanders or windings; the eyes larger than those of an ox, only half spherical, flat before; the sclerotica formed like a cup, very thin, but very hard; the cornea very tender and soft; the crystalline perfectly spherical; the uvea grayish; the choroides of the same colour, and pierced for the production of the retina, by a very large hole; the bottom of this choroides had that lustre of mother of pearl which is found in terrestrial animals, but with less vivid colours; and the retina was also streaked with very apparent sanguineous vessels.

Concerning the lion it was observed,

That in its outward shape, and in the constitution of many parts, this animal resembles very much a cat.*

That there was an admirable structure of his claws; a peculiar shape and position of his teeth; a very stiff neck; a rough and sharp tongue, having points like claws both in hardness and shape. Eyes very clear and bright, even after death, which without closing the eye-lids, lions can cover with a thick and blackish membrane,† placed towards the great angle, which by raising itself and reaching towards the small angle, can extend itself over the whole cornea, as in birds, and especially in cats; the reverse of the anterior uvea, where it lies over the crystalline, is altogether black; the crystalline very flat, and its greatest convexity, which is not usual, in its anterior part, as in cats; the aqueous humour very plentiful, equalling almost the sixth part of the vitreous, which plenty was judged to be the cause of the brightness that remains in the eyes after death.

His throat was not above an inch and a half large: the stomach 6 inches wide, and 18 inches long; all the guts 25 feet long; the liver divided into 7

* Hence in the Linnæan system the lion and the cat are congeneric species.

† Membrana nictitans.

lobes, as in cats; its cavity under the bladder of gall was full of gall, shed abroad in the substance of the liver, and of the neighbouring parts; which was suspected, by the physicians administering this operation, to have been the cause of this lion's death; the bladder of gall was seven inches long, and 1½ inch wide, of a peculiar structure; the spleen a foot long, 2 inches broad, and half an inch thick; the kidney weighed somewhat above 7 ounces; the genitals of a peculiar conformation, causing this animal to cast his urine backwards, and to couple like camels and hares.

His lungs had six lobes on the right side, and three on the left; the windpipe had its annular cartilages entire, excepting two or three; it was above four inches in compass, being very firm, and by this largeness and firmness enabling a lion strongly to push air enough through it for his dreadful roaring.

His heart was dry, and without water in the pericardium, much greater in proportion than of any other animal, being six inches long, and four inches thick towards the basis, and terminating in a sharp point. It had very little flesh, and was all hollow; the ventricles very large; the auricles very small; the proportion of the branches which the ascending aorta casts out was such, that the carotids were as large as the left subclavian branch, and as the rest of the right subclavian whence they issue, which is considerable, seeing the brain is so small; for the brain was but two inches big, the rest of the head being very fleshy, and consisting of very firm bones. By comparing the little quantity of the lion's brain with the plenty of that of a calf, it was judged, that the having but little brain is rather a mark and a cause of a fierce and cruel temper than want of wit. Which conjecture was strengthened by the observation formerly made in the sea-fox, in whom almost no brain was found, though it be thought that his craft and address have occasioned men to give him that name.

IV. Historia Ambræ, Authore Justo Klobio, D. in Academ. Wittebergensi. This author reckons up 18 opinions concerning ambergris; and having examined every one of them, he embraces that which states it to be the dung of a bird, called in the Madagascar tongue Aschibobuch: of which he gives the description out of Odoardus Barbosa and others; who affirm it to be of the size of a goose, curiously feathered, with a large head well tufted. These birds being found in great numbers in Madagascar, the Maldives, and other parts of the East Indies, are affirmed by authors to flock together in great numbers, as cranes; and frequenting high cliffs near the sea side, and there voiding their excrement, the sea washes it thence, if it fall not of itself into it.

There is another opinion among the said 18, for which the author has a good inclination, but yet dares not embrace it, viz. that it is the excrement of a certain kind of whales. If this amber were but in those other places where there is good store of such whales, it seems that would make the author relinquish the former opinion.

An Account of an Experiment, made by Mr. HOOK, of preserving Animals alive by blowing into their Lungs with Bellows. N° 28, p. 539.

I did heretofore give this illustrious Society an account of an experiment I formerly tried of keeping a dog alive after his thorax was all displayed by the cutting away of the ribs and diaphragm; and after the pericardium of the heart also was taken off. But divers persons seeming to doubt of the certainty of the experiment (by reason that some trials of this matter made by some other hands failed of success) I caused at the last meeting the same experiment to be shown in the presence of this noble company, and that with the same success as it had been made by me at first; the dog being kept alive by the reciprocal blowing up of his lungs with bellows, and then suffered to subside, for the space of an hour or more, after his thorax had been so displayed, and his aspera arteria cut off just below the epiglottis, and bound on upon the nose of the bellows.

And because some eminent physicians had affirmed, that the motion of the lungs was necessary to life, upon the account of promoting the circulation of the blood, and that it was conceived the animal would immediately be suffocated as soon as the lungs should cease to be moved, I did (the better to fortify my own hypothesis of this matter, and to be the better able to judge of several others) make the following additional experiment, viz.

The dog having been kept alive, (as I have now mentioned) for above an hour, in which time the trial had been often repeated, in suffering the dog to fall into convulsive motions by ceasing to blow the bellows, and permitting the lungs to subside and lie still, and of suddenly reviving him again by renewing the blast, and consequently the motion of the lungs: This, I say, having been done, and the judicious spectators fully satisfied of the reality of the former experiment, I caused another pair of bellows to be immediately joined to the first, by a contrivance I had prepared, and pricking all the outer coat of the lungs with the slender point of a very sharp penknife, this second pair of bellows was moved very quick, whereby the first pair was always kept full and always blowing into the lungs; by which means the lungs also were always kept very full, and without any motion; there being a continual blast of air forced into the lungs by the first pair of bellows, supplying it as fast as it could find its way quite through the coat of the lungs by the small holes pricked in it, as was said before. This being continued for a good while, the dog, as I expected, lay still as before, his eyes being all the time very quick, and his heart beating very regularly: But upon ceasing this blast, and suffering the lungs to fall and lie still, the dog would immediately fall into dying convulsive fits; but he as soon revived again by the renewing the fulness of his lungs with the constant blast of fresh air.

Towards the latter end of this experiment a piece of the lungs was cut quite off; where it was observable that the blood did freely circulate and pass through the lungs, not only when the lungs were kept thus constantly extended, but also when they were suffered to subside and lie still. Which seem to be arguments, that as the bare motion of the lungs without fresh air contributes nothing to the life of the animal, he being found to survive as well when they were not moved as when they were; so it was not the subsiding or movelessness of the lungs that was the immediate cause of death, or the stopping the circulation of the blood through the lungs, but the want of a sufficient supply of fresh air.

I shall shortly further try whether the suffering the blood to circulate through a vessel, so as it may be openly exposed to the fresh air, will not suffice for the life of an animal; and make some other experiments, which I hope will thoroughly discover the genuine use of respiration; and afterwards consider of what benefit this may be to mankind.*

Description of Mr. Gascoigne's Micrometer.† *By Mr. Hook.* *N° 29, p. 541.*

In pl. 6, the fig. 1, 2, 3, represent the several parts of this instrument; fig. 4, part of the telescope with the instrument applied to it, and 5 the rest on which the whole is supported.

Fig. 1 represents the brass box with the whole instrument, except only the moveable cover, and the screws by which it is fixed to the telescope. In this fig. *a a a a*, is a small oblong brass box, serving both to contain the screws, and their sockets or female screws, and also to cause all the several moveable parts of the instrument to move very true, smooth, and in a simple direct motion. To one end is screwed on a round plate of brass, *b b b b*, about three inches over; the extreme limb of its outside being divided into 100 equal parts, and num-

* The fact established by this experiment is of the utmost importance in physiology and the practice of physic, viz. that the mere mechanical action of the lungs is not sufficient for the support of life; which ceases unless these organs (the lungs) be duly supplied with *fresh air*. On a knowledge of this fact is founded the treatment of apparent death in new-born infants and in drowned persons.

† This instrument was before mentioned in No. 25, p. 161.

bered by 10, 20, 30, &c. Through the middle of this plate, and the middle of the box *a a a*, is placed a very curiously wrought screw, about the size of a goose quill, and of the length of the box, the head of which is, by a fixed ring or shoulder on the inside, and a small springing plate *d d* on the outside, so adapted to the plate that it is not in the least subject to shake. The other end of this screw is, by another little screw (whose small point fills the centre or hole made in the end of the longer screw for this purpose) rendered so fixed and steady in the box, that there appears not the least danger of shaking. On the head of this screw, without the springing plate, is put on a small index *e e*, and above that a handle *m m*, to turn the screw round as often as there shall be occasion, without at all endangering the displacing of the index; it being put on very stiff on a cylindrical part of the head, and the handle on a square. The screw has that third part of it which is next the plate larger than the other two thirds of it, by at least as much as the depth of the small screw made on it: the thread of the screw of the larger third is as small again as that of the screw of the other two thirds. To the thicker screw is adapted a socket *f*, fastened to a long bar or bolt *g g*, on which is fastened the moveable sight *h*, so that every turn of the screw moves the sight *h* either a thread nearer or a thread farther off from the fixed sight *i*. The bar *g g* is made exactly equal, and fitted into two small staples *k k*, which will not admit of any shaking. There are 60 of these threads, and answerable thereto are made 60 divisions on the edge of the bolt or ruler *g g*; and a small index *l*, fixed to the box *a a a*, denotes how many threads the edges of the two sights *h* and *i* are distant; and the index *e e* shows, on the circular plate, what part of a revolution there is more; every revolution, as was said before, being divided into 100 parts. At the same time that the moveable sight *h* is moved forwards or backwards, or more threads of the coarser screw, is the plate *p p*, in fig. 2, by the means of the socket *q*, to which it is screwed, moved forward or backward, or more threads of the finer screw: so that this plate being fixed to the telescope by the screws *r r* in fig. 2, so as the middle between the sights may lie in the axis of the glass, however the screw be turned, the midst between the sights will always be in the axis, and the sights will equally either open from it or shut towards it.

Fig. 2 represents the moveable cover containing the screws to be fitly placed on fig. 1; according to the taking off, as it were, or folding up of this cover, the inward contrivance of the screws and sights may appear.

And because it is conceived by some ingenious men, that it will be more convenient, instead of the edges of the two sights *h* and *i*, to employ two sights fitted with hairs, therefore is added fig. 3, representing the two sights *r* and *s*, so fitted with threads *t* and *u*, that they may be conveniently used instead of the solid edges of the sights *h* and *i*.

The 4th figure represents how the screws are to be put on. The tube A D is divided into three lengths; of which B C is to lengthen or contract as the object requires; but A B is here added, that at A there may be put on such eye-glasses as shall be thought most convenient, and, to set them always at the distance most proper for them, indexes or pointers, which here are supposed to be at B; which length alters also in respect of divers persons' eyes. E is a screw by which the great tube can be fixed so as by the help of the figures any smaller part of it can immediately be found, measuring only or knowing the divisions on B C, the distance of the object glass from the pointers. F is the angular piece of wood that lies on the upper screw of the stand,* which is represented by figure 5.

For a description of the uses of this ingeniously contrived and very curious engine, see No. 25, p. 161.

An Account of making a Dog draw his Breath exactly like a Wind-broken Horse. By Dr. Richard Lower.† *N° 29, p. 544.*

After I had often considered the manner and way of respiration, and by many observations been induced to believe that the diaphragm is its chief organ, I thought there could be no better way to try it, than by dividing the nerves, by which its motion is performed; which may be easily done after the following manner:

First, pierce the side of the animal between the 6th and 7th rib in the middle of the thorax, just opposite the region of the heart, with a small incision-

* This rest (by Mr. Hook's suggestion) may be rendered more convenient, if, instead of placing the screw horizontal, it be so contrived that it may be laid parallel to the equinoctial, or to the diurnal motion of the earth. For by that means the same thing may be performed by the single motion of one screw, which in the other way cannot be done but by the turning of both screws.

† Richard Lower was one of the best anatomists of the 17th century. He was educated at Oxford, took his degree of M. D. in that university, and exercised his profession there for some years; but at length removed to London, where he got into extensive practice. He and Dr. King appear to have been the first who performed the experiment of the transfusion of blood. Besides several papers inserted in the Phil. Trans. he wrote a treatise, which procured him a great and deserved renown, De Corde, item de motu et Colore Sanguinis et Chyli in eum transitu, 1669. Among other things in this treatise, he pointed out the difference between arterial and venous blood, proving that the florid colour of the arterial blood is derived from the air. Lower was very warm in his attachments, and when his friend Willis's Theory of Fevers was attacked by De Meara, he wrote a Latin treatise in defence of it. In the latter part of his life he sided with the political party in opposition to the court; a line of conduct which proved detrimental to his professional interests.

knife, passing the knife but just into the cavity of the breast (which you may know by finding no resistance to the point of it;) then take it out, and put in a director or a small quill made like it, and thrust it in about an inch, directing the end of it toward the sternum close to the inside of the breast. Then cut upon it about an inch on the intercostal muscles; by which you may be secured from touching the lungs with the point or edge of your knife. This done, put in your finger, and with your nail separate the nerve which passes along the side of the pericardium toward the diaphragm. Then put in a probe, a little inverted at the end like a hook, and lay hold of the nerve and pull it to the orifice of the breast, and cut it off, and sew the hole up very close. Do the same on the other side, and presently let the dog loose, and you will plainly see him draw his breath exactly like a wind-broken horse. Which yet you will see plainer if you run him a little in a string after he is cut. But that any one may perform this experiment the easier, let him first take notice how the nerves of the diaphragm pass along on each side of the pericardium in a dead animal, before the trial be attempted in a living one.

The most obvious remarks upon this experiment are:

1. That the whole manner of respiration is quite altered. For, as in a sound animal, in inspiration the belly swells by the lifting up the bowels by the contraction of the diaphragm; and in exspiration the belly falls by the relaxing of the same: in a wind-broken dog or horse it is quite contrary; for in them it is to be seen plainly, that when they draw their breath, their belly is drawn in very lank and small, and when they breathe up, their belly is relaxed and swells again.

2. It being certain that the lungs do not move of themselves at all, but wholly depend upon the expansion of the thorax by the intercostal muscles and the diaphragm; by this experiment it appears how much the single motion of either of them particularly contributes to respiration. For all inspiration being made by the dilatation of the thorax, and that dilatation being caused partly by the intercostal muscles drawing up the ribs, and partly at the same time the diaphragm by its contraction drawing downward the lower small ribs, to which it is joined, and also lifting up the viscera of the lower belly, by which they jointly make all the space they can, for the air to come in and distend the lungs: it must hence necessarily follow, that the intercostal muscles and the diaphragm being constituted for two distinct employments (though both to the same end) and neither being able to perform the other's office, where one ceases from its work, the other for the exigence of nature must take more pains to supply the defect of the former. Which is very evident to be seen; for the diaphragm being made useless by losing its nerves, the intercostal muscles

dilate the ribs much more than formerly, even to the utmost distance they can when there is need of it; as appears when you make the dog run a little after he is cut, or when you gallop a wind-broken horse.

3. The manner of respiration being the same in a dog, whose diaphragmatic nerves are cut, and in a wind-broken horse, it is more than probable, that the cause may be as nearly the same, and that, though there may be other faults found in the lungs of such creatures, yet it is very likely they may be induced from the weakness of respiration, but that they had their occasion from the relaxation or rupture of the nerves of the diaphragm at first: which will seem more credible, if we remember, that by the straining the midriff too much (by which the nerves may be quite broken or stretched beyond their proper tone) most commonly that accident happens.

Anatomical Observations on a human Body, dead of odd Diseases. By Dr. Nathaniel Fairfax. N° 29, p. 546.

A young woman, after experiencing repeated attacks of pulmonary and febrile complaints, died in the 24th year of her age. Two or three years before her death she "heard a frightful jolting in her breast," [chest] whence it was suspected there was a collection of water in that cavity. She had great dyspnœa, and never could lie on her left side. Several remedies were tried, but without relief. The winter in which she died she caught a fresh cold, and had symptoms of pneumonia, which terminated fatally in a few days. On opening the thorax, Dr. Fairfax was surprised to see (as he thought) almost its whole cavity empty above (as the body lay supine) and filled with nothing but a thick milky fluid beneath; but searching farther, he found there was only all the right side of the chest, and about a third part of the left in that condition. It took up in the part toward the neck a hand-breadth, and ran three fingers thickness to the left of the mediastinum. The liquor was like cream, or rather like a size of a Spanish white, having a cast of yellow like beestings. For, putting a spoon into it, from the bottom he took up a thick clammy matter just like that Spanish white that sinks to the bottom of its size. In quantity it might be about three pints, contained in a bag which was capable to hold as much more and better. The bag ran along from the left shoulder to the extremity of the right side of the midriff: not straight along nor stiffly stretched; but about a hand-breadth from its rise it went directly down to the midriff, with which it closed all along. Its skin or coat was thicker than that of the stomach, as well as its capacity larger, inasmuch as the flexures of the ribs joined with it, and made up above half the compass. Where it adhered to the midriff it was near a finger thick; and in one place, where he endeavoured to separate it from the midriff, he hit upon a thinner bag, whence issued out two or three spoonfuls of clear water. The mediastinum was either wholly wasted or else woven into the thickness of the bag, as was also the pleura, as far as the bag reached. It lay loose and flapping from the left axilla to the chest, having been before filled and distended either with lenid or the liquor. All the hollow was wet with the liquor, as that lobe of the lungs which should have been on the right side was gone, and that on the left wasted to near a third part. In the lower belly all was well.*

Two other Anatomical Observations. By the same Author. N° 29, p. 548.

In the first of these observations it is stated, that a servant-man, of a dull melancholy disposition, who died at the age of 27, after having been in a dropsical state the four preceding years, was found, on opening the body, to have the left lobe of the lungs almost quite wasted, but without any ulcer. The lobe of the liver which rests on the diaphragm was black outwardly for about a hand's breadth, and a thumb's breadth within the parenchyma. Other parts sound.

The second observation mentions, that in the body of a felon, he noticed that the right vas præparans (arteria spermatica) sprang clearly from the right emulgent; whereas ordinarily the vas præparans arises on the *right* side out of the aorta, as on the *left* out of the emulgent.

Divers Instances of Peculiarities of Nature, both in Men and Brutes. By the same. N° 29, p. 549.

1. An asthmatic patient being advised to take a spoonful of honey, was immediately affected with an universal swelling, as if he had swallowed poison. It being suspected that something hurtful might have been mixed with the honey, a fresh quantity was procured from another place, which however on being swallowed produced the very same effect.

2. A clergyman of Metigham in Suffolk, about 40 years of age, who was accustomed to drink *warm* or rather *hot* beer, riding out in Midsummer, stopped at a house at some distance from home, and was offered a cup of *cold* beer, which he drank; and then got on his horse and rode on; but soon fell sick, and afterwards vomited. By the time he got to his journey's end, his vomiting in-

* This appears to have been a large abscess formed between the pleura and the ribs, and between the pleura and the muscular substance of the diaphragm. The bag or cyst was the pleura itself, which invests these and other parts of the cavity of the chest.

creased, and he was obliged to go to bed. The next day he grew worse, could find no help from physic, and died the following morning.

3. This observation merely contains an instance of great timidity in a female mind. A lady had a great dread of wasps, and confined herself to her room "whilst their season of swarming about in houses lasted."

4. In another lady, the dread excited by thunder operated so powerfully as to produce fainting, vomiting and diarrhœa, in short all the symptoms of cholera.

5. A woman affected with chlorosis had a longing to suck the wind out of bellows, which, as often as she could, she received with open mouth, forcing the air in by blowing with her own hands the bellows inverted.—Another was fond of crackling cinders under her feet.

6. Somewhat like this is to be found in brutes. In May last a greyhound bitch at Brightwell-Hall, about five or six days before she cast her whelps, had such a wild kind of hunger (though she was fed sufficiently every day with usual food) that finding another bitch's whelps, she devoured them all (four or five as I remember) and fell next upon the bitch herself, who made a shift to get from her as well as she could, being helped. From this, and from sows devouring whole litters of pigs, I am prone to think otherwise of the longings of pregnant women than is the common opinion.

A Confirmation of the Experiments mentioned in N° 27, (p. 170 of this Abridgement) to have been made by Sig. FRACASSATI in Italy, by injecting Acid [and other] Liquors into the Blood. By the Hon. BOBERT BOYLE, in a Letter to Mr. OLDENBURG, dated Oxford, Oct. 19, 1667. N° 29, p. 551.

SIR,

I hinted to you in my last something about the original of the experiments made in Italy, by injecting acid liquors into blood: To explain which, I shall now tell you, that about this time three years I mentioned at Gresham College to the Royal Society an odd experiment I had formerly made upon blood yet warm, as it came from the animal, viz. That by putting into it a little aqua-fortis or oil of vitriol, or spirit of salt, the blood not only would presently lose its pure colour and become of a dirty one, but instantly be also coagulated; whereas if some fine urinous spirit, abounding in volatile salt, such as the spirit of sal ammoniac, were mingled with the warm blood, it would not only not curdle it, or imbase its colour, but make it look rather more florid than before, and both keep it fluid and preserve it from putrefaction for a long time.

This experiment I devised, among other things, to show the amicableness of volatile spirits to the blood. And I remember it was so much taken notice of that some very inquisitive members of the Society came presently to me, and desired me to acquaint them more particularly with it; which I readily did, though afterwards I made some further observations about the same experiment that I had no occasion to relate.

This having been so publicly done, though I shall not say that Signior Fracassati may not have hit as well as I upon the experiments published in his name, yet there is so little difference between the warm blood of an animal out of his veins and in them, that it is not very improbable that he may have had some imperfect rumour of our experiment without knowing whence it came, and so may, without any disingenuity, have thence taken a hint to make and publish what now appears in the Transactions. If it be thought fit that any mention be made of what I related so long since, I think I can send you some other circumstances belonging to it. For I remember I tried it with other liquors (as spirit of wine, oil of tartar, oil of turpentine,) and I think also I can send you some remarks upon the colour of the upper part of the blood.

An Observation concerning the Epiploon, or Double Membrane, which covers the Entrails of Animals, and is filled with Fat. By* MALPIGHI. *N° 29, p.* 553.

The epiploon, looked at through a good microscope, is like a great sack, full of abundance of other small sacks, which inclose gatherings of grease or fat. There are many vessels, which may be called *adipous* or *fatty*, which, issuing out of this membrane, and spreading themselves all over the body, convey fat to it, just as the arteries carry the blood all over the same.† Where-ever is fat or grease there is found store of these little sacks, wherein that is inclosed, whence it is, that in lean and emaciated bodies, instead of fat you find nothing but skins.

The structure of these small sacks and of the adipous vessels sufficiently shows that the fat is not formed accidentally out of the thick vapours of the blood, as is the common belief. Nor is its chief use to foment the natural heat, but it seems rather to conduce to the allaying of the acrimony of the salts

* This observation should have been added to those inserted in No. 27 of this Abridgement, since it is contained in the Exercitatio de Omento annexed to the Tetras Anatom. Epist. M. Malpighi.

† What are here termed adipous vessels issuing out of the substance of the omentum, and terminating in the sacks or cells wherein the fat is lodged, are nothing more than arterial ramuli, from the extremities of which it would appear that the fat is deposited by transudation; for anatomists have never detected a truly glandular apparatus for the secretion of the adipous fluid.

that are in the blood and the serosities. And indeed lean persons, and those whose epiploon has been cut, are more subject than others to rheumatisms, lienteries, and the like diseases that are caused by the sharpness of the humours. And those that are fat are not so easily seized on by them, because the acrimony of the serosities is corrected by the mixture of the fat, just as the sharpest lixivium will lose its force if oil be mingled therewith.*

An Account of the Experiment of Transfusion, performed in London Nov. 23, 1667, upon the Person of Arthur Coga, at Arundel House, in the Présence of many considerable and intelligent Spectators, under the Management of Dr. RICHARD LOWER *and Dr.* EDMUND KING; *by the latter of whom the Relation was drawn up. N° 30, p. 557.*

The experiment of transfusion of blood into a human vein was made by us in this manner: Having prepared the carotid artery in a young sheep, we inserted a silver pipe into the quills to let the blood run through it into a porringer, and in the space of almost a minute about 12 ounces of the sheep's blood ran through the pipe into the porringer, which was somewhat to direct us in the quantity of blood now to be transfused into the man. Which done, when we came to prepare the vein in the man's arm, the vein seemed too small for that pipe which we intended to insert into it; so that we employed another about one third part less at the little end. Then we made an incision in the vein, after the method formerly published, No. 28, which method we observed without any other alteration, but in the shape of one of our pipes, which we found more convenient for our purpose. And having opened the vein in the man's arm with as much ease as in the common way of venesection, we let thence run out six or seven ounces of blood. Then we planted our silver pipe into the said incision, and inserted quills between the two pipes already advanced in the two subjects, to convey the arterial blood from the sheep into the vein of the man. But this blood was near a minute before it had passed through the pipes and quills into the arm; and then it ran freely into the man's vein for the space of two minutes at least; so that we could feel a pulse in the said vein just beyond the end of the silver pipe; though the patient said he did not feel the

* In many species of cachexy there is a rapid absorption and consumption of fat, whereby the acrimony existing in the fluids is considerably blunted, and the irritation on the nervous and arterial systems is proportionably lessened.

blood hot (as was reported of the subject in the French experiment) which may very well be imputed to the length of the pipes through which the blood passed, losing thereby so much of its heat, as to come in a temper very agreeable to venal blood. And as to the quantity of blood received into the man's vein we judge there was about nine or ten ounces: For, allowing this pipe $\frac{1}{3}$ less than that through which 12 ounces passed in one minute before, we may very well suppose it might in two minutes convey as much blood into the vein as the other did into the porringer, in one minute, granting withal that the blood did not run so vigorously the second minute as the first, nor the third as the second, &c. But we conceive that the blood ran during the whole of those two minutes: First, because we felt a pulse during that time: Secondly, because when, upon the man's saying he thought he had enough, we drew the pipe out of his vein, the sheep's blood ran through it with a full stream; which it had not done, if there had been any stop before in the space of those two minutes; the blood being so very apt to coagulate in the pipes upon the least stop, especially the pipes being so long as three quills.

The man after this operation, as well as in it, found himself very well, and has given in his own narrative under his own hand, enlarging more upon the benefit which he thinks he has received by it, than we as yet think fit to own. He urged us to have the experiment repeated upon him within three or four days after this; but it was thought advisable to put it off somewhat longer. And the next time we hope to be more exact, especially in weighing the emittent animal before and after the operation, to have a more just account of the quantity of blood it shall have lost.

A Relation of some Trials of Transfusion lately made in France. By Mr. OLDENBURG. *N° 30, p.* 559.

1. Mr. Denys, Professor of Mathematics and Natural Philosophy at Paris, states in a letter to the publisher, that they had lately transfused the blood of four wethers into a horse 26 years old, and that this horse had thence received much strength, and more than ordinary appetite.

2. The same person was pleased to send to the same hand a printed letter written to the Abbot Bourdelot, by M. Gadroys, being an answer to a paper of one M. Lamy. In this answer the author vindicates the experiments of transfusion from the objections that have been urged against them.

Some new Experiments of Injecting medicated Liquors into Veins, together with [an Account of] considerable Cures performed thereby. Communicated by Dr. FABRITIUS, *of Dantzick. [Translated by Mr.* OLDENBURG *from the original Latin.] N° 30, p. 564.*

As we had a great desire to try what would be the effect of the surgical experiment of injecting liquors into human veins, three fit subjects presenting themselves in our hospital, we thought good to make the trial upon them. But seeing little ground to hope for a manifest operation from merely altering medicines, we thought the experiment would be more convenient and conspicuous from laxatives; which made us inject by a syphon about two drams of such a kind of physic into the median vein of the right arm. The patients were these: One was a lusty robust soldier dangerously infected with the venereal disease, and suffering grievous exostoses of the bones in his arms. He, when the purgative liquor was infused into him, complained of great pains in his elbows, and the little valves of his arm swelled so visibly that it was necessary by a gentle compression of one's fingers to stroke up that swelling towards the patient's shoulders. About four hours after it began to work, not very troublesomely; and so it did the next day, insomuch that the man had five good stools after it. Without any other remedies, those protuberances were gone, nor are there any traces left of the above-mentioned disease.*

The two other trials were made upon the other sex. A married woman of 35, and a servant maid of 20 years of age, had been both of them from their birth very grievously afflicted with epileptic fits, so that there were little hopes left to cure them. They both underwent this operation, and there was injected into their veins a laxative rosin, dissolved in an anti-epileptic spirit. The first of these had gentle stools some hours after the injection, the next day the fits recurred now and then, but much milder, and are since altogether vanished. As for the other, viz. the maid, she went the same day to stool four times, and several times the next; but by going into the air, taking cold, and being careless in her food, she died.

It is remarkable that all three vomited soon after the injection, and that excessively and frequently.

* That venereal exostoses should be thus removed in the short space of two days, appears a most extraordinary fact. Might not the supposed "protuberances of the bones" have been tumors formed by obstructions taking place in the lymphatic vessels? In that case their sudden disappearance after the cathartic operation of the injected liquor will cease to excite astonishment.

Of the Tides, of Wells, Salt and Fresh Water; and of the Whale Fishing, &c. at Bermudas. By RD. NORWOOD.* *N° 30, p. 565.*

Concerning the tides, I have only taken a general notice of them; as that it is high water about 7 o'clock on the change day; in some creeks an hour or two later. The water rises but little, as about 4 feet at a high water; but at the spring-tides it may be a foot more. The tides without, are very various in their setting. Sometimes the tide of flood sets to the eastward, sometimes to the westward; but in fair, calm and settled weather, the tide sets from the south-east toward the north-west.

We dig wells of fresh water sometimes within 20 yards of the sea or less, which rise and fall with the tide; and so do most of the wells in the country, though further up, as I am informed. Wherever they dig wells here, they dig till they come almost to a level with the superficies of the sea, and then they find either fresh water or salt. If it be fresh, and if they then dig 2 or 3 feet deeper, and often less, they come to salt water. If it be a sandy ground, or a sandy crumbling stone that the water soaks gently through, they find usually fresh water; but if they be hard lime-stone rocks, which the water cannot soak through, but passes in chinks or clefts between them, the water is salt or brackish. Yet I never saw any sand in the country such as will grind glass, or whet knives, &c. as in England, but a substance like sand, though much softer; neither have we any pebble-stones or flint.

For the killing of whales, it has been formerly attempted in vain, but within these two or three years in the spring time and fair weather they take sometimes one, or two, or three in a day. They are smaller it seems than those in Greenland, but more quick and lively, so that if they be struck in deep water, they presently plunge with such violence, that the boat is in danger of being hauled down after them, if they cut not the rope in time. Therefore they usually strike them in shoal water. They have very good boats for that purpose, manned with six oars, such as they can row forwards or backwards as occasion requires. They row up gently to the whale so as he can scarcely avoid them; and when the harpooner, standing ready fitted, sees his opportunity, he strikes his harping iron into the whale, about or before the fins, rather than

* Mr. Norwood was a teacher of mathematics, particularly navigation, in which it seems he had some practice. He published several books; as, The Epitome and Doctrine of Triangles; Trigonometry; and the Seaman's Practice, where is found that for which he has been mostly noted, viz. his determination of the magnitude of the earth, and the degrees of the meridian, by means of the distance measured between London and York.

toward the tail. The harping irons are like those which are usual in England in striking porpoises, but of excellent metal that will not break, but bend about a man's hand. To the harping iron is made fast a strong flexible rope, and into the socket of that iron is put a staff, which, when the whale is struck, comes out of the socket; and when the whale is something quiet, they haul up to him by the rope, and so strike into him another harping iron, or lance him with lances in staves till they have killed him. I hear not that they have found any spermaceti in any of these whales; but I have heard from credible persons, that there is a kind of such as have the sperma at Eleutheria, and others of the Bahama Islands, where also they often find quantities of ambergris, and that those have great teeth, which ours have not, and are very sinewy.

To find the Number of the Julian Period, by M. de Billy's *Method; with the Demonstration of that Method. By Mr.* John Collins,* *F. R. S. N° 30, p. 568.*

This method was given in these Transactions, No. 18, and is thus:

Multiply the {Solar / Lunar / Indiction} Cycle by {4845 / 4200 / 6916} Then divide the sum of the products by 7980, the Julian period; the remainder of the division, without having regard to the quotient, will be the year required. Concerning the demonstration, Mr. John Collins, now a member of the Royal Society, communicated what follows, viz.

That the Julian period is a basis whereon to found chronology not liable to controversy, as the age of the world is; and it is the number abovesaid, to wit 7980, which is the product of the 28th solar cycle, 19th lunar, 15th indiction.

The problem may be thus proposed generally:

Any number of divisors, with their remainders after division, being proposed, to find the dividend.

This is thus proposed in this general manner, but is no new problem, and was

* An eminent accountant and mathematician, born near Oxford in 1624. Besides many papers in the Phil. Trans. he published a number of useful works on a variety of subjects, mathematics, navigation, astronomy, trade, commerce, &c. also editing the works of many other authors, which he procured to be printed, as those of Barrow, Brouncker, Pell, &c. Mr. Collins was also very useful to the Royal Society, &c. in conducting a literary correspondence with the most learned men of those times, both at home and abroad, as Barrow, Newton, Wallis, Leibnitz, &c. particularly in the famous dispute concerning the invention of Fluxions between Newton and Leibnitz, the letters passing chiefly through Collins's hands, and published in his name in 1712, under the title of Commercium Epistolicum. He died in 1683, at 59 years of age.

resolved long since by John Geysius, by the help of particular multipliers, such as those above mentioned, and published by Alsted in his Encyclopædia, An. 1630, and by Van Schooten in his Miscellanies.

We shall clear up what authors have omitted concerning the definition and demonstration of such fixed multipliers, &c. And therefore say, that each multiplier is relative to the divisor to which it belongs, and thus define it:

It is such a number as, divided by the rest of the divisors or their product, the remainder is 0, but, divided by its own divisor, the remainder is an unit.

We required the divisors proposed to be primitive to each other, *i. e.* that no two or more of them can be reduced to less terms by any common divisor. For if so, the question may be possible in itself, but not resolvable by help of such multipliers, such being impossible to be found. The reason is, because the product of an odd and an even number is always even, and that divided by an even number, leaves either nothing or an even number.

Divisors	28	The multipliers relative to them are	4845
	19		4200
	15		6916

The difinition affords light enough for the discovery of these numbers. To instance in the first; the product of 19 and 15 is 285, which multiply by all numbers successively, and divide by 28 till you find the remainder required. Thus twice 285 is 570, which divided by 28, the remainder is 10; also thrice 285 is 855, which divided by 28, the remainder is 15. Thus if you try on successively, you will find that 17 times 285, which is 4845, is the number required, which divided by 28, the remainder is an unit. Hence then we shall find that

4845	is equal to the solid or product of	19, 15, 17.
4200		28, 15, 10.
6916		28, 19, 13.

More easy ways of performing this postulatum are to be found in Van Schooten's Miscellanies, and Tacquet's Arithmetic.

For illustration of the rule proposed, take this example.

					Products.
In the year 1668, the	Solar cycle	25	The multipliers	4845	121125
	Lunar cycle	16		4200	67200
	Indiction	6		6916	41496
				The sum of the products	229821

which divided by 7980, the remainder is 6381, for the year of the Julian period; from which subtracting 709, there remains 5672 for the age of the world, according to archbishop Usher.

For demonstration of this rule we argue thus :

1. Each multiplier multiplied by its remainder, is measured or divided by its own divisor, leaving such a remainder as is proposed. For each multiplier was defined above to be a multiple of its own divisor, plus an unit. Therefore multiplying it by any remainder, it only renders it a greater multiple in the said divisor, plus an unit, multiplied by the remainder; which is no other than the remainder itself; but if 0 remains that product is destroyed.

2. The sum of the products divided by each respective divisor, leaves the remainder assigned. For concerning the first product, it is by the first section measured by its own divisor, leaving the remainder proposed; and if we add the rest of the products, we only add a multiple of its own divisor, which in division enlarges the quotient, but not the remainder. In particular the second multiplier is 28 × 15 × 10 × remainder, all which is but a multiple of 28. And so the third product is 28 × 19 × 13 × remainder.

And what has been said concerning the sum of the products, being divided by the first divisor, and leaving the remainder thereto assigned, may be said of each respectively.

3. The sum of the products divided by the solid of the three divisors, leaves a remainder so qualified as the said sum. For that sum, by the 2d article, is the first product increased by adding a just multiple of the first divisor, that thereby we only enlarge the quotient, not alter the remainder. By the like reason, the subtracting a just multiple, only alters the quotient, not the remainder; but the solid of all three divisors multiplied here by the quotient, as there by the remainder, is only a just multiple of the first divisor. Wherefore the remainder, after this division is performed, is of the same quality as the sum of the products, and divided by the first divisor, leaves the remainder proper thereto: and the like may be said concerning each divisor.

To find the year of the Julian period for any year of our Lord proposed: It is necessary to know the sun's cycle, the prime number, and the number of the Roman indiction, which Mr. Street performs by the following verse;

When 1, 9, 3, to the year have added been,
Divide by 19, 28, fifteen.

The remainders are the numbers sought, or the cycles and indiction required; as we found them for the year 1668, in the foregoing example.

The use of the prime is to find the epact, and thereby the moon's age, the time of high water, &c. A farther use of the sun's cycle is, to find the dominical letter, and thereby to know the day of the week on which any day of any month happens. But this is more easily obtained by finding on what day of the week the first of March happens for ever, according to such rules and verses as I have elsewhere published.

In brief thus:—To the number 2 add the year of our Lord, suppose 1669, and its even fourth part, neglecting what remains, if any, as 417; the sum 2088 divide by 7, noting the remainder, which shows the number of the day of the week, accounting Sunday first. If 0 remain the first of March falls on a Saturday. In this example there remains 2, showing the first of March to fall on a Monday. If it were required To perform this for years preceding our Saviour's nativity, then take this rule: To the year add its even fourth part, the sum divide by 7; the remainder shows the day of the week, accounting Sunday first, Saturday second, and so backwards.

To find what day of the month in the first week of each month happens to be on the same day of the week as the first of March. Use the following verses, in which the 12 words relate to the 12 months of the year, accounting March the first:

Ask endless comfort, God enough bestows,
From divine axioms faith confirmed grows.

The alphabetical number of the first letter of the word, proper to the month proposed, is the answer:

For example.—If the month were April, the word proper thereto is endless, and E is the fifth letter in the alphabet. Therefore conclude, that the first of March and fifth of April do for ever happen on the same day of the week.

To find on what day of the week the first day of each month happens. Supposing the first of March known, it might be reckoned from the former problem; but the following verse, beginning with March as the former, is more ready for the purpose:

A dreadful fire, beholders daily gaze,
Chastised England. Ah cruel fatal blaze!

Explication.—In the year 1669, the first of March is Monday; I would know on what day of the week the first of October happens. The word proper to the month is England; then count alphabetically to E, viz. A Monday, B Tuesday, C Wednesday, D Thursday, E Friday, which is the day sought. Whence conclude, that the 1st, 8th, 15th, 22d, 29th days of October are all Fridays. Thence it is easy to reckon on what day of the week any day of that month happened, and so for all other months.

To find on what day of the month the sun enters into any sign of the Zodiac. For this we give the following verse;

Charles brought content, divers effects ensue,
Envy, fear, dolour, danger, bid adieu.

Here again, the 12 words relate to the 12 months, March being the first. To the number of the letter of the alphabet, the word begins with, add 7. Example.—Fear is the word for October, and F the sixth letter; therefore the suns enters into the 8th sign, to wit Scorpio, on the 13th of October.

An Account of some Books. N° 30, *p.* 575.

I. Petri Lambeci Lib. Primus Prodromi Historiæ Literariæ, &c.
II. Thomæ Cornelii Consentini Progymnasmata Physica.
III. Les Essays Physiques du Sieur De Launay, Liv. premier.
IV. Francisci du Laurens Specimina Mathematica, duobus Libris comprehensa.

New Experiments concerning the Relation between Light and Air in shining Wood and Fish. By Mr. Boyle. N° 31, *p.* 581.

Exper. 1. On putting a piece of shining rotten wood, of the size of a groat* or less, into the receiver of a pneumatic engine or air-pump, and the pump being set to work, we observed not, during the five or six first exsuctions of the air, that the splendour of the included wood was lessened, but about the seventh suck it seemed to grow a little more dim, and afterwards, losing of its light more and more as the air was further pumped out, at length, about the tenth exsuction, we could not perceive any light at all to proceed from the wood.

Exper. 2. We let in the air again by degrees, and had the pleasure to see the seemingly extinguished light revive, so fast and perfectly, that it looked to us all almost like a little flash of lightning, and the splendour of the wood seemed rather greater than before it was put into the receiver. On including the wood in a very small receiver of clear glass, it was found that in this the light would begin to grow faint at the second or third exsuction of the air, and at the sixth or seventh would quite disappear.

Exper. 3. To discover whether this luminousness of the wood would more resemble a coal, or the life of a perfect animal, in being totally and finally extinguished in case the air were kept from it a few minutes, or else the life of insects, which in the exhausted receiver I had observed to lose all appearance of its continuing, and that for a much longer time than a few minutes, and yet

* A silver coin then in use, rather less than a sixpence.

afterwards, upon the restitution of air, to recover presently, and show manifest signs of life; having therefore exhausted the receiver, till the wood quite disappeared, we stayed above a quarter of an hour in the dark, without perceiving that the wood had regained any thing of light, though about the end of this time we made the place about it as dark as we could; and then, it being too late at night to protract the experiment, we let in the air, and on the admission the wood presently recovered light enough to be conspicuous at a distance, though it seemed to me somewhat less vivid than before.

The following night we put in a piece of wood larger than the former, being above an inch long, which shone very bright. And having by a few exhaustions quite deprived it of light, we left it in the exhausted receiver for full half an hour, then coming into the dark room again, we found that some small portion of air had insinuated itself into the receiver.

Exper. 4. Having observed that sometimes the operation, which the withdrawing the air has upon a body included in the receiver, proves more considerable some minutes after we have ceased pumping than it is immediately after, I imagined that even in such cases where the light is not made wholly to disappear by the emptying of the pneumatical glass, the suffering the body to remain a while there, though without any pumping, the remaining light of the body might probably be further impaired, if not be made quite to vanish. To examine this conjecture, we put in a body that was not wood, which had some parts much more luminous than the rest; and having drawn out the air, all the others disappeared, and even the formerly brighter ones shone but faintly, when the receiver seemed to be exhausted. But keeping the included body a while in that situation, we perceived the parts that had retained light to grow more and more dim, some of them disappearing, and that which was formerly the most conspicuous being now but just visible to an attentive eye. The air being let in, the body began to shine again.

Exper. 5. The rarefaction of the air having so notable an effect on the shining wood, I thought it would not be amiss to try what the compression of the air would do to it. For which purpose, we included a piece of it in such a little instrument to compress, as that devised and proposed by M. Hook. But though we impelled the air forcibly enough into the glass, yet by reason of the thickness requisite in such glasses, and the opacity thence arising, we were not able then to determine whether or no any change was made in the luminousness of the wood.

Exper. 6. To try whether a small quantity of air, without being renewed, might not suffice to maintain this cold fire, though it will not that of a live coal, or a piece of match, we caused a piece of shining wood to be hermetically sealed up in a pipe of clear and thin glass; which being carefully done, the wood retained its light very well when the operation was over, and shone for a long time afterwards.

Exper. 7. On placing a piece of red-hot iron properly within the receiver, and exhausting the air, the operation seemed not to have any effect on it as to alter its shining.

Exper. 8. Having hermetically sealed up a small piece of shining wood in a slender pipe, and placed it in a small receiver, made of clear glass, it was exhausted of air, and afterwards let in again. But by neither of the operations could we perceive any sensible decrement or increase of the light of the wood: which shows that the motion of such bodies as the particles of light may be freely made *in vacuo,* at least such a vacuum as the engine produces.

Exper. 9. Taking a cylindrical glass tube sealed at one end, whose bore was about half an inch, and its length a foot or more. Into this pipe near the sealed end was put a piece of shining wood, wedged in with a piece of cork to keep it from falling; and having inverted the nose of it into another slender glass with quicksilver, and put them both into a long receiver; having pumped a while, that the air included in the pipe expanding itself depressed the quicksilver, and so made escapes into the receiver; we then letting in the outward air, that the stagnant quicksilver might be impelled into the cavity of the pipe now freed from much of the air, to the height requisite for the purpose. This done, on plying the pump again, it was observed that as the air in the pipe, by its own spring, expanded itself more and more, and grew thinner and thinner, the shining wood grew dimmer and dimmer, till at length it ceased to shine, the internal air being then got a good way lower than the surface of the external quicksilver: whereupon opening the communication between the cavity of the receiver and the atmosphere, the quicksilver was driven up again, and consequently the air above it was restored to its former density, on which the rotten wood also recovered its light.

Exper. 10. Having taken a stale and shining fish that was almost all over luminous, though much more in the belly and some parts of the head than elsewhere, and having suspended him in a conveniently shaped receiver, and having exhausted the receiver as much as usual, it appeared indeed, especially towards the latter end of the operation, that the absence of the air considerably lessened, and in some places eclipsed the light of those parts that shone less strongly; but the belly appeared not much less luminous than before. On readmitting the air, the light was perceived to be as it were revived and increased, those parts of the fish that were scarce visible before, or shone but dimly, receiving presently their former splendour.

Exper. 11. Having put into the receiver a large piece of wood, whose luminous superficies might be perhaps ten or twelve times as great as that which the eye saw at once of the surface of such fragments of shining wood as were before employed; and though some parts of this large superficies shone vividly, yet upon withdrawing the air it was deprived of light as the smaller ones had been formerly, the returning air restoring its light to the one, as it had done to the other.

Exper. 12. Having put into the receiver small pieces of rotten fish, that shone some of them more faintly and some of them more vividly in respect to one another, and having in a very small and clear receiver so far drawn off the air as to make the included body disappear; after thus keeping out the air for about twenty-four hours, and then allowing it to re-enter in a dark place and late at night, upon its first admittance the fish regained its light.

Exper. 13. Having put a piece of shining fish into a wide-mouthed glass, about half filled with fair water, and placed this glass in a receiver, the air was exhausted for a good while; it could not be perceived that either the absence or return of the air had any great effect upon the light of the immersed body.

Exper. 14. Placing a very luminous piece of shining fish in the receiver, after exhausting it was kept there 48 hours, in which time its light gradually and wholly vanished; but on restoring the air it recovered its light again, as in the former instances.

All these experiments were made with whitings, being the fittest kind for such trials.

The suddenness with which the included body appeared to be as it were rekindled on the first contact of the air, revived in Mr. Boyle some suspicions he had about the possible causes of these short-lived apparitions of light, which, disclosing themselves upon men's coming in, and consequently letting in fresh air into vaults that had been very long close, did soon after vanish.

An Account of the Pathologiæ Cerebri & Nervosi Generis Specimen: in quo agitur de Morbis Convulsivis et Scorbuto, studio THOMÆ *WILLIS, M. D.* N° 31, p. 600.*

The author gives here a very good specimen of what he formerly promised of the whole pathology of the brain and nerves. The knowledge of the dis-

* Thomas Willis was born at Great Bedwin, in Wiltshire, in 1621. He was educated at Oxford, and was appointed Sedleian professor of natural philosophy in that university in 1660; in which year he also took his degree of doctor of physic, though his original intentions were for the church. Some years afterwards he removed to London, became a member of the Royal Society, and made himself further known by several publications on medical, anatomical, and pharmaceutical subjects; viz. by

eases to which these parts are liable is esteemed very difficult and intricate, and particularly the true causes of convulsions are of a very deep research. With a view to the elucidation thereof, this author reasons after this manner: He teaches that there are indeed animal spirits, that they constitute the being of the corporeal soul, and are also the next and immediate instruments of all animal motions, producing them by a kind of explosion or shooting; upon which elastic or explosive power he establishes his whole doctrine of convulsions. To which he annexes a disquisition on the scurvy, as relating to the same doctrine, and grounded upon the same hypothesis.

Comparison between burning Coal and shining Wood. By Mr. Boyle. N° 32, p. 605.

Their resemblances are as follow.—1. Both live coals and shining wood are luminous by their own light; for both of them shine the more vividly, by how much the place wherein they are put is made the darker by the careful exclusion of the adventitious light—2. Both shining wood and burning coal require the presence of the air, and that too of a particular density.—3. Both shining wood and a burning coal having been deprived for a time of their light, by the withdrawing of the contiguous air, may presently recover it by letting in fresh air upon them.—4. Both live coal and shining wood are easily extinguished by water and many other liquors. This is evident as to the coal. And on wetting a piece of shining wood with a little common water in a clear glass, it presently lost all its light. The event was the same with strong spirit of salt, and also with weak spirit of sal ammoniac; as also with highly rectified spirit of wine, and with rectified oil of turpentine.—5. As a live coal is not extinguished by the coldness of the air, so neither is a piece of shining wood.

Their differences are as follow.—1. The first difference observed between a

his treatises de Fermentatione, de Febribus, de Urinis, de Cerebri Anatome. In the last-mentioned treatise, which was his *chef-d'œuvre*, he had the assistance of Lower in the anatomical dissections, as well as in the Latin composition, and of Sir Christopher Wren, (at that time Savillian professor of astronomy in the university of Oxford,) in the drawings for the plates. These publications were followed by the work above noticed (Pathologia Cerebri), by his Anima Brutorum, and lastly by his Pharmaceutice Rationalis; all of which have been printed together at different times under the title of Opera Omnia, in fol. and in 4to. He died in 1675. Dr. Willis was too much addicted to chemical theories, on which he endeavoured to establish a pathology incompatible with the properties of living bodies. Nevertheless much ingenuity is displayed in all his writings, and those which relate to anatomical subjects may be consulted with advantage for the descriptions and accompanying plates. Succeeding anatomists, however, have remarked that he has not always distinguished between the parts as they appear in the human and brute subject; having at times made dissections of the latter subservient to exemplifying and illustrating the structure of the former.

live coal and shining wood is this, that although the light of the former is readily extinguishable by compression, the latter is not affected by it.—2. A live coal will in a very few minutes be totally extinguished by withdrawing the air; whereas shining wood immediately recovers its light if the air be admitted again, even though excluded for half an hour.—3. A live coal being put into a small close glass, continues to burn only a very few minutes; but a piece of shining wood continues to shine for whole days.—4. A coal as it burns emits a great deal of smoke or exhalations; but luminous wood does not so.—5. A coal in shining wastes at a great rate; but shining wood does not.—6. Live coal is actually and vehemently hot; whereas shining wood is not sensibly lukewarm.

An Observation concerning a Blemish in a Horse's Eye, not hitherto discovered by any Author. By Dr. Rich. Lower. N° 32, p. 613.

The eyes of horses are peculiarly affected with a disorder which no animal besides is troubled with, as far as I have observed; neither do I remember any author hitherto to have taken notice of it. It is a spungy excresence (commonly of a dark musk colour) which grows out of the edge of that coat of the eye called the uvea; which spunge if it grow large or increase in number (as frequently happens) it depraves the sight very much, or totally intercepts it. But that you may more easily conceive the manner how it is done, you may remember that the uvea is a muscular part, the use of it being chiefly to contract and dilate itself for the admission of objects with as much light as the eye can conveniently bear; so that the brighter and more refulgent the light is to which the eye is exposed, that membrane contracts itself into a narrower compass; and the darker the place is the more it dilates itself, as you may see most readily in a cat's eye: so that if that spungy substance which grows out of the edge of the uvea be so great, or the number of them such as that they grow in several places about the pupil of the eye, where it contracts itself, the pupil or sight is very much (if not totally) obstructed, and consequently the horse sees very little or nothing at all: as I have many times taken exact notice in some horses, which being brought into the sun-shine, could not see at all, but suffered me to touch the sight of their eye with my finger without the least winking; which horses being led back into the stable, the uvea in that obscure place dilating itself, they could see very well again, and would not suffer me to show my finger near to the eye without frequent closing their eye-lids and tossing their heads. The same horses I understood by the owners were very apt to stumble in the day-time if the sun shone, but travelled very well and securely in the evening and in dark cloudy weather.

What the cause may be of that fungous excrescence, or why horses are peculiarly obnoxious to it, or what kind of horses most, I have not considered. But I cannot think it comes from straining in great draughts, in races, or from hard travelling, because I have seen very large spunges (as I may call them) in young horses' eyes of two and four years old before they were backed; which after they have been taken up from grass and kept with dry meat, have very much abated, and afterwards being turned again to grass in the spring, have increased again to the wonted largeness. But whether it were from their moist feeding, or holding down their heads to eat (whereby there might be a greater deflux of humours to that part) I cannot determine. But as there are few horses quite free from this evil, and many rendered very inconsiderable by it, I will recount the most remarkable cases which make horses most useless and suspected:

1. The more and greater those excrescences are, the more the pupil of the eye or sight is in danger of being quite obstructed; which you may farther examine by turning the horses' eye to the light, and observing how much of the pupil they obstruct.

2. Spunges on the upper edge of the uvea are apt to grow the largest, and hinder the sight most.

3. That which grows on the middle of the uvea hinders the sight more by distracting the object, than that which grows in either corner or angle of it.

As for the cure, I suppose there can be none expected, but from a drying kind of diet; though perhaps outwardly something may be devised to shadow the eyes, and keep them from being nakedly exposed to the sun, whereby the pupil will not be so closely contracted, and consequently the sight not so much obstructed.

Of Spots seen in Venus. By S. Cassini. N° 32, p. 615.

S. Cassini discovered, October 14, 1666, at 5 h. 45 m. p. m. near the centre of Venus, on the north side, a part brighter than the rest; and at the same time westward two obscure spots, somewhat oblong. Which parts he could not well perceive again till April 28, 1667; on which day, a quarter of an hour before sun rising, he saw again a bright part, situated near the section, and distant from the southern horn a little more than $\frac{1}{4}$ of its diameter; also near the eastern ring he saw a dark and somewhat oblong spot, which was nearer to the northern than the southern horn. At sun rising he perceived that this bright part was then not so near the southern horn, but distant from it $\frac{1}{3}$ of its diameter, which gave him great satisfaction. But he was surprised at the same time to find, that the same motion which was made from south to north in the inferior part of the disk, was on the contrary made from north to south in the superior part. The next day at sun rising, the bright part was not far from the section, and distant from the southern horn $\frac{1}{7}$ of the diameter. When the sun was 4 deg. high, the same was situated near the section, and remote from the southern horn $\frac{2}{5}$ of the diameter. The sun being 6° 10′ high, it seemed to have passed the centre, and that the section of the disk intersected it. The sun being 7° high, it appeared yet more advanced northward, together with two obscure spots seated between the section and the circumference, and equally distant from one another, and from each horn on both sides. And the sky being very clear, he observed the motion of the bright part for $1\frac{1}{8}$ hour; which then seemed to be exactly made from south to north, without any sensible inclination east or westward.

May 10 and 13, before sun rising, he still saw the bright part near the centre northward. Lastly, June 5 and 6, before sun rising, he saw the same between the northern horn and the centre of the planet, and noted the same irregular variation in the obscure spots. But when Venus began to be further removed from the earth, it was more difficult to observe these phænomena.

These appearances in Venus being seen for so small a time, makes it very difficult to know with certainty when they return to the same place. Yet this I can say, supposing that this bright part of Venus which I have observed, especially this year 1667, has been always the same; that in less than one day it completes its motion, whether of revolution or libration, so as in near 23 hours it returns about the same hour to the same situation in this planet; but yet not without some irregularity.*

Extract of a Letter written by Dr. J. Denis, of Paris, touching a late Cure of an inveterate Phrensy by the Transfusion of Blood; translated from the original French. Addressed to the Editor, Mr. Oldenburg. N° 32, p. 617.

You have doubtless heard of a madman that has been lately cured and restored to his understanding by means of transfusion. Some spread a rumour that he died soon after the operation; others that he was relapsed into a greater madness than before; and in short it has been so differently spoken of that I

* From these observations, though rather imperfect, Cassini deduced a conclusion pretty near the truth; more modern and accurate observations having shewn that the period of Venus's rotation is completed in 23 h. 22 m.

thought myself obliged, in consequence of many false rumours, to give you a faithful and exact account of the condition to which this poor man was reduced before the transfusion; of what passed during that operation; and of the surprising effects that have followed upon it hitherto.

The patient is about 34 years of age. His phrensy began first of all to appear seven or eight years ago, and as far as can be judged, it was occasioned by a disgrace he received a little before in some amours where he hoped to gain a very considerable fortune. This first fit of extravagance was very violent, and lasted 10 months without any lucid interval; but returning afterwards by degrees to his wits, and having given all possible marks of a sound understanding, he was married to a young gentlewoman, who was persuaded that his madness was the relick of a sickness he had before, and that there was no appearance he would ever relapse into it. But this was far from proving so, and even the first year of his marriage ended not without his returning to his former extravagancies.

Thus then he relapsed, and has been several times restored within these seven or eight years. But what is chiefly to be observed, is, that the fit always lasted at least eight or ten months without any respite, notwithstanding all the care and means used to relieve him. It is also worthy of notice, that a person of quality having determined to attempt his cure by all manner of ways, caused him to be bled in his feet, arms, and head, even 18 times, and made him bathe himself 40 times, not to mention innumerable applications to his forehead, and potions. But instead of amendment, the distemper seemed to be provoked by those remedies, and this poor creature fell into such a rage, that it was necessary to bind him up from doing mischief. His madness has been always periodical, and never abated but by little and little, and that rather at times when nothing has been done to him, than when he has been tormented with medicines.

His last relapse was about four months since, in a place twelve leagues from Paris; and his wife hearing of it went immediately to him to relieve him. She soon shut him up, and was even constrained to tie him for some time, because he was in such an extraordinary rage as to beat her. But notwithstanding all her care, he once got loose stark naked, and ran away to Paris, though in a dark night. His wife had him searched for in all the neighbouring villages, whilst he ran up and down the streets of Paris without finding any place to retire to, as those who at first had the charity to receive him into their houses, began to fear the danger they were exposed to.

He was not less outrageous in this last fit than in the former. He has spent three or four months without sleep, and his greatest diversion during that time was to tear the clothes that were given him, to run naked abroad, and to burn in the houses where he was whatever he could meet with. He moved to compassion all good people that saw him, and especially those in the Marais du Temple, where he was most known, and where he had been wont to be seen before this distemper as well clothed and fashioned as any one of his condition could be.

Monsieur de Montmor, among others, was the person most touched with it, and resolved to employ his interest to procure him a place in one of the hospitals. But first he thought of transfusion, and believed there would be no danger in trying it upon this man, being so persuaded by many experiments we had already made in his presence. He therefore had him taken up for that end, and having sent for me and M. Emmerez to ask our opinion of the fitness of trying the transfusion upon this man, we answered, that we could indeed give good assurance for his life, and that the operation was in itself incapable of causing the death of any one, if discreetly managed; but as to the cure of such an extravagance as that appeared to us, we had not yet experience enough to dare to promise him that, and that our conjectures went no farther than to think that the blood of a calf by its mildness and freshness might possibly allay the heat and ebullition of his blood being mixed therewith. The matter having been sufficiently examined, we resolved to carry this man into a private house; and there we appointed for his keeper that porter on whom we had already practised the transfusion eight months ago, both that the thing might not appear so new to him as to others who never had seen the experiment before, and that he might serve to convince our patient and others who should be present at the operation, that there was no danger in it at all.

December 19, we used what art we could to dispose the fancy of our patient to suffer the transfusion, which we resolved should be tried upon him that night about six o'clock. Many persons of quality were present, together with several physicians and surgeons too intelligent to suspect them of being capable of the least surprise. Mr. Emmerez opened the crural artery of a calf, and did all the necessary preparations in their presence; and after he had drawn from the patient about ten ounces of blood out of a vein of the right arm, we could give him no more again than about five or six ounces of that of the calf, by reason that his constrained posture, and the croud of the spectators interrupted very much this operation.

Meantime he found himself, as he said, very hot all along his arm, and under the arm-pits; and perceiving that he was falling into a swoon, we presently stopped the blood running in, and closed up the wound. Yet he supped two hours after, and notwithstanding some dulness and sleepiness, he yet passed that night with singing, whistling, and other extravagancies usual with him.

On the next morning we found him somewhat less extravagant both in his actions and words, which induced us to believe, that by repeating the transfusion once or twice we might find a more remarkable change in him. We therefore prepared ourselves to repeat it upon him the next Wednesday at six o'clock in the evening again, in the presence also of several very able physicians, Bourdelot, Lallier, Dodar, de Bourges, and Vaillant. But as this man appeared very thin, and as it was not at all probable that his blood was peccant in the quantity after three or four months continual watching, and after the hunger and cold he had suffered in running naked in the streets without finding shelter at nights, we took but two or three ounces of blood from him, and having put him in a more convenient posture, we made this second transfusion into his left arm more plentiful than the first. For considering the blood remaining in the calf after the operation, the patient must have received more than one whole pound.

As this second transfusion was larger, so were the effects of it quicker and more considerable. As soon as the blood began to enter into his veins, he felt the like heat along his arm and under his arm-pits which he had felt before. His pulse rose presently, and soon after we observed a plentiful sweat over all his face. His pulse varied extremely at this instant, and he complained of great pains in his kidneys, and that he was not well in his stomach, and that he was ready to choke unless they gave him his liberty.

Presently the pipe was taken out that conveyed the blood into his veins, and whilst we were closing the wound, he vomited much bacon and fat which he had eaten half an hour before. He found himself urged to urine, and asked to go to stool. He was soon made to lie down, and after two good hours strainings to void divers liquors which disturbed his stomach, he fell asleep about ten o'clock, and slept all that night without waking till next morning about eight o'clock. When he awoke, he showed a surprising calmness, and great presence of mind, in expressing all the pains, and a general lassitude he felt in all his limbs. He made a great glass full of urine, of a colour as black as if it had been mixed with the soot of chimnies.

Hearing of some of the company that we were in a time of jubilee, he asked for a confessor, to dispose himself to be made participant of it. And he confessed himself accordingly to M. de Vean with that exactness, that the confessor gave him the public testimony of a sound understanding, and even judged him capable to receive the Sacrament, if he continued in that state and devotion.

He remained sleepy all the rest of that day, spoke little, and prayed those that came to importune him with interrogatories, to give him rest. And he went on to sleep well also the whole night following. Friday morning he filled another urinal with his water, almost as black as that of the day before. He bled at the nose very plentifully, and therefore we thought it proper to take two or three small porringers of blood from him.

Saturday morning, the last day before Christmas, he desired again to go to confess, and so to dispose himself for the Communion. Then one Mr. Bonnet examined him in hearing him confess, and after he had found him to have all the reason necessary to receive the Sacrament, he presently gave him the Communion. That same day his urine cleared up, and after that time it resumed by little and little its natural colour.

His wife, mean time, that had sought him from town to town, came to Paris, and having found him out, when he saw her he soon expressed much joy to see her, and related to her with great presence of mind the several accidents that had befallen him, running up and down the streets; how the watch had seized on him one night, and how calf's blood had been transfused into his veins.

This woman confirmed yet more to us the good effects of the transfusion, by assuring us that at the present season her husband was used to be outrageous against herself, and that instead of the kindness he now showed to her, he used to do nothing but swear and beat her.

It is true that comparing his present calm condition before the transfusion, no man scrupled to say that he was perfectly recovered. Yet to speak the plain truth, I was not so well satisfied as others seemed to be, and I could not persuade myself that he was in so good a temper as to stop there, but I was inclined to believe by some things I saw, that a third transfusion might be requisite to accomplish what the two former had begun.

Yet in delaying the execution of these thoughts from day to day, we observed so great an amendment in his carriage, and his mind so cleared up by little and little, that his wife and all his friends having assured us that he was restored to the same state he used to be in before his phrenzy, we entirely quitted that resolution. I have seen him almost every day since; he has expressed to me all manner of acknowledgment, and been also with M. de Montmor, thanking him very civilly for his goodness in recovering him out of that miserable condition he was in by a remedy which he should remember as long as he lived.

He is at present of a very calm spirit, performs all his functions very well, and sleeps all night long without interruption, though he says he has now and then troublesome dreams. He has carried himself so discreetly in some visits he made this week, that divers physicians and other persons worthy of credit that have seen him, can render an authentic testimony to all the circumstances here advanced by me.*

History and Description of an Hermaphrodite.† *Communicated by Dr.* THOMAS ALLEN, *in a Latin Letter.*‡ *N°* 32, *p.* 624.

Inter varios insolentesque naturæ lusus, dicam? an errores, quos apud eos, qui de Androgynis egerunt (quorum scripta sedulo deditaque opera perlustravi) in lucem productos adhuc videre mihi contigit, vix alium quenquam notatu digniorem memini occurrere hoc ipso quem tibi, erudite vir, impræsentiarum exhibeo. Neque enim hunc, quem jam descriptum eo, hermaphroditum, aut spurcissimis illis fœminis, quæ apud Gæcos Τριβάδες audiunt, apud Ægyptios vero frequentissime reperiebantur, annumerandum, aut cum descriptione quacunque hactenus quod sciam evulgata, ullatenus quadrare existimo. Unde nec prorsus indignus mihi videtur, qui nativis depictus coloribus, absque omni verborum fuco, Illustrissimæ Lectissimæque Regiæ Societatis et tuis oculis usurpandus veniat.

Nomen ipsi est Anna Wilde; natus vero est (condonandus enim hermaphrodito solæcismus) mense Februario, ipso Purificationis festo, Anno salutis, 1647, in pago non ignobili Agri Hamptoniensis, vulgo Ringwood. Sexto ætatis anno inter saltandum colluctandumque cum pueris coætaneis (quos omnes viribus facile superabat) extuberationes duæ, Herniarum Βȣβωνοκηλων dictarum, primum emicuere; quibus in ordinem redigendis (id enim illis animi erat) Chirurgi diu operam luserunt. Testiculi enim erant, qui jam prægrandes facti, scrotis cutaneis corrugatis, pilisque obsitis inclusi, non alio discrimine a Virilibus naturaliter se habentibus distinguuntur, quam quod singuli testes suo proprio divisoque ab invicem hic scroto gaudeant, ita tamen elongato, ut ex utriusque productione confingantur labia vulvæ.

In sinu muliebri (ut jam à Mercurio ad Venerem transeamus) nymphæ et carunculæ myrtiformes, integræ satis se produnt: Quin et membranula quadam, a perinæo sursum tendente, media pars vulvæ tegitur. Clitoris non apparet. Uterus ejusque cervix a communi sequioris sexus lege ne minimum quidem recedunt. Usque ad tertium supra decimum ætatis annum pro fœmella habitus, et fœmineo vestitu indutus, munera illi sexui destinata inter fœminas assidue obibat. Cum forte vero pani subigendo strenuam navaret operam, en derepente

* A third transfusion was attempted on this man, of which an account is given in No. 36, where we shall offer some remarks on this case, which terminated fatally.

† The hermaphrodite was at that time to be seen in London.

‡ Accounts of other hermaphrodites are to be found in the xvi and xlvii volumes of the Transactions.

priapus, ad id temporis latens, magno cum impetu foras prorupit, accedente non levi ipsius Μεταμορφωμένου stupore. Erectus penis quatuor circiter pollices æquat. Locum virgæ virilis ipsissimum occupat; in glandem pariter desinit; præputio (quod illi etiam frænulo, ut in viris fit, annectitur) instructum: sed glans imperforata (ita tamen ut tenuis membranula eam obturans facile pertundi posse videatur) semini, per urethram, seu potius virgæ canaliculum viam affectanti, exitum negat; unde per pudendum muliebre (refluum forte) excernitur.

Cum annorum esset sedecim, menstrua periodice et modo debito fluere ceperunt, atque per biennium perseveraverunt. Quo elapso, iisdem non amplius comparentibus, pullulavit Barba, et exinde totum corpus pilosum conspicitur; vox corporisque habitus virilem æmulantur. Crinis se habet virorum adinstar. Mammæ nullæ exsurgunt: papillæ perquam exiguæ. Pectus latum est. Ischia non ita dissita. Nates quam sunt fœminarum contractiores.

Se ad utrumque sexum comparatum asserit, sed fœminis misceri præoptare; quas etiam cum videt, et concupiscit, erigitur penis, qui quoties virum appetit, flaccidus manet.

Unum hoc, idque nec extra oleas putem, coronidis loco subnectam; quod nempe, cum nocte quadam, quam totam tripudiis, compotationibus, cæterisque id genus lasciviæ incitamentis, cum aliquot ejusdem farinæ congerronibus insumpserat, oculos in virum quendam formæ venustioris conjecerat, mox eum adeo deperibat, ut sequenti die, præ amoris œstro, in passionem hystericam incideret; quam revera talem fuisse, non solum elevatio abdominis, cantus, risus, fletus, (notissima illius intemperiei symptomata) sed et juvantia, satis liquido comprobarunt: Applicato quippe emplastro ex galbano regioni umbilici, exhibitisque remediis hystericis illico convaluit.

An Account of some Books. *N° 32, p.* 625.

I. Nouveaux Elemens de Geometrie: Or, a Mathematical Treatise, entitled, New Elements of Geometry, printed at Paris in quarto, Anno 1667.

II. Synopsis Optica, Auth. Honorato Fabri, Soc. Jesu, Lugduni Gall. in 4to. An. 1667.

This author pretends to have comprised in this treatise, containing 58 propositions, besides many corollaries, all that has been hitherto discovered in optics, and to have added thereto many curious and useful remarks, not mentioned in other authors.

III. De Vi Percussionis, Joh. Alphons. Borelli.* Bononiæ, in 4to. 1667.

* An Italian, celebrated in mathematics, which science he professed at Florence, Pisa, and Rome, and was one of those philosophers who had the honour to be esteemed and noticed by Queen Christina of Sweden. The works of Borelli are very numerous and valuable. He was born at Naples in 1608, and died at Rome in 1679.

As in the doctrine of percussion several things are to be accurately distinguished, such as the percussive force, the motion or the velocity of the percussion, and the resistance of the body percussed; and then an estimate to be made of the proportion of those three to one another: this author pretends to have both assigned that difference and demonstrated the proportion; adding, that though Galilæo saw and acknowledged that the force of percussion was infinite, or rather unlimited,* yet he there deferred discoursing any farther on that matter. Our author pretends that that proposition concerning the infiniteness of the force of percussion, not having been yet demonstrated by any, he has in this book resumed the whole matter concerning percussion, and clearly demonstrated the true and genuine nature of it, its cause, properties and effects. In doing which, he takes occasion to discourse also of gravity, magnetism, tremor of bodies, pendulums, &c.

IV. Nicolai Stenonis† Musculi Descriptio Geometrica, Florentiæ, 4to. 1667. This work exhibits an ingenious but unsatisfactory attempt to explain the structure and action of a muscle upon geometrical principles. Subjoined to it are two narratives, one of which relates to the dissection of the head of a shark, which he calls canis carcharia,‡ where he delivers many curious observations concerning the skin, eyes, optic nerves, muscles of the eye, exceeding smallness of the brain, as also concerning the mouth and strange teeth of this fish; examining withal whether glossopetræ be the teeth of this creature, or stones produced by the earth; in which controversy he takes their side who maintain that those and divers other substances found in the earth are parts of the bodies of animals; and endeavours to prove that such sorts of earth may be the sediments of water, and such bodies the parts of animals carried down together with those sediments, and in progress of time reduced to a stony hardness.§—The other narrative relates to a female dog-fish, also dissected by himself, where there

* That is, in respect of mere dead weight or pressure.

† This celebrated Danish anatomist studied under his countryman Bartholine, and held for some time the professorship of anatomy at Copenhagen. He afterwards travelled into Holland, Germany, and Italy; in the last-mentioned country he resided a considerable time, during which he changed his religion, becoming a catholic, and receiving some ecclesiastical appointments from Pope Innocent XI. He died in 1686, before he had attained his 50th year. Steno wrote several Latin tracts on anatomical subjects, and discovered the external salivary duct, which has since gone by his name. See his Obs. Anatom. quibus varia oris, oculorum et narium vasa describuntur, novique salivæ, lachrymarum et muci fontes deteguntur. Leidæ, 1662.

‡ Squalus Carcharias, Linn.

§ These glossopetræ are petrifactions of the teeth of some animal, probably some marine animal; but they do not always resemble the teeth of the shark. They were once supposed to be the petrified tongues of serpents, whence their compound name.

occur no less remarkable observations than in the former, both as to the parts in the head and those in the body; concerning the small weight of the brain of this fish compared to the weight of its body; several little fishes found in the stomach, untouched by any teeth; the ureters, the ovarium and oviduct; where he digresses to show that the mulierum testes esse ovario analogos, and refers for further proof of this to his intended treatise, De partium genitalium analogia.

On Grinding Optic and Burning Glasses of Non-Spherical Figures. By Francis Smethwick, *Esq. F. R. S. N° 33, p.* 631.

Mr. Smethwick produced before the society, Feb. 27, 1667-8, as specimens of his invention, a telescope, a reading-glass, and two burning-glasses. The telescope was about four feet long, with four glasses, the three ocular ones, plano convex, were of this newly invented not-spherical figure, and the fourth a spherical object-glass. This being compared with a common, yet very good telescope, longer than it by about four inches, and turned to several objects, it was found to excel the other by taking in a greater angle, and representing the objects more exactly in their respective proportions, and bearing a greater aperture free from colours.

The reading glass, of the same figure, being compared with a common spherical glass, far excelled it, by magnifying the letters to which it was applied up to the very edges, and by shewing them distinctly from one brim, through the centre, to the other, which the spherical glass came far short of. But this effect the new figured glass performed only on one of its sides, whereas the spherical glasses perform their effect nearly alike on both sides.

Lastly, the two burning concaves, of this new invented figure, were, the one of six inches diameter, its focus three inches distant from the centre; the other of the same diameter, but less concave, and its focus ten inches distant. These when approached to a large candle lighted, somewhat warmed the faces of those that were four or five feet distant at least, and when held to the fire burnt gloves and garments at the distance of about three feet from the fire.

At another time, in the presence of Dr. Seth Ward, the deeper of the two concaves turned a piece of wood into flame in the space of ten seconds of time, and the shallower in five seconds, and that in autumn, about nine o'clock in the morning, and the weather gloomy. The deeper concave, when held to a lucid body, would cast a light strong enough to read by at a considerable distance. Also, that exposing the same to a northern window, on which the sun did not shine, or very little, he perceived that it would warm one's hand sensibly, by collecting the warmed air in the day-time, which it would not do after sun-set.

Tides observed at Plymouth. By Mr. S. Colepresse. N° 33, p. 632.

The diurnal tides, from about the latter end of March till the latter end of September, are about a foot higher in the evening than in the morning, that is, in every tide that happens after noon and before midnight. On the contrary, the morning tides, from Michaelmas till Lady-day in March again, are constantly higher by about a foot than those that happen in the evening. And this proportion holds in both, in the intermediate times of increase and decrease. The highest monthly spring tide is always the third tide after the new or full moon, if a cross wind do not oppose the water, as the north-east or north-west usually does. The highest springs make the lowest ebbs. The water neither flows nor ebbs alike in respect of equal degrees; but its velocity increases with the tide, till just at mid-water or half flood, at which time the velocity is strongest, and so decreases proportionably till high water or full sea. As appears by the following scheme, collected from observations made at several times and places; which, though taken at Plymouth Haven, where even the water usually rises about sixteen feet, yet it may indifferently serve for other places, where it may rise as many fathoms, or not so high, by a proportional addition or subtraction.

		Height.				Height.	
Time of Flowing	1 hr.	1 feet	6 inch.	Time of Ebbing	1 hr.	1 feet	6 inch.
	2	2	6		2	2	6
	3	4	0		3	4	0
	4	4	0		4	4	0
	5	2	6		5	2	6
	6	1	6		6	1	6

Inquiries and Directions for the Antilles, or Caribbee Islands. N° 33, p. 634.

I. Of Vegetables.

1. Whether the juice of the fruit of the tree junipa, being as clear as any rock water, yields a brown violet dye, and being put twice upon the same place makes it look black? and whether this tincture cannot be got out with any soap, yet disappears of itself in nine or ten days: and whether certain animals, and particularly hogs and parrots, eating of this fruit, have their flesh and fat altogether tinged of a violet colour?—2. Whether ring doves that feed upon

the bitter fruit of the acomas tree, have their flesh bitter also?—3. Whether the wood of the acajou tree, being red, light and well scented, never rots in water, nor breeds any worms when cut in due season? And whether the chests and trunks made thereof, keep clothes placed therein from being worm-eaten?—4. Whether the leaves of a certain tree, peculiarly called Indian wood, give such a *haut-gout* to meat and sauces, as if it were a composition of several sorts of spices?—5. Whether there be such two sorts of the wood called saponier or soap-wood, of the one of which the fruit, of the other the root serves for soap?—6. Whether the bark of the paretuvier-wood tans as well as oak-bark? —7. Whether the root of the tree laitus, being brayed and cast into rivers, makes fishes drunk?—8. Whether the root of the manioc is so fertile, that one acre planted therewith, yields so plentiful a crop as shall feed more people than six acres of the best wheat?—9. What symptoms do usually follow on taking the juice of manioc, or on eating the juice with the root, and what effects are thereby produced on the body, that infer it to be accounted a rank poison? —10. The palmetto royal being said, by Ligon, to be a very tall and straight tree, and so tough that none of them have been seen blown down and hollow; in all which respects they may serve for special uses, and particularly for long optic tubes; it is much desired that the largest and longest pieces of them that can be stowed in a ship may be sent over.—11. Whether the oil expressed out of the plant ricinus or palma Christi, be used by the Indians to keep them from vermin?—12. Whether in the passage of the isthmus from Nombre de Dios to Panama, there is a whole wood full of sensitive trees, of which as soon as they are touched, the leaves and branches move with a rattling noise, and wind themselves together into a roundish figure?—13. Whether there be certain kernels of a fruit like a white pear plum, which are very purgative and emetic, but having the thin film which parts them into halves taken out, they have no such operation at all, and are as sweet as a Jordan almond?—14. To send over some of the roots of the herb called by our French author l'herbe aux flèches, (the dart-herb) which being stamped, is said to have the virtue of curing the wounds made with poisoned darts.—15. To send some of the grain of the herb musk, putting it up carefully in a box, that it may keep its musk scent.—16. To send over a specimen of all medicinal herbs, with their respective virtues, as they are reputed there: particularly the prickle-with at the Barbadoes, macao, mastic-tree, locust, black-wood, yellow within, five-sprig, tidle-wood, white-wood, Barbadoes-cedar.—17. Whether the fruit mancenille of the mancenillier-tree, though admirably fair and fragrant, yet is fatal to the eater, and falling into the water kills the fishes that eat of it, except crabs, which yet are said to be dangerous to eat when they have fed upon this fruit? Whether under the bark of this tree is contained a certain glutinous liquor as white as milk, very dangerous, so that if you chance to rub it and this juice spurt upon the shirt, it will be like a burning; if upon the naked flesh it will cause a swelling; if into the eye, blindness for several days? And whether the shadow of this tree be so noxious, that the bodies of men reposing under it will swell strangely? Further, whether the natives use the milky juice of this tree, and the dew falling from it, and the juice of its fruit, in the composition of the poison they infect their arrows with?

II. Of Animals and Insects.

18. Whether the skin of the tatou, and the little bone in his tail do indeed, as is related, cure deafness and pains of the ears? And whether this animal be proof not only against the teeth of dogs, but also against bullets?—19. Whether the birds called canides, be so docile, that some of them learn to speak not only Indian, but also Dutch and Spanish, singing also the airs in the Indian tongue as well as an Indian himself? And whether the bird colibry have a scent as sweet as the finest amber and musk? both which are affirmed by a French author?—20. To procure some of the fat of birds, called fregati, reputed to be very anti-paralytical and anti-podagrical.—21. To send over a land-pike, which is said to be like the water pike, but that instead of fins it has four feet, on which it crawls.—22. Whether the skin of the sea wolf, which they otherwise call the requiem, be so rough and stiff, that they make files of them fit to file wood? And whether it be usually guided by another fish, that is beautified with such a variety of curious and lively colours, that one would say such fishes were girt with necklaces of pearls, corals, emeralds, &c.?—23. Whether the skin of sea calfs, otherwise called lamantins, be so hard when dried, that they serve the Indians for shields?—24. Whether the ashes of the fresh water tortoises hinder the falling of the hair, being powdered with it?—25. Whether the land crabs of these islands do at certain times hide themselves all under ground for the space of six weeks, and during that time change and renew their shells?—26. Whether the serpents in those parts that have black and white spots on their backs be not venomous?—27. To send over some of the skins of those huge lizards, they call ouayamaca, which, when come to their full size, are said to be five feet long, including the tail. And especially to send some of those that are said to have the scales of their skins so bright and curious, that at a distance they resemble cloth of gold and silver.—28. Whether the shining flies called cucuyes, hide almost all their light when taken, but when at liberty, afford it plentifully?—29. Whether there be a sort of bees brown and blue, which make a black wax, but the honey in it whiter and sweeter than that of

Europe?—30. Whether in those parts the Indians cure the bitings of serpents by eating fresh citron pills; and by applying the unguent made of the bruised head of the wounding serpent, and put hot upon the wound?—31. Whether the woodlice in those countries, generated out of rotten wood, are able not only to eat through trunks in a day or two, and to spoil linen, clothes, and books, (of which last they are said to spare only what is written or printed;) but also to gnaw the props which support the cottages, so that they fall? And whether the remedy against the latter mischief is, to turn the ends of the wood that is fixed in the ground, or to rub the wood with the oil of that kind of palma Christi with which the natives rub their heads to secure them from vermin?—32. Whether that sort of vermin called ravets spare nothing of what they meet with (either of paper, cloths, linen, and woollen) but silk and cotton?—33. Whether the little cirons called chiques, bred out of dust, when they pierce once into the feet, and under the nails of the toes, over-run the whole body, unless they be drawn out betimes? And whether at first they cause but a little itch, but afterwards having pierced the skin, raise a great inflammation in the part affected, and become in a small time as large as a pea, producing innumerable nits, that breed others?

Answers to some of the Queries relative to Vegetables.

1. There is nothing improbable in this; since it is well known that several animals in whose food madder has been mixed, will, after a certain time, have even the bones tinged red.

2. It is expressly affirmed by authors of good credit, that the white-headed pigeon, (columba lencocephala, Lin.) a species very common in America, is either bitter or sweet according to its food. See Latham's Synopsis, vol. ii. p. 616.

5. The Sapindus Saponaria, Lin. known in North America by the name of the soap tree, is a tree with a stem of moderate thickness, upright branches, and winged leaves: it bears round berries resembling plums, having a large smooth stone or kernel, covered with a slight pulp, which is of a saponaceous quality, and may be used for washing, but is said to injure the linen by its acrimony.

6. This seems to be the mangrove, (rhizophora mangle, Lin.) a tree from thirty to forty feet high, with a thickish stem, smooth bark, and pendulous branches, with laurel-like leaves towards the tips. The branches which happen to touch the ground take root, and producing similar stems and branches, propagate the tree in arcades to a vast extent, in the same manner as the ficus religiosa, Lin. The bark is used in tanning.

8. According to Sloane, the root of the manioc or cassava, (Iatropha manihot, Lin.) "is of the most general use of any provision all over the West Indies, especially in the hotter parts, and is used to victual ships."

9. Swelling of the whole body; severe vomitings and purgings; giddiness, swoonings, and sometimes death. See Sloane's Jamaica, &c.

11. The oil of the ricinus or palma Christi, is at present in high esteem as a cathartic, and is commonly known by the name of castor oil.

12. Nothing improbable in this. Sloane describes a highly sensitive species under the name of *sensible-grass,* Mimosa herbacea, &c. which spreads over large spots of ground in many parts of

Jamaica, and is so very sensible that "a puff of wind from your mouth will make impressions on it." "I have (says this author) on horseback wrote my name with a rod on a spot of it, which continued visible for some time, and it is the most sensible of any of this kind."

15. The seeds of the hibiscus abelmoschus, a plant of the malvaceous tribe.

17. The juice of the manchineel tree, (hippomane mancinella, Lin.) is one of the most acrimonious and dangerous of vegetable poisons, and though the accounts of its malignant properties may be here somewhat exaggerated, yet it is certain that even the effluvia of the tree are often prejudicial to those who remain long under its shade. As to the fruit rendering animals poisonous which happen to feed upon it, we are assured by Catesby that this is a groundless idea.

Answers to some of the Queries relative to Animals.

18. The long armour of the tatu or armadillo, is a sufficient defence against any common accidents to which this animal may be exposed, but by no means proof against a bullet. As to the former part of the query, it surely deserves no answer.

19. The colibris or humming-birds, in their living state, are not naturally impregnated with the musky odour here mentioned, and it may rather be supposed that the specimens are sometimes perfumed in order to preserve them when sent over in a dried state. It may be added that many animal substances by long keeping are known to acquire a musky scent.

20. The bird here mentioned seems to be the pelecanus aquilus, or man of war bird, Lin.

21. This animal is probably the lizard, called in Jamaica by the name of galliwasp: it is from twelve to fifteen inches or more in length, and is covered with large rounded scales resembling those of a fish.

22. The skin of several of the sharks is often used by various artificers for the purpose of rasping wood, &c.

23. The animal here alluded to is not, properly speaking, a phoca or sea-calf, but a species of the genus trichecus, viz. the whale-tailed manati of Pennant. The skin of the body is excessively hard and strong, resembling the bark of a tree.

25. Yes. See Brown's Jamaica, p. 423, where a pretty full account of the economy of this species, viz. (Cancer ruricola, Lin.) may be found. See also Catesby's Carolina, vol. ii. p. 32.

27. Many of the larger Indian and American lizards are very elegantly variegated: among others some varieties of the lacerta monster, Lin. In New Holland is a peculiarly beautiful variety of the same lizard, which is so richly variegated with bright yellow or black ground, as to bear a striking resemblance to a gold embroidery: this may probably occur in several parts of America and the West-Indies, as well as in Austral Asia.

28. Yes, at pleasure. The insect meant is the elater noctilucus, Lin. See Syst. Nat. p. 661. Brown's Jamaica, p. 432, and Sloane's Jamaica, vol. ii. p. 206.

31. The insects here called woodlice, are the termites, (improperly named white ants, in many of the West Indian and African settlements). See Philosophical Transactions, vol. lxxi, where an ample account of their economy and ravages may be found. There is no exaggeration in the reports here mentioned.

32. The insects here called ravets, are the blattæ or cockroaches, the destructive nature of which is too well known to require particular description. See the preface to the 3d volume of Drury's Exotic Insects.

33. The true history of the *chegoes*, here termed chiques, is not yet exactly known; the observations of authors not being sufficiently clear. According to Catesby, the insect is a species of flea: others rather consider it as a species of acarus or mite. Whatever its genus may be, it is observed to deposit its eggs under the skin of the human feet, particularly of the toes, and is most frequent in

those of the labouring negroes: troublesome swellings are the consequence, which by neglect are apt to degenerate into ulcers. See Catesby's Carolina, appendix, p. 10. In the Systema Naturæ of Linnæus, the insect is referred to the genus pulex, under the title of *pulex penetrans*. See also Sloane's Jamaica, introduction, p. cxxiv.

An Account of two Books. N° 33, *p.* 640.

I. Saggi di Naturali Esperienze fatte nell Academia del Cimento, in Firenze, An. 1667, in fol.

The book contains these particulars:—1. An application of the instruments employed in these experiments.—2. Experiment belonging to the natural pressure of the air.—3. Concerning artificial conglaciations.—4. About natural ice.—5. About the change of the capacity of metal and glass.—6. Touching the compression of water.—7. To prove that there is no positive lightness.—8. About the magnet.—9. About amber and other substances of a virtue electrical.—10. About some changes of colours in divers fluids.—11. Touching the motions of sound.—12. Concerning projectiles.—13. Various experiments.

II. Vera Circuli et Hyperbolæ Quadratura, in propria sua proportionis specie inventa & demonstrata, à Jac. Gregorio Scoto,* Patavii, in 4to.

This tract, perused by some very able and judicious mathematicians, and particularly by the Lord Viscount Brounker, and the Rev. Dr. John Wallis, receives the character of being very ingeniously and very mathematically written, and well worthy the study of men addicted to that science: that in it the author has delivered a new analytical method for giving the aggregate of an infinite or indefinite converging series: and that thence he teaches a method of squaring the circle, ellipsis, and hyperbola, by an infinite series, thence calculating the true dimensions as near as you please. And lastly, that by the same method from the hyperbola he calculates both the logarithms of any natural number assigned, and *vice versa*, the natural number of any logarithm given.

* James Gregory, a celebrated mathematician, was born at Aberdeen, 1639. He very soon distinguished himself by his ingenious writings and inventions in various branches of the mathematics. He was the contemporary of Newton, as well as a formidable competitor in some of his discoveries, as the reflecting telescopes and infinite series. After returning from his travels on the continent, he became a respectable member of the Royal Society, and contributed several valuable papers to the Philosophical Transactions. Mr. Gregory was engaged in some controversies with several eminent philosophers, as Newton, Huygens, &c. He seems to have been of an irritable temper, and jealous of his discoveries and inventions. He seems also to have been rather severe in his contemptuous attack on the harmless Mr. Sinclair of Glasgow. Besides the before mentioned papers in the Philosophical Transactions, he was the author of several learned works; as Optica Promota, 1663; Vera Circuli et Hyperbolæ Quadratura, 1667; Geometriæ Pars Universalis, 1668; Exercitationes Geometricæ, 1668; The Great and New Art of Weighing Vanity, &c., 1672. Mr. Gregory became successively

The Squaring of the Hyperbola. By Lord Viscount BROUNCKER.* *N° 34, p. 645.*

Let A B be one asymptote of the hyperbola E d C, fig. 7. pl. 7 ; and let A E and B C be parallel to the other; also let A E be to B C as 2 to 1 ; and let the parallelogram A B D E be equal to 1. And supposing that E A, $\alpha\zeta$, K H, $\beta\eta$, d θ, $\gamma\kappa$, $\delta\lambda$, $\epsilon\mu$, C B, &c. are in an harmonic series, or the reciprocals of an arithmetical progression. Then will

$$ABCdEA = \frac{1}{1\times 2} + \frac{1}{3\times 4} + \frac{1}{5\times 6} + \frac{1}{7\times 8} + \frac{1}{9\times 10} \text{ \&c.}$$

$$EdCDE = \frac{1}{2\times 3} + \frac{1}{4\times 5} + \frac{1}{6\times 7} + \frac{1}{8\times 9} + \frac{1}{10\times 11} \text{ \&c.}$$

$$EdCyE = \frac{1}{2\times 3\times 4} + \frac{1}{4\times 5\times 6} + \frac{1}{6\times 7\times 8} + \frac{1}{8\times 9\times 10} \text{ \&c.}$$

For (in fig. 2 and 3) the Parallelograms		And (in fig. 4) the Triangles	Note.
	$CA = \frac{1}{1\times 2}$	$EdC = \frac{1}{2\times 3\times 4} = \frac{\square dD - \square dF}{2}$	
$dD = \frac{1}{2\times 3}$	$dF = \frac{1}{3\times 4}$	$Ebd = \frac{1}{4\times 5\times 6} = \frac{\square br - \square bn}{2}$	$\frac{1}{2} CA = dD + dF$
$br = \frac{1}{4\times 5}$	$bn = \frac{1}{5\times 6}$	$dfC = \frac{1}{6\times 7\times 8} = \frac{\square fG - \square fk}{2}$	$\frac{1}{2} dD = br + bn$
$fG = \frac{1}{6\times 7}$	$fk = \frac{1}{7\times 8}$	$Eab = \frac{1}{8\times 9\times 10} = \frac{\square aq - \square ap}{2}$	$\frac{1}{2} dF = fG + fk$
$aq = \frac{1}{8\times 9}$	$ap = \frac{1}{9\times 10}$	$bcd = \frac{1}{10\times 11\times 12} = \frac{\square cs - \square cm}{2}$	$\frac{1}{2} br = aq + ap$
$cs = \frac{1}{10\times 11}$	$cm = \frac{1}{11\times 12}$	$def = \frac{1}{12\times 13\times 14} = \frac{\square et - \square el}{2}$	$\frac{1}{2} bn = cs + cm$
$et = \frac{1}{12\times 13}$	$el = \frac{1}{13\times 14}$	$fgC = \frac{1}{14\times 15\times 16} = \frac{\square gu - \square gh}{2}$	$\frac{1}{2} fG = et + el$
$gu = \frac{1}{14\times 15}$	$gh = \frac{1}{15\times 16}$	&c.	$\frac{1}{2} fk = gu + gh$
&c.	&c.		&c.

professor of mathematics in the universities of St. Andrews and Edinburgh; where he was suddenly struck with total blindness while employed in showing to his pupils the satellites of Jupiter, and died a few days after, in 1675, being only 36 years of age; to the great loss of the mathematical and philosophical world.

* William Brouncker, or Brounker, Lord Viscount of Castle Lyons in Ireland, was born about the year 1620. He very early manifested a genius for mathematics, in which he afterwards became very eminent. He was made M. D. at Oxford, 1646. Afterwards he maintained an honourable cor-

Therefore, in the first series, half the first term is greater than the sum of the two next; and half the sum of the second and third greater than the sum of the four next; and half the sum of those four greater than the sum of the next eight, &c. *in infinitum*. For $\frac{1}{2}\, dD = br + bn$; but $bn > fG$, therefore $\frac{1}{2}\, dD > br + fG$, &c. And in the second series, half the second term is less than the sum of the two next, and half this sum less than the sum of the four next, &c. *in infinitum*.

Then the first series are the even terms, viz. the 2d, 4th, 6th, 8th, 10th, &c. and the second, the odd terms, viz. the 1st, 3d, 5th, 7th, 9th, &c. of the following series, viz. $\frac{1}{1\times 2}, \frac{1}{2\times 3}, \frac{1}{3\times 4}, \frac{1}{4\times 5}, \frac{1}{5\times 6}, \frac{1}{6\times 7}$, &c. *in infinitum* $= 1$. Whereof a being put for the number of terms taken at pleasure, $\frac{1}{a^2+a}$ is the last, $\frac{a}{a+1}$ is the sum of all those terms from the beginning, and $\frac{1}{a+1}$ the sum of the rest to the end.

Also that $\frac{1}{4}$ of the first term in the third series, is less than the sum of the two next; and a quarter of this sum, less than the sum of the four next; and one fourth of this last sum, less than the next eight, I thus demonstrate.

Let $a =$ the 3d or last number of any term of the first column, viz. of divisors.

$$\frac{1}{a \times a-1 \times a-2} = \frac{1}{a^3 - 3a^2 + 2a} = \frac{16a^3 - 48a^2 + 56a - 24}{16a^6 - 96a^5 + 232a^4 - 288a^3 + 184a^2 - 48a} = \mathrm{A}$$

$$\left.\begin{array}{l} \dfrac{1}{2a \times 2a-1 \times 2a-2} = \dfrac{1}{8a^3 - 12a^2 + 4a} \\ \dfrac{1}{2a-2 \times 2a-3 \times 2a-4} = \dfrac{1}{8a^3 - 36a^2 + 52a - 24} \end{array}\right\} = \frac{16a^3 - 48a^2 + 56a - 24}{64a^6 - 384a^5 + 880a^4 - 960a^3 + 496a - 96} = \mathrm{B}$$

$$\frac{64a^6 - 384a^5 + 928a^4 - 1152a^3 + 736a^2 - 192a}{64a^6 - 384a^5 + 880a^4 - 960a^3 + 496a^2 - 96a} \times \tfrac{1}{4}\mathrm{A} = \mathrm{B}.$$

And $48a^4 - 192a^3 + 240a^2 - 96a =$ excess of the numerator above the denominator.

$$\left.\begin{array}{rrcr} \text{But the affirmatives} & & > & \text{the Negatives} \\ \text{That is,}\ 48a^4 + 240a^2 & & > & 192a^3 + 96a \\ \text{Because}\ a^4 + 5a^2 & & > & 4a^3 + 2a \\ \text{Or}\ a^3 + 5a & & > & 4a^2 + 2 \end{array}\right\} \text{if } a > 2.$$

Therefore $\mathrm{B} > \frac{1}{4}\mathrm{A}$.

respondence on mathematical subjects with Dr. Wallis, by whom this correspondence was published in his Commercium Epistolicum, 1658. Lord Brounker was one of the personages who signed the remarkable declaration concerning King Charles II, in 1660. After the restoration, he was appointed Chancellor and Keeper of the Great Seal to the Queen, also a Commissioner of the Navy, and Master of St. Katharine's Hospital, near the Tower of London. He was one of those learned men who laid the foundation of the Royal Society, of which he was, by charter in 1662, appointed the first President: which office he held, with great honour to himself and benefit to the Society, till the anniversary election, Nov. 30, 1677, when he was succeeded in the presidency by Sir Joseph Williamson. He died April 5, 1684.—Lord Brounker made several communications to the

Therefore $\frac{1}{4}$ of any number of A, or terms, is less than their so many respective B, that is, than twice so many of the next terms Q. E. D.

By any one of which three series, it is not hard to calculate as near as you please, these and the like hyperbolic spaces, whatever be the rational proportion of *A E* to *B C*. As for example, when *A E* is to *B C*, as 5 to 4, (whereof the calculation follows after that where the proportion is, as 2 to 1, and both by the third series.)

First then when (in fig. 1) A E : B C :: 2 : 1.

2 × 3 × 4)	1.	(0,0416666666—	0,0416666666
4 × 5 × 6)	1.	(0,0083333333—	0,0113095237
6 × 7 × 8)	1.	(0,0029761904—	
8 × 9 × 10)	1.	(0,0013888888—	0,0029019589
10 × 11 × 12)	1.	(0,0007575757—	
12 × 13 × 14)	1.	(0,0004578754—	
14 × 15 × 16)	1.	(0,0002976190—	
16 × 17 × 18)	1.	(0,0002042484—	0,0007306482
18 × 19 × 20)	1.	(0,0001461988—	
20 × 21 × 22)	1.	(0,0001082251—	
22 × 23 × 24)	1.	(0,0000823452—	
24 × 25 × 26)	1.	(0,0000641026—	
26 × 27 × 28)	1.	(0,0000508751—	
28 × 29 × 30)	1.	(0,0000410509—	
30 × 31 × 32)	1.	(0,0000336021—	
32 × 33 × 34)	1.	(0,0000278520—	0,0001829939
34 × 35 × 36)	1.	(0,0000233426—	
36 × 37 × 38)	1.	(0,0000197566—	
38 × 39 × 40)	1.	(0,0000168691—	
40 × 41 × 42)	1.	(0,0000145180—	
42 × 43 × 44)	1.	(0,0000125843—	
44 × 45 × 46)	1.	(0,0000109793—	
46 × 47 × 48)	1.	(0,0000096361—	
48 × 49 × 50)	1.	(0,0000085034—	
50 × 51 × 52)	1.	(0,0000075415—	
52 × 53 × 54)	1.	(0,0000067193—	
54 × 55 × 56)	1.	(0,0000060125—	
56 × 57 × 58)	1.	(0,0000054014—	
58 × 59 × 60)	1.	(0,0000048704—	
60 × 61 × 62)	1.	(0,0000044068—	
62 × 63 × 64)	1.	(0,0000040002—	

0,0416666666
0,0113095237
0,0029019589
0,0007306482
3)0,0001829939(0,0000609980
0,05679179
+0,00006100
0,05685279 < EdCy

But 0,0007306482
0,0001829939 } ÷
0,0000458315

Therefore 0,05679179
+0,00004583
+0,00001528
0,05685290 > E d C y.

Royal Society; particularly, experiments concerning the recoiling of guns; also the series for the quadrature of the hyperbola, in the article above, being the first invented series of the kind on that subject, besides some papers not printed. Several of his letters to Archbishop Usher were also printed in Usher's Letters; as well as some to Dr. Wallis, in his Commercium Epistolicum, before mentioned.

For it has been demonstrated that $\frac{1}{4}$ of any term in the last column, is less than the term next after it; and therefore that $\frac{1}{3}$ of the last term, at which you stop, is less than the remaining terms; and that the total of these is less than $\frac{1}{3}$ of a third proportional to the two last.

And therefore ABCyE being = 0,75. 0,75
and E d C y > 0,05685279. . . . . . and < 0,05685290

And A B C d E is < 0,69314720. . . . . . and > 0,69314709

But when A E : B C :: 5 : 4, or as E A to K H, then will the space A B C E, or now the space A H K E (A H = $\frac{1}{4}$ A B) be found as follows:

8 × 9 × 10) 1. (0,0013888888			0,0013888888
16 × 17 × 18) 1. (0,0002042484	} 0,0003504472		0,0003504472
18 × 19 × 20) 1. (0,0001461988			3)0,0000878204(0,0000292735
32 × 33 × 34) 1. (0,0000278520	} 0,0000878204		0,0018271564
34 × 35 × 36) 1. (0,0000233426			+0,0000292735
36 × 37 × 38) 1. (0,0000197566			0,0018564299 < E a b
38 × 39 × 40) 1. (0,0000168691			

But 0,0003504472
0,0000878204 } ∺
0,0000220073 7

Therefore 0,0018271564
+0,0000220074
+0,0000073358

0,0018564996 > E a b

Therefore E M b (fig. 4) being = 0,025. 0,025
E a b > 0,0018564299 and < 0,0018564996

EMba (fig. 4) or EKM (fig. 1) > 0,02685643 and < 0,02685650
A H K M < 0,22314356 and > 0,22314349

Therefore 3A B C d E = 2,07944154
and A H K E = 0,2231435

ABCdE (when AE : BC :: 10 : 1) = 2,3025850

Therefore the logar. of 10 is to the log. of 2 as 2,302585 to 0,693147.

Extract of a Letter (written in Latin) by Mr. MICHAEL BEHM, Consul at Dantzick, to Mr. JOHN HEVELIUS, concerning some Chemical, Medicinal, and Anatomical Subjects. N° 34, p. 650.

In the beginning of this letter Mr. Behm conveys a well-merited eulogium on Mr. Boyle's Treatises on Colours and on Fluidity and Solidity, of which he had met with a Latin translation. At the same time he expresses a wish that the illustrious author would publish his further experiments on the nature of saline bodies. He then observes that he entertains great hopes of finding out a liquor, which, when injected into the bladder, may dissolve calculi without irritating the bladder itself; also of finding out other mixtures which may prevent or attenuate various viscidities that are injurious to the stomach. He afterwards mentions, that during an attack of the gout, in which he was disengaged from political concerns, he made some experiments on blood, the serum of which he found to be coagulable by a gentle heat and by acids, in the same manner as the white of an egg; but that it remained fluid when alkalies were mixed with it. He next enters upon a theory of the gout, which he supposes to be owing to a urinous acrimony, (urinosa putrilago) not eliminated by the kidnies or by perspiration from the mass of blood, but carried along with it and deposited upon the ligaments, joints, &c.* He hopes this subject will be more thoroughly investigated by some ingenious person in England; so that the gout may cease to be acknowledged even by physicians themselves to be an incurable affection. After noticing the inefficacy of the usual remedies, and the salutary operation of the warm mineral springs (especially of such as are diuretic) he remarks that he had experienced considerable relief from bathing the affected limbs with a liquor of his invention, which exactly resembled in smell, taste, and other properties, those warm mineral waters. Moreover he derived great benefit from some pills which were diuretic without being purgative; but (contrary, he says, to the advice of physicians) he has found in his own person and in the instances of his friends, the application of blisters to the part affected to give the speediest and most effectual relief; though he would not recommend this remedy to those whose skin is liable to ulcerate badly. Then follow some remarks on Highmore's Anatomical Account of the Spleen, and on Sylvius's absurd theory of the effervescence of the pancreatic juice, (which he supposed to be of an acid nature) with the bile in the duodenum, being the cause of numerous diseases. Mr. Behm says he never could detect any acidity in the juice of the

* The composition of gouty depositions has been accurately analysed by modern chemists, as will be seen in the subsequent volumes of this work.

pancreas, and that he never observed any effervescence to take place on mixing acids with the bile; but that in such experiments a precipitate was thrown down (in the form of a coagulum*) similar to the precipitation of lac sulphuris by acids. Hence he conjectures that the bile is subservient to chylification, by correcting the acid fermentation of the food.†

On the Variety of the Annual Tides in several Places of England. By Dr. Wallis. N° 34, p. 652.

In my Hypothesis for Tides I cast the annual high tides not on the two equinoxes, about the 11th of March and September, nor yet on the apogee and perigee of the sun, about the middle of June and December, but as proceeding from a complication of those two causes, on a middle time between the perigee and the two equinoxes, as is the greatest inequality of the natural days, proceeding from a complication of the same causes. And particularly for the coast of Kent, about the beginning of November and February; which agrees with observations on those coasts.

The last year an account was brought us of the annual high tides on the Severn, and at Chepstow-bridge, to be about the beginning of March and the end of September. Which though they agree not with the particular times on the coast of Kent, yet in general they agree thus far, that the one is about as much before the one equinox as the other is after the other equinox. You now acquaint me with high tides about February 22, about the coast of Plymouth, which is later than that of the coast of Kent, but sooner than that on the Severn. And I doubt not but in other parts of the world will be found other varieties.

The reasons of these varieties are to be attributed to the particular position of those parts, rather than to the general hypothesis. The general hypothesis of the earth's diurnal motion, from west to east, would cast that of the waters, not following so fast, from east to west; which causes the constant current within the tropics where the circles are greatest, westward from the coast of Africa to that of America, which is also the cause of the constant eastern breeze blowing in those parts. But the sea thus beating on the coast of America, is cast back as with an eddy on either hand, and consequently returns from the American shore eastward towards the coast of Europe; where, the parallel circles to the equator being less, and consequently the diurnal motion slower, it doth not cast

* The precipitate here mentioned would consist of albumen, and perhaps some of the resinous parts of the bile.

† This idea of bile correcting the acid fermentation of the food is consistent with its known composition, soda being obtained from it by chemical analysis.

the waters so strongly westwards as between the tropics, and so not strong enough to overcome the eddy, which it meets with from the other motion, which gives the sea a north-easterly motion, on these coasts, as to its usual course. The current therefore of our seas being north-easterly, we are next to consider, at what times it runs more to the north, and at what more to the east. When it runs most northerly it runs up the Irish sea, and so up the Severn; when most easterly it runs straight up the channel, and so to the coast of Kent; when between these, it beats against Devonshire and Cornwall, and those parts. We are therefore to consider, as to the annual periods, that the annual motion of the earth in the zodiac, and the diurnal in the equator, are not precisely in the same direction, but make an angle of $23\frac{1}{2}$ deg. at the equinoxes, but run, as it were, parallel at the solstices; and as they be nearer or farther from these points, so is the inclination varied. Which several directions of motion cause the compound motion of both to vary from the east and west more or less, according as the sun's position is farther from or nearer to the solstices. And therefore, nearer to the equinoxes, this inclination casts the constant current of our seas more to the north and south; and further from it, more to the east and west. Which is the reason why the current up the Irish sea is nearer to the equinoxes at the beginning of March and end of September, and up the Channel or narrow seas, farther from it, at the beginning of February and of November; and against the coasts of Devonshire and thereabout, at some inter mediate time.

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Time of the Tides observed at London. Addressed to Dr. Wallis. By Mr. Henry Philips. N° 34, *p.* 656.

The true time of the tides is very rudely and slightly estimated by most seamen and astronomers; most of them reckoning as if the moon being upon such a set point of the compass as the seaman calls it, or so many hours past the meridian as the almanack makers reckon, it were high tide in such and such a port at all times of the moon. And thus they reckon the tides every day to differ constantly 48m. As for instance: A south-west moon makes a full tide at London, that must be understood, that it is high tide at London when the moon is three hours past the meridian. Now this is true indeed at the new and full moon, but not at other times of the moon, which few take any notice of. But observing this more narrowly, I find that at London the tides fall out at the least two points, that is one hour and a half, sooner in the quarters, than in the new and full moon. Now this being a very considerable difference of time, which might very well make many seamen and passengers to lose their tides, I

* Omitted here a letter from Dr. Wallis, pointing out a mistake in a book, viz. Specimena Mathematica, by Franciscus Du Laurens.

set myself to watch this difference of the time of the tides, and to find out some rule how to proportion the time of the tides between the spring tides and the neap tides, and I found by many trials, that the true time of the tides might be found out to be somewhat shorter and shorter from the new and full moon unto the quarters; yet not in an equal manner, neither gradually decreasing from the new and full moon until the quarters; but rather that there was some little difference of alteration, both at the new and full moon, and also at the quarters; and that the greatest difference fell out in the midst between them, agreeing very well to a circular proportion after this manner: (See fig. 5, pl. 7.)—First, Divide a circle into 12 equal parts, or hours, according to the moon's motion or distance from the sun, from the new moon to the full.—2d. Let the diameter of the circle be divided into 90 parts or minutes, that is, according to the time of the difference of tides between the new or full moon, and the quarters; which is one hour and a half.—3d. Make perpendicular lines cross the diameter of the circle, from hour to hour.—4th. Reckon the time of the moon's coming to the south in the circumference of the circle, and observe the perpendicular line that falls from that point on the diameter; then the proportional minutes cut thereby, will show how many hours or minutes are to be subtracted from the time of high tides at the new and full moon, that you may have the true time of the tides that present day.

For example: At London, on the day of new and full moon, it is high tide there at three o'clock, that is when the moon is three hours past the meridian: so that by the common rule, the moon being about four days old, it will be south about three o'clock, and it will be high tide three hours afterwards, that is at six o'clock. But now by this rule, if you count this time of the moon's coming to the south in the circumference, the perpendicular line which comes from three to nine cuts the diameter in the half, or at 45 m. which shows that so much is to be abated from the time of high tide in the new and full moons. So that it is high tide 45 min. before six o'clock, that is, at five hours 15 min. and not at six o'clock, according to the common rule.

The like you may do for any other port or place, knowing the time of high water at the new and full moon in that place: And it will be done the more readily, if you set down the time of high water at the new and full moon under the diameter, as I have done for London, where it is high tide at three o'clock. So that when the moon is south at three o'clock, the perpendicular cuts the diameter at 2 hours 15 m. which added to the time of the southing, makes it 5 hours 15 m. and so when the moon is south at nine o'clock, by adding 2 h. 15 m. you have the time of high water, which is 11 h. 15 m.

And thus you may easily make a table, which by the southing of the moon shall readily show the time of high tide at any time of the moon.

An Account of some Books. N° 34, p. 660.

I. W. Sengwerdius, P. D. de Tarantula. Lugd. Bat. 1668, 12mo.

As the subject of this treatise will present itself more than once in the subsequent volumes of these Transactions, (See Lister in vol. vi. and Cirillo, in vol. lx.) we shall only observe on the present occasion, that the very extraordinary effects formerly attributed to the bite of this insect, and minutely described by this author, together with the pretended cures by music and dancing, are now justly referred to imposture in many instances, and to the influence of the imagination in others. It is nevertheless true that the bite of the aranea tarantula is not wholly innoxious. On the genus aranea, see Linnæan Trans. vol. ii.

II. Regneri de Graaff,* M. D. Epistola, de nonnullis circa Partes Genitales Inventis Novis. Lugduni Batav. 1668. (See also N° 38).

III. Johannis Van Horne,† M. D. Observationum circa Partes Genitales in utroque Sexu, Prodromus. Lugd. Batav. 1668. (See also N° 38).

De Graaf, 1. Rejects the opinion of those that teach a conjunction of the seminal arteries with the veins by visible anastomoses, and that reckon the testicles among glands. 2. He affirms that he has often unravelled the whole substance of the testicles to a great length. 3. He asserts to have showed in a clear manner the communication of the vesiculæ seminales with the vasa deferentia, and the size, shape and termination of these last in the urethra. To which he says he has added a very easy way of examining the body of the prostatæ. From

* Regner de Graaf was born at Schoonhaven in 1641; he studied at Leyden under De le Boe Sylvius and Van Horne; but took his doctor's degree at Angers, and practised at Delft. He was author of the following anatomical treatises: De Succi Pancreatici Natura, 1664 and 1666; De Virorum Organis Generationi Inservientibus, 1668; De Mulierum Organis Generationi Inservientibus, 1672; Defensio Partium Genitalium, 1673. These were collected into one 8vo. vol. and reprinted after his death under the title of Opera Omnia, Leid. 1677. He died prematurely when only 32 years of age, in consequence, as is supposed, of great uneasiness of mind, brought on by the warm disputes in which he was involved with Swammerdam. In his tract on the pancreatic juice, he gives an account of a very difficult anatomical experiment which he performed on a living dog, opening the abdomen, and inserting a tube into the pancreatic duct, for the purpose of collecting the juice thereof; to which (with Sylvius) he ascribed acid properties. By his other writings he threw considerable light on the structure and uses of the different parts belonging to the organs of generation in both sexes.

† Van Horne was a native of Amsterdam; but he spent many years in Italy, practising at Venice, Padua, and other places. On his return to Holland he was appointed to the anatomical and chirurgical professorships at Leyden, where he bestowed great pains on the art of injecting the vessels, preparing the various parts of the body, and making anatomical drawings. He wrote several anatomical treatises, (such as his Novus Ductus Chyliferus, 1652; De Ductibus Salivalibus, 1656; Microcosmus seu Manuductio ad Histor. Corp. Hum. 1660; Prodromus Obs. circa Partes Genitales, 1668, &c.) all which have been since reprinted in one 8vo. vol. under the title of Opuscula Anatomico-chirurgica, Leips. 1707. He died at Leyden in 1670, being only 49 years of age.

the consideration of all which he concludes, that there is but one material of the seminal liquor, which after being secreted in the testicles is deposited in the vesiculæ, and ejected from thence into the urethra, not (as Vesling asserts) by one but by two foramina. 4. He affirms to have an easy and more accurate way of dissecting the penis than any other anatomist he knows; and he assigns to the muscles thereof a far other use than has been hitherto done. (See N° 38).

Van Horne refutes the above mentioned anastomoses between arteries and veins; then describes the spermatic arteries and veins; the pyramidal figure they make, where they meet near the testes, the direct and retrograde passage of the said arteries through the testes, and such a strange anastomosis between the spermatic veins, that they represent a kind of *rete mirabile* most elegantly. He also will not admit the testes to be glandular, but affirms, (which is the same with the doctrine of De Graaf) that the whole substance of the testicles is nothing more than a congeries of extremely minute tubes (the author uses the term funiculorum concavorum) wherein the semen is elaborated and conveyed; adding, that if the greater globe of the epididymis be well examined, there will appear through its membrane such anfractus and funiculorum gyri, as resemble those of the brain. He holds the semen to consist of three different fluids, one of which comes from the testes, another from the vesiculæ, and a third from the prostate glands. He deduces from the wonderfulness of the structure of the penis, tensionem ejus, et impetuosam seminis per eundem ejaculationem.—He asserts, with Steno, that mulierum testes esse ovario in oviparis analogos, they containing perfect eggs full of liquor, and encompassed with a skin of their own.*

Observations on Deafness. By the Rev. Dr. William Holder.† *N° 35, p. 665.*

A young gentleman was born deaf, and continued dumb till the age of 10 or 11 years. His mother, when pregnant with him, received a sudden fright; by

* Further information relative to the male organs of generation may be obtained by referring to Haller's Observ. de Viis Seminis in the 46th volume of the Philosophical Transactions, and to Monro's Dissert. de Testibus. Concerning the uses of the vesiculæ seminales, the late Mr. J. Hunter entertained some singular opinions, not warranted by the structure and connexion of these parts in the human subject. See his Observations on certain Parts of the Animal Economy. Beautiful specimens of the serpentine vessels or seminiferous tubes of the testicles, filled with quicksilver, are to be seen in the private anatomical museums of this metropolis.

† Dr. Holder, a learned divine, and skilled in the sciences, was rector of Blechingdon in Oxfordshire. At the restoration he took his doctor's degree, became subalmoner to the king, and a fellow of the Royal Society. He had great skill in teaching deaf persons to speak, and wrote a treatise on that art. He was also well skilled in music, and published a treatise on the Natural Grounds and Principles of Harmony, in 8vo. 1694. His other works are, A Discourse on Time, in 8vo. Also some controversial papers against Dr. Wallis, on the art of teaching deaf persons to speak. Dr. Holder died in 1697.

occasion of which the child's head and face were a little distorted, the whole right side being somewhat elevated and the left depressed, so that the passage of his left ear was quite shut up, and that of the right ear proportionally distended and too open. This gentleman being for some time recommended to my care, amongst other things, I spent some thoughts in searching the cause of his deafness in the ear whose passage was open. And having found that the auditory nerve was not perished, but that he could hear the sound of a lute-string, holding one end thereof in his teeth, and had some perception of any very vehement sound, I supposed the defect to lie in the want of due tension of the tympanum of his ear; whose use I took to be only to preserve the auditory nerve and brain, and inward parts of the ear from outward injury by cold, dust, &c. and to be no more to hearing than glass in the window is in a room to seeing, *i. e.* as the one intromits light without cold or offence to those in the room, so the other permits sound to pass; and shuts out what else might offend the organ; as appears in the experiment of breaking the tympanum of a dog, who hears never the worse for some few weeks, till other causes, as cold, &c. vitiate the organ.*

But for the free passage of the sound into the ear, it is requisite that the tympanum be tense and hard stretched; otherwise the laxness of that membrane will certainly deaden and damp the sound.

Now as to the case of the young gentleman before mentioned, I supposed the requisite degree of tension of the tympanum to be wanting; and that if by any remedy it could be restored, I assumed that he might recover his hearing in that ear: to which end, I advised his mother to consult with learned physicians.

Mons. L'ABBE MARIOTTE'S† *new Discovery touching Vision; with Mons.* PECQUET'S *Answer: both communicated by Mons.* JUSTEL. *N°* 35, *p.* 668.

Having often observed in anatomical dissections of men as well as brutes, that the optic nerve does never answer just to the middle of the bottom of the

* The fact here mentioned of the hearing being impaired in experiments made on dogs, a few weeks after the perforation of the membrana tympani, would be an objection to the modern chirurgical remedy against deafness, (See Astley Cooper in the Phil. Trans. for 1800 and 1801) were it not possible to counteract, in a great measure, the effects of cold and other injurious impressions of the atmosphere, by wool or cotton occasionally introduced into the cavity of the ear.

† Edmund Mariotte, an eminent French philosopher and mathematician, was born at Dijon, and died in the year 1684. Mariotte became prior of St. Martin near Dijon, and a member of the Academy of Sciences of Paris in 1666, to which he communicated a number of curious and valuable papers, which were printed in the collection of their memoirs in 1666, viz. from vol. i. to vol. x. But all his works were collected into 2 vols. 4to, and printed at Leyden in 1717. Mariotte was a

eye, *i. e.* to the place where is made the picture of the objects we directly look on; and that in man it is somewhat higher, and on the side towards the nose; to make therefore the rays of an object fall upon the optic nerve of my eye, and to find the consequence thereof, I made this experiment:

I fastened on an obscure wall, about the height of my eye, a small round paper to serve me for a fixed point of vision; and such another on the side towards my right hand, at the distance of about two feet; but somewhat lower than the first, that it might strike the optic nerve of my right eye, while I kept my left shut. Then I placed myself over against the first paper, and drew back by little and little, keeping my right eye fixed and very steady upon the same; and being about ten feet distant, the second paper totally disappeared.

That this cannot be imputed to the oblique position of the second paper, is evident, as I can see other objects further to the side of it; so that one would believe that the second paper was taken away, if one did not find it again by the least stirring of the eye.

This experiment I made often, varying it by different distances, and removing or approaching the papers to one another proportionally. I made it also with my left eye, by keeping my right shut, after I had fastened the second paper on the left side of my point of vision, so that from the situation of the parts of the eye it cannot be doubted but that this deficiency of vision is upon the optic nerve.

This discovery I communicated to many of my friends, who found the same thing, though not always at the same distances; which difference I ascribed to the different situation of the optic nerve. Some at the distances mentioned have lost sight of a paper eight inches large, but others not so soon, which must be caused by the different magnitudes of the optic nerve in different eyes.

This experiment has given me cause to doubt, whether vision was indeed performed in the retina (as is the common opinion) or rather in that other membrane which at the bottom of the eye is seen through the retina, and is called the choroides. For if vision were made in the retina, it seems that then it should be made wherever the retina is; and since the same covers the whole nerve, as well as the rest of the bottom of the eye, there appears no reason to me why there should be no vision in the place of the optic nerve where it is:

good mathematician for the age he lived in, and was the first French philosopher who applied much to experimental physics. The law of the shock or collision of bodies, the theory of the pressure and motion of fluids, the nature of vision, and of the air, more particularly engaged his attention. And he was remarkable for carrying into his philosophical researches that spirit of scrutiny and investigation, so necessary to those who would make any considerable progress in discovery or improvement.

on the contrary, if it be in the choroides that vision is made, it seems evident that the reason why there is none on the optic nerve, is because that membrane (the choroides) parts from the edges of the said nerve, and does not cover the middle as it does the rest of the bottom of the eye.

The following is M. Pecquet's Answer to the above.

Every one wonders that no person before you has been aware of this privation of sight, which every one finds, now you have given notice of it. But as to the result you draw from this discovery, I see it not sufficiently cogent to abandon the opinion of the retina being the principal organ of vision. For it will be sufficient now to take notice, that at the place of the optic nerve there is something that may very well cause this loss of the object. There are the vessels of the retina, the trunks of which are large enough to intercept the vision. These vessels, which are only the ramifications of the veins and arteries, are derived from the heart, and having no communication with the brain, they cannot carry thither the species of the objects. If therefore the visual rays, issuing from an object fall on these vessels at the place of their trunk or main body, it is certain that the impression made thereby will produce no vision, and that the picture of that object will be deficient; as when on a white paper in an obscure chamber there is some black spot, or in it some hole of a considerable size: for the more sensible this blackness or hole is, the more of the image of the object it intercepts from our eyes. It is not so in respect of the small ramifications that issue from those trunks, and shoot into the retina. For if they be met with at the place of the bottom of the eye where vision is made distinct, they will not render the image of the object deficient, because they are so small as not to be sensible. Thus it is, that in looking-glasses, when they want lead or tin in any place large enough to be perceived, the image we there see appears to have a hole; which happens not when there is but so small a one as might be made by the point of a needle.

Thus much being observed as to the deduction made from this experiment, I shall further note, that that paper, the sight whereof is lost, must be further off or nearer, according to the diversity of the structure of eyes. For some lose this paper at the distance of two feet, some at a less, others at a greater distance; some lose it a little higher, others a little lower, according as the trunks of the vessels are situated in respect of the optic nerve; and some lose more of it than others, according as those vessels are larger or smaller. And because it is hard to determine precisely the place where the object is lost in all sorts of eyes, we have ground to believe that this loss is not always made on the extent of the nerve where the retina is, but sometimes on the side of it where the choroides is found. For the trunks of the vessels of the retina are large and long enough to extend on this or that side of the nerve, and so to hide some part of the choroides according to their magnitude. And in this case it will be true that vision is not made in all the parts where the choroides is found, though they be exposed to the light. Which may very well give a check to your opinion, forasmuch as those trunks would hinder the objects falling on them from coming to the choroides; which would render the image deficient in that place, in regard that those species would not be able to make an impression on the organ of vision through those vessels.

In the mean time such a discovery as this could not be long without being confirmed. For as the secret of your experiment consists in contriving that the picture of an object may fall just on the optic nerve, or thereabout, M. Picard has devised a way by which an object is lost keeping both eyes open, by letting the image of the object fall on both the optic nerves at one and the same time, after this manner:

Fasten against a wall a round white paper of the size of an inch or two, and on the side of this paper put two marks, one on the right the other on the left side, each about two feet distant; then place yourself directly before the paper, at the distance of about nine feet, and put the end of your finger opposite both eyes, so that it may hide from the right eye the left mark, and from the left eye the right mark. If you remain firm in that posture, and look steadily with both eyes on the end of your finger, the paper, which is not at all covered thereby, will altogether disappear; which must be the more surprising, because without this particular encounter of the optic nerves, where no vision is made, the paper would appear double, the reason of which is sufficiently known.

The application of this method is easily made to that of yours. For when one looks steadily with both eyes on the end of one's finger, held before the marks, it is the same thing as if you directed each eye by itself to the place which is to be looked on to lose the paper; so that one may with both eyes do the same thing, that you do with one, keeping the other closed.

A Letter from Dr. Timothy Clarck, one of his Majesty's Physicians in Ordinary, concerning some Anatomical Discoveries and Observations, particularly the Origin of the Injection into Veins, the Transfusion of Blood, and the Organs of Generation. N° 35, p. 672.

After some prefatory remarks on the state of medicine and physiology in his days, Dr. Clarck animadverts upon the abuse of experimental philosophy; by which some (he is persuaded) have acquired a temporary unmerited fame. From this charge, however, he excepts, among anatomists, Asellius,* the discoverer (though, as Asellius himself acknowledges, the accidental discoverer) of the lacteals; Pecquet,† the discoverer of the thoracic duct; and Bartholine ‡ and Rudbeck,§ who by chance hit upon the lymphatics while they were busied in tracing the lacteals; though Dr. C. asserts that his countryman Jolif, while he was examining the spermatic vessels in 1652, tying a ligature above, and with his hand squeezing the testicle and its involucra beneath, in order to render the blood vessels more turgid, unexpectedly saw for the first time the lymphatic vessels in like manner rendered turgid. These observations are followed by some

* Caspar Asellius was born at Cremona, and taught anatomy in the university of Pavia, with great celebrity, in the beginning of the 17th century. In 1662 he discovered the lacteals in the mesentery of a dog. He drew up an accurate description of these vessels, illustrated by coloured plates, in a work entitled Dissertatio de Lactibus seu lacteis Venis, published at Milan, in 1627, the year after his death. These vessels had been seen by Erasistratus many centuries before, in the mesentery of goats; but no farther notice had been taken of them until they were again detected by Asellius in his anatomical investigations.

† Some biographical notices of this anatomist have been given at p. 163, vol. i. of this Abridgement.

‡ Thomas Bartholine, one of the greatest anatomists of the 17th century, was the son of Caspar Bartholine, (a man of universal erudition, and equally distinguished as a theologian, a philosopher, and physician) and was born at Copenhagen in 1616. He spent many years at foreign universities, travelling through Holland, France and Italy. On his return to Copenhagen in 1647, he was at first made professor of mathematics, but afterwards filled the anatomical and medical chair in that university. In this situation he discovered, in conjunction with Rudbeck, the lymphatic vessels. He also traced the course of the thoracic duct in the human subject, confirming and elucidating Pecquet's description thereof. His anatomical and medical writings are very numerous. Next to his tracts concerning the lymphatics, lacteals, and thoracic duct, the chief are his edition of his father's Institutiones Anatomicæ, with notes and copious additions of his own; his Collegium Anatomicum; his Historiæ Anatomicæ; his Epistolæ Anatomicæ; his Dissertatio de Medicina Danorum; his Cista Medica, and his Orationes Varii Argumenti. He was besides a principal contributor to the Acta Medica and Philosophica Hafniensia. The number of his works would have been still greater, had not his library and MSS. been destroyed by fire in 1670. This celebrated man died in 1680, aged 64. Some years before his death he was appointed physician to Christian the Vth, was made rector of the university of Copenhagen, and had other honours conferred upon him.

§ Olaus Rudbeck was born at Arosia in Westmania, a province of Sweden, in 1630. He was professor of physic in the university of Upsal, and founder of the botanic garden there. The lymphatics of the liver (which he called ductus hepatis aquosos) were discovered by him and Bartholine jointly. He undertook, with the assistance of his son, a magnificent botanical work in folio, entitled Campi Elysii; but lost most of his MS. in the great conflagration which happened at Upsal in 1702, during which year he died, aged 73. Besides his anatomical Exercitatio exhibens ductus hepatis aquosos, 4to. 1653, and the botanical undertaking above mentioned, he also wrote a large historical and archaiological work, entitled Atlantica sive Manheim, in 3 vols. folio. It is said that he performed the Cæsarian operation upon his own wife, so successfully as to save both mother and child. This was certainly an instance of great chirurgical intrepidity as well as skill. Fragments of the Campi Elysii were published some years ago by Dr. E. Smith, purchaser of the Linnæan Museum, founder of the Linnæan Society in London, and one of the first botanists of the age.

reflections on Bartholine, for his insinuations that Pauli, of Venice, was acquainted with the circulation of the blood before Harvey * published his account of it; whereas it is certain that whatever knowledge Pauli (between whom and Harvey there was a long established friendship) might have had of this subject, it must have been derived through the medium of the Venetian envoy, then in England. He afterwards proceeds to observe, that although he had been diligently engaged for several years past in mixing various liquors with the blood of living animals, and had infused into the circulating mass not only a variety of alimentary drinks (potulenta) to the quantity of 2 lbs. but had likewise made similar trials with emetics, cathartics, diuretics, cardiacs, and opiates, as well as the transfusion of blood itself; yet he confesses he still entertains many doubts as to the utility (and in some instances the safety) of such experiments, in a medical point of view. At the same time he is ready to allow that the infusing of different liquors into the veins of animals may answer some anatomical purposes, and tend to throw light upon the nature and composition of the blood. He is further of opinion, that in sudden and profuse hæmorrhages the transfusion of blood may possibly have the effect of recruiting the exhausted frame in a speedy and powerful manner; in support of which opinion he appeals to the experiment witnessed by Mr. Oldenburg and himself, of an animal which was bled until it was seized with convulsions, and apparently in a dying state, being restored to its former vigour in the space of seven minutes, by transfusing into it the blood of another animal of a different species. Nevertheless he doubts much of the applicability of transfusion of blood to the cure of diseases in general, and particularly of the possibility of conferring, by its means, upon persons far advanced in years, the health and vigour of youth. In regard to the infusing of nutrimental or medicated liquors into the veins, he justly suspects that no such liquors can be really beneficial, unless they previously undergo those changes in the first concoctions which render them fit for being mixed with the circulating mass. Much of what follows relates to the priority of the discovery (claimed by the French and other foreigners) of the transfusion of blood, and the injecting of medicated liquors into the veins. Dr. Clarck shows (as Mr. Oldenburg had done before) that both these experiments originated with the English; Dr. Lower having been the first who performed transfusion on brutes, and the French anatomist Dr. Denis the first who tried it upon man. The account of Dr. Lower's experiment was published in the Phil. Trans. for December 1666; but nothing was heard of Dr. Denis's operation until March

* An opportunity will hereafter occur of giving some account of this great physician's discoveries and writings.

in the following year, 1667. Dr. C. then rectifies an error into which Mr. Oldenburg had fallen, respecting the time when the experiment of infusing liquors into the veins was first tried by Dr. Christopher Wren; showing that it was performed in the house of the French ambassador, the Duc de Bourdeaux, in the year 1657, and not 1659, as would appear from Mr. Oldenburg's statement. And the experiment (he says) was several times repeated in the course of that same year. In regard to a certain Hamburgh physician who would attribute the invention of infusing liquors into the veins to his countrymen (the Germans), stating that he had heard of the experiment being made in the presence of a prince of the Palatinate; it is accounted for by the circumstance of the experiment having been performed before the Palatine Prince Rupert, in England, through whose correspondence the fame of it might easily pass into Germany. Moreover, Dr. Clarck remarks, that it appears from this physician's own words that he had never tried this operation (though he says he had thought of it) at the time he mentions.

Dr. Clarck then observes, that with the present letter he sends a drawing of the vasa deferentia and vesiculæ seminales, representing them in the state in which they were cut out of the human body by Lower and himself. He congratulates De Graaf,* or rather himself, that they both should have hit upon the same discovery. So evident, he says, is the communication between the vasa deferentia and vesiculæ seminales, that if a person injects a liquor into the vas deferens, not a single drop of it will get into the urethra before it has reached the upper extremity of the vesiculæ seminales. For in the angle A (see figure 1, plate 8), this communication is so contrived, that the vesiculæ seminales must be entirely filled before any of the liquor can make its way into the urethra. He allows that the semen is emitted into the urethra by two foramina; but he cannot readily assent to De Graaf's doctrine, that there is but one material of the seminal fluid (unam solummodo esse materiam seminis); for if the testicle differs in its structure, colour, and substance, from the epididymis, in the same manner as the epididymis differs from the prostate; and if in these several parts we meet with juices that are of a different consistence and colour, he infers that different materials of the seminal fluid are elaborated in them.

With regard to De Graaf's and Van Horne's assertion, that the substance of the testicle is nothing but a conglomeration of funiculi, or rather extremely minute tubes; this fact, Dr. Clarck remarks, was known to himself, as well as to Riolan† and others, before. But although these funiculi may be drawn out to

* Of whose anatomical labours an account has been already given at p. 241. (See also N° 38).

† John Riolan was the son of a physician of the same name, and was born in 1577 at Paris, where

a certain length, yet he says he could never find that the whole substance of the testes could be untwined, like yarn from the spindle.

In the concluding part of his letter, Dr. C. gives notice of his intention of communicating to the world his observations on the organs of generation, and on impregnation in the human subject, tracing its progress from the first fortnight to the seventh and eighth months; deduced from data, furnished partly by abortions, and partly by dissections of women dying at different periods of gestation.

Explanation of the Figures referred to in this Letter.

Figure 1 exhibits part of the vas deferens with the vesiculæ seminales of one side, as they appeared before they were cut out of the body. A, The angle of communication. B, The upper extremity of the vesiculæ seminales. C, The vas deferens where a small injecting pipe was inserted. D, The foramen which opens into the urethra. aaa, Part of the vas deferens. bbb, The vesiculæ seminales. ccc, The duct which leads from the vesiculæ into the urethra.

Fig. 2 represents a part of the vas deferens, with the vesiculæ seminales of the other side, after being cut out, inflated, and dried for preservation. The letters denote the same as in fig. 1.

Observations concerning the Comet which lately appeared in foreign Parts, communicated from Italy and Portugal. N° 35, p. 683.

The Italian account, given by Gio. Domin. Cassini:—

Anno 1668, the 10th of March, 1 h. of the following night, (after the Italian way of counting) at Bononia, S. Cassini observed a path of light extended from the Whale through Eridanus; which he judged to be the train of a comet, both by the figure and colour, as also because that the direction of it, being by the fancy continued, seemed to proceed to the 21st degree of Pisces, where the sun then was, and so tended to the part opposite to the sun, like other comets. By its extreme point reached to that star in Eridanus marked 14 by Bayer. But it issued out of the horizontal clouds, so that it seemed the head of the comet was either veiled by them, or hid under the horizon. It followed

he afterwards distinguished himself as a teacher of anatomy. He died in 1657, aged 80. He wrote a Schola Anatomica; Anatomica, Corp. Hum.; Osteologia; Anthropologia (afterwards republished with large additions under the title of Opera Anatomica); Enchiridion Anat.; besides several controversial tracts. He was a man of a most invidious and overbearing temper, and was almost constantly involved in disputes with contemporary writers, concerning some of the most important anatomical discoveries which were then made. He would not allow Pecquet and Bartholine the merit of their respective discoveries; and even attempted to refute Harvey's doctrine of the circulation of the blood.

the motion of diurnal revolution westward, and it was to be seen about the second hour of the night; for then it was demersed in the mists of the horizon.

Not far from its pointed end eastward, a star appeared, equal to the brightest of the fourth magnitude, almost in the same place where was observed the comet of 1664, December 31; which star was not then seen, nor at other times elsewhere, nor is described in any catalogue, or any globe or map; which therefore he deems to be a new one, that is, of new appearance.

March 11, in the evening, the horizon was in the west overcast with thin clouds; among which after one hour of the night, there was seen a brightness in the Whale, at least for half an hour, which was very like the splendour of Venus, likewise veiled by thin clouds.

March 12, at night, the lower parts of the heavens in the west were clouded, and when the great Dog-star was in the mid-heaven, the same tail appeared again. It passed through the star in Eridanus which Bayer calls the 15th, and left to the southward the 14th, where it terminated March 10. Being by the imagination drawn out to about three degrees and further, it tended to that southern star which precedes the ear of Lepus. It was therefore more northerly than the day before yesterday, and more easterly; and it also reached to the opposite part of the sun. The apparent part of the train reached out in length about 32 degrees.

So far the Italian relation; the following is that from Lisbon.

March 5, N. S. Forasmuch as it seems to follow the regular course of the sun, and sets few hours after it, there could hitherto be taken no considerable observations of it. The body is not seen, because it remains hid in the horizon. Its train is of a stupendous length, extended in appearance over almost the 4th part of the visible heaven, from west to east; its apparent breadth is of a good palm, and its splendour very great, but it lasts only a few hours.

Several letters written from France also mention its having been seen in several parts of that kingdom, as at Lyons, Tholouse, Toulon, but not at Paris; no more than it hath been observed at London, or in any other part of England yet heard of.

An Account of some Books. N° 35, p. 685.

I. Geometriæ Pars Universalis, Quantitatum Curvarum transmutationi et mensuræ inserviens, auth. Jacobo Gregorio Scoto, Patavii, 1668. In 4to.

This work and the other before noticed, on the quadrature of the circle and hyperbola, were both composed and printed in Italy, while the ingenious author was on his travels in that country. In the preface of the present work he observes, that the defect of algebra in the mensuration of curved figures may in some manner be supplied, if out of some essential property of any such figure thence be given a method of changing it into another equal figure, having known properties, and of that into another, and so on, till at last it be changed into some known quantity; which he says is effected in this work.

To square a circle organically, or divide an angle in a given ratio, he supposes there is no easier method, than by the common linea quadratrix, the properties whereof are treated at large in Leotaudi Cyclomathia, Lugduni, 1663, in 4to.

He then remarks that all things concerning logarithms, and the composition of ratios, may be performed by help of a curved line, drawn through the tops of a rank of lines in continual proportion, standing as perpendiculars on a right line and at equal distance, being the logistic or logarithmic curve. That however the operations performed thereby are not to be accounted geometrical, because they are not performed by the sole aid of rule and compass. The confirmation of which the author thus demonstrates, that no cubic equation irreducible to a quadratic, can be resolved by the sole aid of rule and compass. For every cubic equation has either only one real root or three real roots; hence if they could be found by the sole aid of rule and compass, or by the intersection of a circle and a right line, then a right line should cut a circle either in one point or three points; either of which is absurd. And for the like reason a cubic equation, having three real roots, can never be reduced to a pure equation which has only one root; for in these equations, it is impossible, by aid of any reduction, to change an imaginary root into a real one, and the converse.

The book itself contains these several heads:—1. The mensuration of sundry solids, with general methods for that purpose. He here cubes or measures either of the segments of a parabolical conoid cut by a plane parallel to the axis. —2. The mensuration or plaining of the surfaces of divers solids and spiral spaces unknown to antiquity, and not treated of by any modern authors, till of very late years; from whom the author differs in his method: particularly, he finds a circle equal to the surface of a parabolical or hyperbolical conoid, resembling a cup or bowl; viz. when the revolution is about their axes, Prop. 46 and 49. Also, the parabolical hour-glass or solid, when the revolution is about a tangent at the vertex, Prop. 52. Also the oblong spheroid, Prop. 47, 48; and Prop. 67, the surface of any segment of a cone. Generally it its shown, Prop. 36, that the surface of every round solid is equal to a rectangle, whose base is the circumference of the figure, by the rotation whereof the solid is generated, and the height equal to the circumference which the centre of gravity of the perimeter of the figure describes.—3. A method for straightening of curved lines in the first six propositions; and in particular he finds a right line equal to a parabolical curve, Prop. 51.—4. Divers optic propositions towards the end of the book, concerning the imperfection of the eye and the confusion of the sight; the apparent magnitude of the sun low and high; the tails of comets; what proportion the earth's illumination by the sun, at the full of the moon, bears to the illumination of the earth by the moon; and the like comparison between the sun and Sirius; that vision by aid of a telescope or microscope is not deceitful: and an observation of the similitude between the earth and the moon.

This same author, in his letter to Mr. John Collins, suggests, that Cassini has observed the motion of Jupiter about his axis in 10 hours; of Mars in 23 hours; that Venus has the like rotations, but the precise period not yet known. That Cassini has published tables of the motion of the satellites of Jupiter, with an ephemeris of the same for this present year. The like tables have been formerly published by the learned John Baptist Hodierna at Rome about 1656.—In another letter of this author to the same Mr. Collins, he states that Mich. Angelo Ricci only, since Viviani, has written de Maximis et Minimis in two sheets, but to extraordinary good purpose.

II. An Introduction to Algebra, translated out of High Dutch into English by Tho. Branker, M. A. much altered and augmented by Dr. John Pell. Also a table of such odd numbers as are less than one hundred thousand, showing those that are incomposite, and resolving the rest into their factors or coefficients. Printed at London in 4to.

The author of this book, in the German language, was J. H. Rohn. The method of it is new, containing much in a little, and each distinct step of ratiocination or operation has a separate line, the operations being registered in the margin. The author puts small letters for unknown quantities, and capitals for known ones.—The book consists of many excellent problems; some of which are such as Bachet either confesses he did not attain, or at least left obscure: and others of them are such as the celebrated Descartes and Van Schooten have left doubtful, as not being by them thoroughly understood. And some such as being unlimited, have for their answers certain ranks or series of all possible whole or rational numbers, whereby the student may be accomplished for the resolution of other questions of the like nature.

The remainder of Rohn's book, but omitted in this translation, treats of circular tangencies; also of the construction of equations by means of the circle and parabola; and of sines, tangents and secants, in 105 propositions.

III. An Essay towards a Real Character and a Philosophical Language, by John Wilkins, D. D. Dean of Ripon, and Fellow of the Royal Society.*

The description of the contents of this work, here given, is now no longer a curiosity, the book being in every person's hand.

IV. Stanislai de Lubienietz Theatrum Cometicum, duabus partibus constans.

Account of a Controversy between STEPHANO *de* ANGELIS, *Professor of the Mathematics at Padua, and* JOH. BAPTISTE RICCIOLI, *a Jesuit. Communicated by Mr.* JAMES GREGORY, *Fellow of the Royal Society. Translated in part from the Latin.* N° 36, *p.* 693.

Riccioli, in his Almagestum Novum, pretends that he has found out severa new demonstrative arguments against the motion of the earth. Steph. de Angelis, conceiving his arguments to be none of the strongest, takes occasion to let the world see that they are not more esteemed in Italy than in other places. Manfredi, in behalf of Riccioli, endeavours to answer the objections of Angeli; and this latter replies to Manfredi's answer. The substance of this dispute is as follows:

Although the arguments of Riccioli be many, yet the strength of them consists chiefly in these three.—1st. Bodies let fall through the air in the plane of

* Bishop Wilkins was one of the first institutors of the Royal Society, and one of its most useful members, as well as the first or principal secretary, under whom Mr. Oldenburg acted as the sub or copying secretary. He was well skilled in mathematical and philosophical literature, producing several useful works and inventions, one of which, though never noticed in any accounts of his life, was that of the perambulator, or surveying wheel, for measuring roads and great distances. Bp. Wilkins was a man who thought it prudent to submit to the powers in being; he therefore subscribed to the solemn league and covenant while it was enforced, and was equally ready to swear allegiance to King Charles when he was restored. He accordingly had favours and promotion from both parties; and, being of a good and amiable mind, he always used his power and interest for the benefit of individuals and of the public weal. Dr. Wilkins was born in 1614, and studied at Oxford, where he took his degrees. During the civil wars the parliament appointed him warden of Wadham College in 1648. In 1656 he married the sister of the Protector, Oliver Cromwell, and, by the son Richard Cromwell, was made master of Trinity College, Cambridge, in 1659; but was ejected on the restoration the year following. From the prudence of his conduct, however, and his superior learning and piety, he met with great encouragement and patronage, and at last was promoted to the see of Chester in 1668. Like most studious and sedentary men, he became much afflicted with the gravel, and at length died of the stone in 1672, at 58 years of age.

Of his publications, which are all of them very ingenious and learned, and several of them highly curious and entertaining, the first was in 1638, when he was only 24 years of age, viz. The Discovery of a New World, or a discourse to prove that it is probable there may be another habitable world in the moon; with a discourse concerning the possibility of a passage thither, In 1640, a Disthe equator, descend to the earth with a velocity constantly increasing as they fall. But if the earth were moved by a diurnal motion only about its own axis or centre, no heavy bodies dropped through the air in a perpendicular direction would descend to the earth with a real and remarkable increase of velocity, but with an apparent one only. Therefore the earth either does not move at all, or at least not with a diurnal motion.—2d. If the earth were moved by a diurnal motion, or even by an annual one, the force of a cannon ball would be much weaker, when discharged towards the north or south, than from the west towards the east. But the consequent is false; and therefore the antecedent also.—3d. If the earth were moved by a diurnal rotation, a ball of baked earth of eight ounces let drop through the still air from the height of 240 Roman feet, would fall obliquely towards the earth, without real or physical increase of velocity, or certainly not by so much as is the proportion of the percussion and sound occasioned by the fall from the said altitude. But the latter is absurd, and therefore the former.

In answer to the first of these arguments, Angeli denies the minor, which Riccioli pretends to prove thus:—If the earth is moved by a diurnal motion, any heavy body dropped from the top of a tower C, in the plane of the equator, should describe by its own natural motion a portion of the line C T I, which would be to all appearance circular. See fig. 6, pl. 7.

This Angeli denies, showing, by computation, that Riccioli's observation proves no such thing. For, (says Angeli) according to Riccioli, in one second of an hour the weight descends 15 feet; in two seconds, 60 feet; in three seconds, 135 feet; and so continually, the spaces from the beginning in the duplicate proportion of the time from the beginning; and, according to the same author, A B, the semidiameter of the earth, is 25870000 feet, and B C, the height of the tower of the Asinelli, in Bononia, 240 feet; therefore A C is 25870240, which has the same proportion to F S, 15 feet, to wit, the fall in one second, which A C, in parts 20000000000, has to F S 11596 $\frac{54356}{224189}$; but supposing, with Riccioli, C S I A a semicircle, F S is 53 parts, of which A C is 10000000000: hence Angeli concludes, that C S I A is no ways near to a

course concerning a New Planet; tending to prove that our earth is one of the planets. In 1641, Mercury, or the secret and swift messenger; showing how a man may, with privacy and speed, communicate his thoughts to a friend at any distance. In this work are descriptions of many ways of telegraphic communications, as practised by several people. In 1648, Mathematical Magic; being a relation of the wonders effected by engines and mechanical contrivances. And lastly, in 1668, The Essay towards a real Character and Philosophical Language; a very ingenious performance. Besides numerous theological writings. All the foregoing mathematical and philosophical works were collected, and published 1708, in one vol. 8vo. with an account of the life and writings of the author.

semicircle; which is most true, if the weight fall not to the centre of the earth precisely in 6 hours; for, in this case of Riccioli, the weight falls to the centre of the earth in 21 minutes and 53 seconds.

Manfredi, in his answer for Riccioli, affirms, that Angeli understands not the Rule-of-Three, in giving out F S for 11596 $\frac{54356}{224189}$, of which A C is 20000000000: and Angeli, in his reply, affirms his analogy to be so clear, that there can be nothing said more evident than itself to confirm it: referring in the mean time the further determination to geometricians.

Angeli might have answered Riccioli's argument, granting the weight to move equally in a semicircle, by distinguishing his minor thus: No heavy bodies descend to the earth with a real and notable increase of velocity, if the velocity be computed in the circumference of a semicircle; then the minor proposition is true. But the descending motion is not so to be computed: for here the equal motion in the circumference of the semicircle C I A is compounded of the equal motion in the quadrant C D, and of the accelerated motion in the moveable semidiameter C A; and this accelerated motion in the semidiameter is a true and simple descending motion; in which acceptation the minor proposition is most false, and likewise contrary to the experiments of Riccioli. But it seems that Angeli answers otherwise, to make Riccioli sensible that C I A is no semicircle; concerning the nature of which line they debate very much throughout the whole dispute.

The second argument is much insisted on by Angeli, to make his solution clear to vulgar capacities; but the substance of all is, that the cannon ball has not only that violent motion impressed by the fire, but also all those motions proper to the earth, which were communicated to it by the impulse received from the earth; for the ball, going from west to east, has indeed two impulses, one from the earth, and another from the fire; but this impulse from the earth is also common to the mark, and therefore the ball hits the mark only with that simple impulse received from the fire, as it does when shot towards the north or south; as Angeli excellently illustrates by familiar examples of motion.

To Riccioli's third argument Angeli answers, desiring him to prove the sequel of his major, which Riccioli does, supposing the curve in which the heavy body descends to be composed of many small right lines; and proving that the motion is almost always equal in these lines; and after some debate concerning the equality of motion in these right lines, Angeli answers, that the equality of motion is not sufficient to prove the equality of percussion and sound, but that there are necessary also equal angles of incidence; which in this case he shows to be very unequal. To illustrate this more, let us prove that, other things being alike, the proportion of two percussions is composed of the direct proportion of their velocities, and of the direct proportion of the sines of their angles of incidence.

Let us suppose also the following axiom, to wit, that percussions, *cæteris paribus*, are in the direct proportion of the velocities, by which the moving body approaches the resisting plane. Suppose C F the plane, (fig. 7, pl. 7); and let there be two moveable bodies, in every respect alike, which approach with an equal motion the plane C F from the point A, in the right lines A D, A F; I say, the percussion at the point D is to the percussion at the point F, in the ratio compounded of the ratio of the velocity in the right line A D, to the velocity in A F, and of the ratio of the sine of the angle A D E, to the sine of the angle A F E. From the point A to the plane C F draw the perpendicular; also make the right line A C equal to the right line A F, A B equal to the right line A D, and the plane B G H parallel to the plane C F. Let us suppose the moving body, as before mentioned, alike in all respects, to be moved equally in the right line A C, with the same velocity with which the body is moved in the right line A D; then because the planes B G H, C F are parallel, and the motion in the right line A C is equable, therefore the moving body approaches the plane B H with the same velocity with which it approaches the plane C F, and thence the percussions at the points B and C are equal; also the percussion at the point D, is to the percussion at the point B, as the right line A E to the right line A H, or (because of the equals A B, A D) as the sine of the angle A D E to the sine of the angle A B H, which I thus prove: the velocity of the body in the straight line A D, is equal to the velocity in the right line A B, which is equal to A D, and therefore both the right lines A D, A B, are passed over in the same time; and so in the same time the accessions to the resisting planes A E, A H, are performed; therefore the velocities of the accessions to the resisting planes are in the direct ratio of A E to A H, and likewise the percussion at the point D is to the percussion at the point C, in the same ratio of A E to A H, namely, as the sine of the angle of incidence A D E to the sine of the angle of incidence A C E, or A F E. But because the right lines A C, A F, are equally inclined to the plane C F, the moving bodies in the right lines A C, A F, approach to the plane C F, in the same ratio in which they are moved in the right lines A C, A F; and therefore the percussion at C is to the percussion at F, in the ratio of the velocity of the motion at A C, or in A D, to the velocity of the motion in A F. But since it is before demonstrated that the percussion at the point D, is to the percussion at the point C, in the ratio of the sine of the angle A D E to the sine of the angle A F E; and now it is demonstrated that the stroke at the point C, is to the percussion at the point F, as the velocity of the motion in A D to the velocity in A F: therefore (by

5 defin. 6 Eucl.) the percussion at D, is to the percussion at F, in the ratio compounded of the ratio of the sine of the angle of incidence A D E, to the sine of the angle of incidence A F E, and of the ratio of the velocity in A D to the velocity in A F; which was to be demonstrated.

It makes no difference that this demonstration is confined to equal motions in right lines and resisting planes; for it is true in every case, since the percussions are made in the point in which the right and curved lines coincide and agree: but if the percussions be not made in points, from these no geometrical considerations can be given, but the defect of the conclusion is to be judged of according to the defect of matter from the requisite conditions; as it ought always to be, when geometrical demonstrations are applied to a physical body.

In Angeli's reply to Manfredi, he mentions an experiment, which, as it was related to him by a Swedish gentleman, had been made with all due circumspection by Descartes, to prove the motion of the earth. The experiment was; he caused a cannon to be erected perpendicular to the horizon; which being 24 times discharged in that position, the ball fell 22 times towards the west, and only twice towards the east.*

An Enlargement of the Observations formerly published in Numb. 27. *By Dr.* Stubbes. *N°* 36, *p.* 699.

Of no consequence now to any one.

Extract of a printed Letter, addressed to the Editor, by Dr. Denis, *of Paris, touching the Differences that have arisen about the Transfusion of Blood. Dated Paris, May* 15, 1668. *N°* 36, *p.* 710.

You have sensibly obliged me in assuring me, by your letter of April 29, that the magistrates of London had not at all concerned themselves to prohibit the practice of the transfusion of blood, and that that operation had been hi-

* There seems to be a strange blunder made here, both by Angeli and the Swedish gentleman, with regard to Descartes's object in this experiment; since it rather proves the composition of forces, than the earth's motion. For as it is constantly found that, in all such experiments as this, the ball falls down again very nearly on the same spot from whence it was discharged, the inference would rather be (independent of the composition of forces) that the earth did not move. For, granting the earth's motion, either diurnal or annual, or both, then the ball must fall very far indeed from the place of the gun, viz. by more than three miles in the middle latitudes, in consequence of the diurnal motion, and not less then 300 miles by the annual motion, were it not for the composition of motion, or of forces, supposing the ball to ascend only to the moderate height of 1000 feet. So that, such composition being a fact very well established, the experiment proves nothing as to the earth's motion, neither for nor against it. But, granting such motion, then the circumstance of the ball falling down again near the place of the cannon, is a splendid proof of the composition of forces or motion.

therto practised with good success on brutes, and without any ill consequence upon a man (Arthur Cogan). The enemies of new discoveries had taken such great pains to publish every where this false report to decry this experiment, that there needed an authentic testimony to undeceive the multitude. If one should undertake to dissipate all the false rumours on this subject, one should never lay aside the pen; but the best of it is, that men of discretion so much disdain these wild reports, that they listen to them with disgust. And as to me, I was resolved to write no more upon it, until some new experiments should countenance my first conjectures. But your last letters do so civilly engage me to impart to you the secret cabal, practised by some persons to embarrass the history of that madman, that was cured* six months ago by means of transfusion, that I could not omit sending you the sum of what hath hitherto passed upon that subject, expecting mean time what the parliament of Paris, who I believe will be the judges and arbitrators thereof, shall determine.

You already know, that the transfusion of calf's blood so tempered the excessive heat of the blood of the madman,† who for four months had run naked up and down the streets night and day, that he fell asleep two hours after the operation, and that after ten hours sleep he awakened in his senses, and that he remained in that condition about two months, until the too frequent company of his wife, and his debauches in wine, tobacco, and spirituous liquors, had cast him into a very violent and dangerous fever.

You may also have heard, that this operation had effects quite contrary at the same time, and that for one brain cooled thereby, it fired many, forasmuch as by curing the madness of one poor wretch, it disturbed the wits of many such as aim at nothing, but to signalize themselves by opposing all new discoveries, which themselves are not capable of making. It was indeed but three or four days after this man was recovered, that some malicious spirits began to publish that he died under our hands, and that we had put an end to his extravagancies by putting an end to his life. This first story having been proved to be false, they mended the tale, and were resolved to make people believe he was relapsed into his former madness, and even was grown worse than ever. This obliged the first president and many other persons of quality to send for him to their houses, to examine the truth themselves; who, after they had entertained him awhile, were all satisfied of the good effect of the transfusion, and that those wanted no malice, who reported things so contrary to what they saw with their own eyes.

These things you may have learned from our formerly printed letters; but

* He was not cured. His madness was periodical, and he relapsed.

† This tempering of the heat of the patient's blood by the blood of the calf, is a strange idea.

what perhaps you know not yet, is, that these envious spirits were not the only ones that were troubled at this cure. The wife of the patient was most alarmed at it, though she used artifice enough to show us the contrary, and to persuade us, that she thought of nothing else, but to relieve him in his distempers. The truth is, that this man having been a lackey, and since a valet de chambre, had no profession that could bring in a subsistence for his family. And indeed the time of his madness was not so troublesome to his wife as the time when he was in his wits; for whereas she had her freedom to make certain visits, and to live as she listed, when he was not at home, but ran up and down, and even lay at night in the streets; she was on the contrary in great pain when he came to stay at home, because he observed her narrowly, and could not forbear reproaching her, for having often attempted to poison him; now and then expressing also some jealousy he had conceived against her. These are the complaints she herself hath often made to credible persons, who thought themselves obliged to depose it judicially, thereby to discover the misunderstanding, which doubtless hath been the cause of the unfortunate sequel of this affair.

And indeed this poor man falling ill again, his wife urged us beyond measure to try a third transfusion upon him, insomuch that she threatened she would present a petition to the solicitor general to enjoin us to do what we absolutely refused. At last she came one morning to my house, and not finding me, she left word, that she entreated me in charity to come after dinner to her house, where would be a certain meeting. I went, and there met M. Emmerez, and finding a calf and every thing ready for a transfusion, we were about to go away, telling her that her husband was not in a condition for this operation. Then she fell down with tears in her eyes, and by unwearied clamour she engaged us not to go away without giving her the satisfaction of having tried all possible means to recover her husband. Her art was great enough to make us condescend to another trial, to see whether we could give him any relief. M. Emmerez, to content her, passed a pipe into the vein of the patient's arm; and since it is necessary to draw away some of the old blood when new is to be infused, he opened a vein in his foot for that end. But a violent fit having seized on him in that instant, together with a trembling of all his limbs, there issued no blood out of the foot, nor the arm; which obliged M. Emmerez to take out the pipe put into the arm, without opening the artery of the calf, and so without any transfusion.

This poor man dying the night after, and news thereof being brought us, we went thither next morning, together with M. Emmerez and another surgeon, and remembering the complaints the dead man had often made of his wife's attempt to poison him, we would gladly have opened his body in the presence of seven or eight witnesses. But she so violently opposed it, that it was not possible for us to execute our design. We were no sooner gone, but she bestirred herself exceedingly, as we were informed, to bury her husband with all speed. But being in an indigent condition, she could not compass it that day. Meantime a famous physician of the faculty of Paris, happening to be that night at the house of a lady who was solicited for a charity towards this burial, was of the same mind with us, that his body should be opened, and therefore sent instantly for surgeons to execute it. But she being resolved against it, used lies and other arts to elude this design. And when we threatened her, that we would return next morning and do the thing by force, she caused her husband to be buried an hour before day, to prevent our opening of him.

As soon as his death was buzzed abroad, the enemies of the experiment began to triumph, and soon after they published defaming books against us. I then resolved to be silent, but that silence made our adversaries keener. And I was surprised when two months after I was informed, that there were three physicians that did not budge from the widow, importuning her by promises of a great recompense, only to let them use her name to accuse us before a court of justice for having contributed to the death of her husband by the transfusion; and that even they addressed themselves to the neighbours of this woman to engage them to bear false witness against us. And some time after, this woman, raised by the hopes given her by those men, came and told us, that some physicians did extremely solicit her against us, and that she had always refused them, knowing her obligations to us for having relieved her husband freely. But she drawing from hence no profit, as she expected she should, she turned her notices into menaces, and sent us word, that in the present necessity to which she was reduced, she was obliged to accept of the offer made her by certain physicians, if we would not assist her. I sent her this answer, that those physicians and herself stood more in need of the transfusion than ever her husband had done, and that, for my part, I cared not for her threats. But yet I then thought it time to break silence, not only my interest being concerned, but the public, to discover to the world those persons that would be engaged in intrigues so unworthy of learned men. I complained of it to the lieutenant in criminal causes, who presently allowed me to inform both against the widow and those that solicited her. Some witnesses having been called before justice, they deposed against the three physicians and this woman, accusing them of having secretly given to her husband certain powders, which might have contributed to his death.

This information, brought in by five witnesses, having been presented in a full court to the said lieutenant by Mr. Dormesson, the king's advocate, he gave sentence, that the woman should have a day set her, to appear in person to be examined upon my informations, and that in the mean time new informations should be taken against her at the desire of his majesty's attorney. And because he thought that there might be danger in permitting indifferently the practice of transfusion to all sorts of persons, he ordered, that for the future it should be used but under the inspection of physicians; this is what you will see more fully in the sentence itself.

Extract of the Sentence, given at the Chastelet, by the Lieutenant in Criminal Causes, April 17, 1668, *in Paris.*

In this cause there are proofs and evidences of these particulars;

1. That the operation of transfusion was twice performed upon Antony Mauroy, a madman, and that it was attempted the third time: that it succeeded so well those two times, that the patient was seen for two months after it in his good senses and in perfect health.

2. That from the time of the two first operations his wife gave him eggs and broths, and bedded with him four times, notwithstanding the prohibition of those that treated him, and that she carried him to her house without speaking to them of it, and with great reluctancy of her husband.

3. That since that time, he went from one public house to another, and took tobacco, and falling ill again, his wife gave him spirituous liquors to drink, and broths, wherein she mixed certain powders; and that Mauroy having complained that she would poison him, and gave him arsenic in his broths; she hindered the assistants from tasting thereof, and making a show of tasting it herself, cast upon the ground what she had in a spoon.

4. That du Mauroy had frequent quarrels with his wife since, and that she gave him many strokes, as sick as he was, but having once received a box on the ear from him, she said he should repent it though she should die for it.

5. That when transfusion was attempted the third time, it was at the instant request of his wife; those that were to perform the operation refusing to do it without permission of the Solicitor General; that some days after that the operation was begun, but that as scarce any blood issued either out of the foot or of the arm of the patient, a pipe was inserted, which made him cry out, though it appeared not that any blood of the calf had passed into his veins: that the operation was given over, and that the patient died the next night.

6. That this woman would no ways suffer any person to open the body of her husband, saying in excuse, he was already in the coffin, when he was not.

7. That a good while after the decease of the said du Mauroy, three physicians did solicit the said woman to take money, and to make complaints that the transfusion had killed her husband: that she said when those persons were gone away from her, that they had been with her upon that account; and that unless those that had made the operation would give her wherewith to return into her country, she should do what those others pressed her to: that a witness deposeth, that she came to pray him, that he would inform those who had made the operation, that unless they would maintain her during her life, she would accept of the offer made her by the said physicians: that another witness deposeth, that one was come to him from a physician, and had offered him 12 *louis d'or*, if he would depose that du Mauroy died in the very act of the transfusion.

That the matter was important enough to inquire into the bottom of it; that there was cause enough to examine this woman, Where she had those powders? Why she had given them to her husband? And by whose order? Why she had hindered the opening of the body by a lie? That he required further information might be taken about it, and she in the mean time put in safe custody.

That as to the three physicians who had solicited her with money to prosecute those that had made the operation, and who had been seen with her, he demanded that a day might be set them to appear in person.

Lastly, that since the transfusion had succeeded well the first two times, and had not been undertaken the third but at the earnest request of the woman, who otherwise had so ill observed the orders of those that had made the operation, and who was suspected to have caused the death of her husband, he demanded that the execution of the decree of prefixing them a day for personal appearance might surcease.

Whereupon it was decreed, that the widow of du Mauroy should on a set day appear personally, and undergo the examination upon the alleged informations; and that more ample informations should be made of the contents in the complaint of Mr. Denis: And then, that for the future no transfusion should be made upon any human body but by the approbation of the physicians of the Parisian faculty.

Since this sentence new informations have been given in, considerably stronger than the former; and witnesses have been discovered, to whom the woman had committed it as a secret, that it was arsenic she mingled in her husband's broth, and even that the patient before his death having given the remainder of one of the messes of broth to a cat, the animal died of it a few days after.

As to the experiment of transfusion, you see it is not absolutely prohibited by this sentence; there needs no more to practise it but to have the approbation of some physicians of Paris; of whom seven or eight have already signed the proposal made for one. And I have now before me a paralytic woman (a neighbour and friend of her who was cured of the palsy this way) who is resolved to present a petition to the magistrate, and therein to desire the transfusion may be allowed her.

Meantime, if ever the faculty of the Parisian physicians meet upon this business, I do not believe that they will act with that precipitancy as is supposed. And as to the parliament, I do not see that those who compose it are of a resolution to strike at this operation, unless it should happen that the experiments that may be made before them should not succeed as those have done that have been made hitherto. It is well known to that court, that the faculty made a decree a hundred years since against antimony, which was then used by the physicians of Montpelier, and that after they had given it a place among poisons, they obtained a sentence prohibiting the use thereof: yet nevertheless these physicians not having forborn to use it under another name, the effects thereof proved so advantageous, and the recovery of our great monarch thereby so famous, that the same faculty of Paris was constrained two years ago, by a decree, to approve what before they had forbidden, and even demanded another sentence for permitting the use of the same.*

* If we put together all the circumstances stated in the preceding narrative, we shall easily be convinced that the operation of transfusion did not occasion this man's death, and that there were strong grounds for suspecting that he was poisoned. The selecting, however, of such a subject for the ex-

A Sand-flood at Downham, in Suffolk. By THOMAS WRIGHT, *Esq.* *N° 37, p. 722.*

It is but about 100 years since the sands first broke loose. I could not without some difficulty trace out their original. But I now find it to be in a warren in Lakenheath, distant about five miles south-west and by west of Downham. There some great sand-hills, having the sward or superficies of the ground broken by the impetuous south-west winds, blew upon some of the adjacent grounds; which being much of the same nature, and having nothing but a thin crust of barren earth to secure it, was soon broken up, and thus contributed to increase the mass. At the first eruption the whole magazine of sand could not cover above eight or ten acres of ground; which increased into 1000 acres before the sand had travelled four miles from its first situation. Indeed it met with this advantage, that till it came into this town, all the ground it passed over was almost of the same nature as itself. All the opposition it met with in its progress hither, was from one farm house, which stood within a mile and a half of its first source. It is between 30 and 40 years since it first reached this town; where it continued for 10 or 12 years in the outskirts without doing any considerable mischief. The reason of which seems to be, that its current was then down hill, which sheltered it from those winds that gave it motion. But that valley being once passed, it went above a mile up hill in two months time, and overspread 200 acres of very good corn the same year. It is now got into the body of this little town, where it has buried and destroyed several tenements and other houses, and has forced us to preserve the remainder at a greater charge than they are worth. Which doubtless had also perished, had not my affection for this poor dwelling obliged me to preserve it at a greater expense than it was built. Where at last I have given it some check, though for four or five years our attacks on both sides were with so various success that the victory remained very doubtful. For it had so possessed all our avenues, that there was no passage to us but over two walls, of eight or nine feet high, which encompassed a small grove before my house, now almost buried in the sand; nay, it was once so near a conquest, that at one end of my house it was possessed of my yard, and had blown up to the eves of most of my out-houses. At the other end it had broken down my garden wall, and stopped all passage that way.

periment of transfusion does not reflect much credit on the judgment of Dr. Denis and his coadjutor; for unless more blood was drawn from the man than was infused into him (which does not appear to have been the case in the two first trials) a sudden and considerable plethora would be produced; a change little suited to relieve a maniacal affection.

But by stopping of it four or five years with furze hedges, set upon one another, as fast as the sand levelled them, by which I have raised sand-banks near 20 yards high, I brought it into the circuit of about eight or 10 acres: And then in one year, by laying some hundreds of loads of muck and good earth upon it, I have again reduced it to *terra firma*, have cleared all my walls, and by the assistance and kindness of my neighbours, who helped me away with above 1500 loads in one month, cut a passage to my house through the main body of it.

But the other end of the town met with a much worse fate, where divers dwellings are buried or overthrown, and our pastures and meadows over-run and destroyed: and the branch of the river Ouse, on which we border, for three miles together so filled with sand, that now a vessel with two loads weight passes with as much difficulty as before with 10. And had not the stream interposed to stop its passage into Norfolk, doubtless a great part of that country had ere now been left a desolate trophy of this conquering enemy.

The situation of the country in which these sands took their rise, lies east-north-east of a part of the great level of the fens, and is thereby fully exposed to the rage of those impetuous blasts, which yearly blow from the opposite quarter, and which I suppose acquire more than an ordinary vigour by passing through so long a tract without any check. Another thing that contributes to it, is the extreme sandiness of the soil, the lightness of which I believe gave occasion to that story of the actions that used to be brought in Norfolk for grounds blown out of the owner's possession.

Of the Magnetical Variation, and the Tides, near Bristol. By Capt. Sam. Sturmy. N° 37, p. 726.

June 13, 1666, Capt. Sturmy made the following magnetical observations in Rownham meadows, near Bristol, by the water-side.

Sun's observed Altitude.		Magnetical Azimuth.		Sun's true Azimuth.		Variation Westerly.	
44°	20′	72°	00′	70°	38′	1°	22′
39	30	80	00	78	24	1	36
31	50	90	00	88	26	1	34
27	42	95	00	93	36	1	24
23	20	103	00	101	23	1	23

In this table he notes the greatest distance or difference to be 14 minutes; and so taking the mean for the true variation, he concludes it then and there to be just 1 deg. 27 min. viz. June 13, 1666.

He observed again in the same day of the next year, viz. June 13, 1667, and then found the variation increased about six minutes westerly.

From many former observations Capt. Sturmy assures, that the highest spring and annual tides there are about the equinoxes, according as the moon is near the full or change, before or after that time.

An easy Help for decayed Sight. N° 37, pp. 727 and 729.

The inventor of this method was about 60 years of age, but his sight much decayed; and I seemed, says he, always to have a kind of thick smoke or mist about me, and some little black balls dancing in the air about my eyes, and to be in such case as if I came into a room suddenly from a long walk in a great snow. I could not distinguish the faces of my acquaintance, nor men from women, in rooms that wanted no light. I could not read the great and black English print in the church bibles, nor keep the plain and trodden paths in fields or pastures, except I was led or guided. I received no benefit by any glasses, but was in the case of those whose decay by age is greater than can be helped by spectacles. The fairest prints seemed through spectacles like blind prints, little black remaining.

Being in this sad plight, what trifle can you think has brought me help more valuable than a great sum of gold? Truly, no other than this: I took spectacles that had the largest circles; taking out the glasses, I put black Spanish leather taper-wise into the emptied circles, which widened enough, took in my whole eye at the wider end; and presently I saw the benefit through the lesser taper-end, by reading the smallest prints, which thus seem as if they had been a large and fair character. I coloured the leather on the inside with ink, to take off the glittering. Finding that the smaller the remote orifice was, the fairer and clearer the smallest prints appeared; and the wider that orifice was, the larger object it took in, and so required the less motion of my hand and head in reading; I therefore cut one of these tapers a little wider and shorter than the other, and the wider I use for ordinary prints, and the longer and smaller for smallest prints: these without any trouble I alter as is necessary. I can only put the very end of my little finger into the orifice of the lesser, but the same finger somewhat deeper, yet not quite up to the first joint, I can insert into the orifice of the wider. Sometimes I use one eye, sometimes another, for ease by the change; for you must expect that the visual rays of both eyes will not meet for mutual assistance in reading, when they are thus far divided by tubes of that length. The lighter the stuff is, the less it will encumber. Remember always to black the inside with some black that has no lustre or glittering. And you should have the tubes so moveable, that you may draw them longer or shorter, allowing also the orifice wider or narrower, according to circumstances.

I have not tried what glasses will do if settled in these tubes, having no need of them. Probably they may be more proper for some that are squint-eyed, whose eyes interfere. Certainly it will ease those that cannot well bear the light; and perchance it will preserve the sight for a longer time.

In another letter the same person adds:

I see now, by these taper tubes, as well as the youngest in my family, and can read the smallest and most confused prints through them as well as ever I could from my childhood, though my sight be almost lost. And having used these empty holes for spectacles little more than a week, I can now use them without trouble all the day long; and I verily believe, that by this little use of them, my sight already is much amended. For I now see the greenness of the garden, and pastures in a florid verdure, whereas very lately dark colours, blue and green, had the same hue to my eye.

If you ask me, how this device came in my head, I shall tell you all I know. Some years ago I was framing one of Hevelius's polyscopes; as I was trying the tube, without the dioptric glasses, I perceived that though the tube took in very little, and seemed scarce serviceable for any considerable purpose; yet the object appeared to me more distinct and clear through the tube, than through the open air. This I recollected, and thereupon made the trial, and found the effect fully answer to my case.

As for your trial of the tubulous spectacles, the tubes may be of paper, only coloured black and pasted on, and with the inner folds drawn out from one inch to three; some of the folds to be taken out, that the orifice may be wider or narrower, as best fits to every degree of defect.

Of the Antiquity of the Transfusion of Blood from one Animal to another. N° 37, p. 731.

There has been of late some contest about the origin of transfusion, the English first claiming it as a late invention of theirs, the French pretending thereupon, that it had been proposed among them ten years ago: after which, it was affirmed upon further investigation, by some ingenious persons in England, that it had been known there 30 years ago; (whereof the publisher of these Tracts has good proof in his hands). But it seems, that an Italian philosopher, in a tract, entitled, Relatione dell' Esperienze fatte in Inghilterra, Francia, & Italia, intorno la Transfusione del Sangue, lately printed in Rome, undertakes to prove that the transfusion is yet of greater antiquity, as having been known to Libavius above fifty years since. For which that Roman author quotes a place out of the said Libavius (in Defensione Syntagmatis Arcanorum Chymicorum contra Heningum Schneumannum, Actione 2, p. 8. edit. Francof. A. 1615), where the transfusion is so plainly described, that one can hardly discourse of it with more clearness than is there done, in these words: Adsit (says Libavius, l. c.) Juvenis robustus, sanus, sanguine spirituoso plenus: Adstet exhaustus viribus, tenuis, macilentus, vix animam trahens. Magister artis habeat tubulos argenteos inter se congruentes, aperiat arteriam robusti, & tubulum inserat muniatque; mox et ægroti arteriam findat, et tubulum fœmineum infigat. Jam duos tubulos sibi mutuo applicet, et ex sano sanguis arterialis, calens et spirituosus saliet in ægrotum, unaque vitæ fontem afferet omnemque languorem pellet. This indeed is clear enough, and obliges us to allow a greater antiquity to this operation than we were before aware of; though it is true, Libavius did not propose it but only to ridicule it; besides, he contrives it with great danger both to the recipient and emittent, by proposing to open arteries in both; which indeed may be practised upon brutes, but ought by no means upon man.

Mr. GREGORY'S Answer to the Animadversions of Mr. HUYGENS, upon his Book, De vera Circuli et Hyperbolæ Quadraturâ; as they were published in the Journal des Scavans of July 2, 1668. N° 37, p. 732.

This letter is omitted, as of no satisfactory use to any person, without the animadversions which occasioned it, and which were printed in another country. The whole controversy, both animadversions and answers, was collected and printed in Huygens's Opera Varia, vol. 2, pp. 463, &c.

An Account of some Books. N° 37, p. 736.

I. Discours Physique de la Parole, par M. De Cordemoy, à Paris, in 12mo.

II. De Infinitis Spiralibus Inversis, Infinitisque Hyperbolis, aliisque Geometricis, Auth. F. Stephano de Angelis, Veneto. Patavii, in 4to.

This author treats here concerning the figures mentioned in the title, measuring their areas very accurately and geometrically; as also concerning several other things conducing to the perfection of geometry. He mentions one of these spirals to be the line described by a heavy body falling towards the centre of the earth, supposing the earth's motion. He also touches on the controversy betwixt himself and Riccioli, given more at large in the foregoing number.

III. Michaelis Angeli Ricci* Exercitatio Geometrica; in 4to. printed at Rome.

This book is reprinted in London, and annexed to Logarithmotechnia N. Mercatoris. It was thought fit to be so reprinted, partly by reason of its scarceness, but chiefly by reason of the excellency of the argument, which is, de maximis et minimis, or the doctrine of limits; wherein, according to the account of the intelligent Mr. John Collins, the author shows a deep judgment in discovering a medium to reduce the lately found out analytical doctrine de maximis et minimis to pure geometry. The tract itself is very small, being little more than two sheets of paper; wherein is demonstrated the doctrine of Caravagio de applicationibus, who affirms, that he who is ignorant therein may mispend his time about equations, in searching for that which cannot be found. He delivers also a method of drawing tangents to all the conic sections, and divers other curves.

IV. La Venerie Royale du Sig. de Salnove, in 4to. à Paris.

Treating of the different kinds of hunting in France.

A Contrivance to make the Picture of any Thing appear on a Wall, &c. in a Light Room. By Mr. Hook. N° 38, p. 741.

This optical experiment is new, though easy and obvious; and has not that I know been ever made by any other person in this way. It produces effects not only very delightful, but to such as know not the contrivance very wonderful; so that spectators not well versed in optics, that should see the various apparitions and disappearances, the motions, changes and actions, that may this way be represented, would readily believe them to be supernatural and miraculous.

Opposite to the place or wall where the apparition is to be, let a hole be made of about a foot in diameter or larger; if there be a high window that has a casement in it, it will be so much the better. Without this hole place the picture or object, which you will represent, inverted, and by means of looking-glasses set behind, if the picture be transparent, reflect the rays of the sun so as that they may pass through it towards the place where it is to be represented; and let the picture be encompassed on every side with a board or cloth, that no rays may pass beside it. If the object be a statue or some living creature, then it must be very much enlightened, by casting the sun beams on it by refraction, reflexion, or both. Between this object and the place where it is to be repre-

* Michael Angelo Ricci was a learned Italian divine, born at Rome, 1619. He was well skilled in the pure mathematical sciences, of which the above article is a good specimen. He was created cardinal in 1681; but did not long enjoy that dignity, dying in 1683, at 64 years of age.

sented, there must be placed a broad convex-glass, so that it may represent the object distinct. The nearer it is placed to the object, the more is the object magnified on the wall; and the farther off the less; which diversity is effected by glasses of several spheres. If the object cannot be inverted, as it is pretty difficult to do with living animals, candles, &c., then let two large glasses of convenient spheres be placed at proper distances, to be found by trials, to make the representations erect, as well as the object.

These objects, reflecting and refracting glasses, and the whole apparatus, as also the persons employed to manage them, must be placed without the window or hole, so that they may not be perceived by the spectators in the room.

Whatsoever may be done by means of the sun-beams in the day time, the same may be done with much more ease in the night, by the help of torches, lamps, or other strong lights placed about the objects, according to the several sorts of them.

So far our inventor; who has not contented himself with the bare speculation, but put the same in practice some years since, in the presence of several members of the Royal Society.

Of Counterfeiting Opal, and making Red Glass. By Mr. S. Colepresse. N° 38, p. 743.

I was two days at Haarlem, on purpose to see the experiment of the making of counterfeited opal glass, which is there done by rule. It is very lively, and, as I guess, performed only by the degrees of heat producing the colours. When the composition is thoroughly melted, they take out some on the point of an iron-rod, which being cooled, either in the air or water, is colourless and pellucid; but being put into the mouth of the furnace on the same rod, and there turned by the hand for a little time, its little particles take such various positions, that the light falling on them being variously modified, represents those several colours that are seen in the true opal. It is remarkable that the colours of it may be destroyed and restored again by different degrees of heat.

They also make there the amethyst and sapphire; and have recovered the hundred years loss of incorporating red glass; and have some metal that is thought to equal crystal in hardness as well as colour.

Some Animadversions, written in a Letter by Dr. John Wallis, on a printed Paper, entitled Responsio Francisci du Laurens ad Epistolam D. Wallisii, ad Cl. V. Oldenburgium scriptam. N° 38, p. 744.

A continuation of the unprofitable controversy, noticed before in N° 34, p. 239, between Dr. Wallis and M. du Laurens.

An Account of two Books. N° 38, p. 750.

I. R. de Graaf, M. D. de Virorum Organis Generationi Inservientibus, Ludg. Bat. 1668, 12mo.

This treatise was promised by the author in a printed epistle of his, of which we gave an account in N° 34, p. 241. There being at that time published a Prodromus of J. Van Horne, wherein it was suspected that the observations of de Graaf were much the same with his upon this subject; we do now, upon the perusal of this book, find chiefly these considerable differences between them:

Van Horne makes the spermatic artery in man to go to the testicles in a winding, but De Graaf in a straight direction. The former affirms, that the vasa deferentia have no communication with the vesiculæ seminales; but the latter maintains and demonstrates that there is so great a communication between them, " ut semen dum a testibus per vasa deferentia affluens in urethram " effluere nequit, propter carunculam clausam; necessario influat in vesiculas, " in iisque pro futuro coitu reservetur." The former is of opinion, " triplicem " esse materiam seminis;" but De Graaf will have only one, answering the arguments of both Van Horne and Dr. Wharton to prove that triplicity.

But what De Graaf much insists on in this book is, to show what is the true substance of the testicles, and to vindicate the discovery thereof to himself, affirming positively that no man before him ever knew the truth of it.* For the making out of which, he first denies that the testes are glandulous or pultaceous; and then affirms that their substance is nothing else but a " Congeries " minutissimorum vasculorum semen conficientium, quæ si absque ruptione " dissoluta sibi invicem adnecterentur, facile viginti ulnarum longitudinem ex" cederent." Which he affirms he can prove by ocular demonstration.

He then shows how the seminal vessels pass " è testibus ad epididymides," viz. not by one trunk (as Dr. Highmore† thinks) but by 6 or 7 small ducts; assigning the cause why Dr. Highmore did not see them.

Farther he examines, " An semen in testibus conficiatur; utrum ex sanguine " vel ex lympha? quomodo elaboretur, crassescat, lactescat: qua via à testibus " ad urethram excurrat."

He also endeavours to prove, " Vesiculas seminales ordinatas esse non seminis " generationi, sed ejus receptioni et asservationi."

* See Dr. Clarck's letter, No. 35.

† Nathaniel Highmore practised at Oxford in the middle of the 17th century, and acquired a considerable reputation by his medical and anatomical writings, viz. by his Corp. Hum. Disquisitio Anatomica, by his History of Generation, and by his Exercitationes de Passione Hyst. deque Hypochondriaca Affectione. But Haller finds fault with the descriptions given in the first of these anatomical works, and says that most of the plates are copied from Vesalius.

He also observes concerning the seminal matter, that it is composed ex duplici materia, which, after Aristotle, he calls λόγον σπερματικὸν καὶ ὄγκον σπερματικόν, considering this twofold matter like dough and ferment, this infecting and quickening that, and the grosser part being a conservatory and vehicle to that which is most elaborate.

When he examines the penis, he takes notice, "Omnes hactenus anatomicos "perperam assignasse usum musculorum penis, quos erectores appellant; eorum "quippe provinciam non esse, penem erigere, et dilatare urethram, cum omnis "musculi actio sit contractio, quæ extensioni contraria est; eos potius penem "versus interiora retrahere quam erigere: Interim, hosce penis musculos, coarc-"tando corpora nervosa circa eorum exortum, materiam seminalem versus penis "partem anteriorem propellere, atque hac ratione corporum nervosorum disten-"sione erectionem augere."*

Before we conclude this account, we cannot but take notice, that the author occasionally inserts in this book several curious and remarkable examples and observations; some of which are the following:

1. Concerning those that are born, either "absque testibus; or, cum testiculo uno; or, cum tribus, idque hæreditario per aliquot familias, admodum fæcundas." 2. About the "situs præternaturalis testiculorum generationis tamen virtutem non impedientis." 3. Concerning lactescent blood in a man living at Delft in Holland, whose blood always turned into milk, when let out either by venesections or by bleeding at the nose, or by a wound. Compare Number 6, p. 37, and Number 8, p. 41 of this Abridgement of these Transactions. 4. Concerning the strange alteration made in females, "à "liquore seminali: quod confirmat exemplo felis, diu sugentis (idque ad in-"tegram fere sui nutritionem) lac mammarum caniculæ, per aliquot annos "à coitu prohibitæ; deinceps vero, postquam catella admiserat canem, nun-"quam ab eo tempore lac ex mammis ejus exsugere volentis." 5. About a strange hæmorrhagy per penem, which amounted to 14 pounds, in a porter of 52 years old, falling down with a heavy load upon a board laid over a ditch, which so turned about on his stepping upon it as to throw him down upon its edge, turned between his legs; yet the patient by the skill and care of our author recovered. 6. Various observations on clysters and suppositories, cast up by vomits. 7. Several ways of performing dissections of animals without effusion of blood.

II. Logarithmotechnia of Nicholas Mercator, by Dr. John Wallis, in a letter to Lord Viscount Brouncker, president of the Royal Society. Translated from the Latin.

* Inter causas quibus penis erectio perficitur, imprimis annumerandus est sanguinis in ejus corpora cavernosa subitus atque impetuosus influxus.

With this book, my Lord, which is just come out, I was so much pleased, that I could not quit it before I read it quite through. The doctrine on which it is founded, and by which the logarithms may be expeditiously and ingeniously constructed, is perspicuous, and ingeniously treated. The quadrature of the hyperbola, subjoined to it, is very elegant and ingenious; and is to this effect.

After the author had demonstrated, prop. 14, that in the hyperbola M B F, (fig. 8, pl. 7.) having its asymptotes, A H, A N, meeting at right-angles, and drawing B I, F H, s p, &c. parallel to the asymptote A N, then the rectangles A I B, A H F, A p s, &c. are all equal among themselves, and that therefore their sides are reciprocally proportional, being the known property of the hyperbola: putting then A I = B I = 1, and H I $= a$; he shows, prop. 15, that F H $= \frac{1}{1+a}$, namely from this analogy H A : A I :: B I : F H, that is $1 + a$: 1 :: 1 : $\frac{1}{1+a}$ = F H $= 1 - a + a^2 - a^3 + a^4$ &c. by dividing the numerator 1 by the denominator $1 + a$, continued by the powers of a, alternately negative and affirmative. And since this holds equally true for every point H beyond I, putting A I, as before, = 1, and making any continuation of it, as I r, $= A$, which is conceived as divided into innumerable equal parts, each of which, as I p, p q, &c. is called a; therefore I p, I q, &c. will be a, $2a$, $3a$, &c. till the last term be A. Then the right lines p s, q t, &c. corresponding to these, comprehending the space B I r u, are,

	Since then it is,
$1 - a + a^2 - a^3 + a^4$ &c.	$1 + 1 + 1$ &c. (to the last) $= A$
$1 - 2a + 4a^2 - 8a^3 + 16a^4$ &c.	$a + 2a + 3a$ &c. (till A) $= \frac{1}{2}A^2$
$1 - 3a + 9a^2 - 27a^3 + 81a^4$ &c.	$a^2 + 4a^2 + 9a^2$ &c. (till A^2) $= \frac{1}{3}A^3$
and so on to	$a^3 + 8a^3 + 27a^3$ &c. (till A^3) $= \frac{1}{4}A^4$
$1 - A + A^2 - A^3 + A^4$ &c.	

and so on, as he shows in prop. 16, and which I have elsewhere demonstrated. Hence he properly infers, that the hyperbolic space B I r u is $= A - \frac{1}{2}A^2 + \frac{1}{3}A^3 - \frac{1}{4}A^4 + \frac{1}{5}A^5$ &c. So that, assigning to $A =$ I r, any value in numbers, and distributing the series into two classes, viz. the affirmative powers A, $\frac{1}{3}A^3$, $\frac{1}{5}A^5$, &c. and the negative powers $\frac{1}{2}A^2$, $\frac{1}{4}A^4$, &c. the aggregate of the latter being deducted from that of the former, the remainder will be the value of the hyperbolic space B I r u.

Then putting $A = 0.1$, or $= 0.21$, or any other decimal fraction, and consequently less than 1, that is, making I r less than A or 1, the last powers of A become so small, that they may be neglected. For example, putting A I = 1, and I r = 0.21, then the terms will be as follow:

$A = 0.21$	
$\frac{1}{3}A^3 = 0.003087$	$\frac{1}{2}A^2 = 0.02205$
$\frac{1}{5}A^5 = 0.000081682$	$\frac{1}{4}A^4 = 0.000486202$
$\frac{1}{7}A^7 = 0.000002572$	$\frac{1}{6}A^6 = 0.000014294$
$\frac{1}{9}A^9 = 0.000000088$	$\frac{1}{8}A^8 = 0.000000472$
$\frac{1}{11}A^{11} = 0.000000003$	$\frac{1}{10}A^{10} = 0.000000016$
$+ 0.213171345$	$- 0.022550984$
$- 0.022550984$	

Gives $0.190620361 =$ B I r u the hyperbolic space.

But if the quadrature of the whole space B I H F be required, when the side I H is longer than A I, this method would not succeed so well; for in that case A being greater than 1, it is manifest that the higher powers of it would be too considerable to be neglected. To remedy this inconvenience, proceed thus: Suppose H F u r the space to be squared, A H being of any length whatever, either greater or less than A I, or equal to it: taking the point r anywhere between A and H, let A H = 1, and H r = A, which is to be conceived as divided into innumerable equal parts, each = a; then, after A H = 1, the other parts continually decreasing will be, $1 - a$, $1 - 2a$, $1 - 3a$, &c. to A r = $1 - A$. Hence, because of the equal rectangles F H A, u r A, B I A, &c. each of which suppose = b^2, it will be H F = $\frac{b^2}{1}$, and the rest in order $\frac{b^2}{1-a}$, $\frac{b^2}{1-2a}$, $\frac{b^2}{1-3a}$, &c. till r u = $\frac{b^2}{1-A}$, completing the space H F u r, as is shown in the *Arithm. Infinit.* prop. 88, 94, 95. Then dividing b^2 by $1 - a$, the quotient will be $b^2 + b^2 a + b^2 a^2 + b^2 a^3$ &c, that is b^2 into $1 + a + a^2 + a^3$ &c, and all the right lines between H F and r u will be,

$$\left.\begin{array}{l} 1 + a + a^2 + a^3 \text{ \&c.} \\ 1 + 2a + 4a^2 + 8a^3 \text{ \&c.} \\ 1 + 3a + 9a^2 + 27a^3 \text{ \&c.} \\ \text{and so on till} \\ 1 + A + A^2 + A^3 \text{ \&c.} \end{array}\right\} \text{into } b^2;$$

Then the aggregate of all is $A + \frac{1}{2}A^2 + \frac{1}{3}A^3 + \frac{1}{4}A^4$ &c. $\times b^2 =$ F H r u, by Arith. Inf. pr. 64.

For example, let A H = 1, H r = $A = 0.21$, A I = b = 0.1, and therefore $b^2 = 0.01$; then,

$A = 0.21$
$\frac{1}{2}A^2 = 0.02205$
$\frac{1}{3}A^3 = 0.003087$
$\frac{1}{4}A^4 = 0.000486203 -$
$\frac{1}{5}A^5 = 0.000081682 +$
$\frac{1}{6}A^6 = 0.000014294 +$
$\frac{1}{7}A^7 = 0.000002573 -$
$\frac{1}{8}A^8 = 0.000000473 -$
$\frac{1}{9}A^9 = 0.000000088 +$
$\frac{1}{10}A^{10} = 0.000000017 +$
$\frac{1}{11}A^{11} = 0.000000003 +$

Their sum 0.235722333
Which drawn into b^2 or 0.01,
Gives 0.00235722333 = F H r u;

such that $1 = AHGN$ a square if A be a right angle, or a rhombus if A be an oblique angle.

The same may also be accommodated both to the constructing of logarithms, and to the finding the sum of the logarithms, which Mercator does in prop. 19. Thus, putting $AH = 1$, $AI = IB = b$, as before, and the plane $BIHF = pl$; it will be $pl - b^2 + b^3 = BIps + BIqt + BIru$, &c. till BIHF.

If we begin, not at BI, but on either side of it, as suppose at ps; then putting $pH = a$, and $psFH = pl$, it will be universally $pstq + psur$, &c. till $psFH = pl - ab^2$, where $1 = AH^3$. Which may be otherwise demonstrated, if necessary.

A Note relating to the former Narrative about Empty Tubes, serving for a decayed Sight; imparted by the same Author, in a Letter of August 10, 1668.* *N° 39, p.* 765.

I have now tried convex spectacles, which about three years ago (before my sight fell into this decay) agreed very well for my use; and putting these glasses into the tapers, I found the smallest prints somewhat larger, but not so clear, so distinct, nor so pleasing to the eye, as when I use the empty tapers. Also I am confirmed that these empty tapers preserve, strengthen, and in some small degree recover the sight. And I find myself best at ease with those leathern tubes I first used, and rather without any fastening to the bone of the spectacles: For as they hang in that slight manner, I can now with a touch of my finger raise them up or bow them down, divide them or unite to take in the same object. And I put them off and on as speedily and as easily as any other spectacles.

Extract of a Letter written from Dantzick to the Hon. Mr. Boyle, *containing the Success of some Experiments of Infusing Medicines into the Human Veins. N° 39, p.* 766.

Mons. Smith, physician in ordinary to this city, having liberty granted him to try an experiment upon some persons desperately infected with the venereal disease, then in the public hospital here, ventured the opening of a vein, and infusing some medicines into the blood. This was tried upon two persons, one of whom recovered, and the other died. Yet being since farther encouraged by corresponding with some of the Royal Society in England, about a month since the said physician, together with Mons. Scheffeler, another old practitioner in

* See N° 37, p. 266 of this Abridgement.

this city, repeated the experiment by infusing altering medicines into the vein of the right arms of three persons, the one lame of the gout, the other extremely apoplectic, and the third reduced to extremity by that singular distemper, the plica polonica. The success of this, as Mons. Hevelius informs me, was, that the gouty man found himself pretty well next day, and shortly after went to work, it being harvest time, and has continued well ever since, leaving the hospital yesterday, and professing himself cured. The apoplectic patient has not had one paroxysm since; and the several sores which the plica polonica had occasioned are healed; and both these persons have been able to work at any time these three weeks.*

A further Account of the Mendip Mines. By Mr. GLANVIL. *N° 39, p. 767.*†

This gentleman says he has been informed by experienced miners to the following effect, viz. that the veins sometimes run up into the roots of trees, yet they observed no difference at the top. The water is accounted healthy to drink, and to dress meat with it. The snow and frost near the grooves melt quickly; but continue longer at a greater distance. Sometimes when a mine has been very near the surface, the grass has been yellow and discoloured. Some have made use of the virgula divinatoria; but experienced workmen account it of no value; yet they say, when the mine is open they may guess by it how far the vein leads.

White, yellow, and mixed earth are leaders to the country, as they call it: changeable colours always encourage their hopes. The stones are sometimes 12 fathoms deep before they are met with. Other times, when a stony reak is at top, they meet ore just under the sward or superficies of the grass, which ore has sometimes gone down above 40 fathoms. A black stone is of bad import, as it leads to a jam, a black thick stone that hinders their work. A grey clear dry one they account best. They seldom meet with damps. If in sinking they come to wet moorish earth, they expect a jam, and to be closed up with rocks. The nearness they guess by short brittle clay; for the tough is not leading. The ore is sometimes shole, and sometimes 14 or 20 fathoms deep. They follow a vein inclining to some depth, when it runs away in flat binns. When the stones part it, then they find a vein again. Their draughts are 14 or 16 fathoms, till they come to a stone, where they cast aside a draught called a cut. Then they sink plumb again 4 or 5 cuts, one under another. They find ore at 50

* The public should have been told what the medicines employed in these experiments were.

† See the former account by this gentleman, in N° 28, p. 186 of this Abridgement.

fathoms. Their best reaks are north and south: east and west are good, though not so deep. The groove is 4 feet long, 2¼ feet broad, till they meet a stone, when they carry it as they can. The groove is supported by timber of different thickness as the place requires. A piece of an arm's thickness will support 10 ton of earth. It lasts long: that which was put in above 200 years since will serve again in new works. It is tough and black, and being exposed to the sun and wind for two or three days, will scarcely yield to an axe. For the supply of fresh air there are boxes of elm exactly closed, of about fix inches in the clear, by which they carry it down above 20 fathoms. When they come at ore, and need an air shaft, they sink it at 4 or 5 fathoms distant, and of the same fashion with the groove, to draw ore as well as air. They make use of leathern bags, eight or nine gallons a-piece, drawn up by ropes to draw up the water. If they find a swallet, they drive an adit on level, till it is dry. If they cannot cut the rock, they use fire to anneal it, laying on wood and coal, and the fire is so contrived that they leave the mine before the operation begins, and find it dangerous to enter again, before it be quite cleared of the smoke, which has killed some. Their beetles, axes, wedges, unless they be so hardened as to make a deep impression upon the head of an anvil, are not fit for their use; and yet they sometimes break them in an hour; others last three or four days. They work in frocks and waistcoats, by tallow candles, 14 or 15 to the pound, each whereof lasts three hours, if they have air enough; which if they want to keep in the candle, the workmen cannot stay there. When a vein is lost, they drive two or three fathoms in the breast, as the nature of the earth directs them. They convey out their materials in elm buckets drawn up by ropes, the buckets holding about a gallon. Their ladders are of ropes.

The ore runs sometimes in a vein, sometimes dispersed in banks. It lies often between rocks: some of it is hard, some milder. They never find any perfect, but it must be refined. They have often branched ore in the spar.

There is about the ore some substances of spar and chalk, and another substance, which they call the crootes, which is a mealy white stone, marted with ore, and soft. The spar is white, transparent, and brittle like glass. The chalk white and heavier than any stone. The vein lies between the coats, and is of different breadths. It breaks off sometimes abruptly in an earth, called a deading bed, and after a fathom or two may come to it again, keeping the same point or direction. It terminates sometimes in a dead clayey earth, without croot or spar; sometimes in a rock called a fore-stone. The clearest and heaviest ore is the best; of which 36 hundred weight may yield a ton of lead. They beat the ore with a flat iron; then cleanse it in water from the dirt; and sift it through a wire sieve. The ore tends to the bottom, and the refuse lies at top.

And these are the preparations they make use of, before it is fit for fusion. For this purpose they have a hearth about five feet high, set upon timber to be turned as a windmill, to avoid the inconvenience of smoke upon a shifting wind. The hearth contains half a bushel of ore and coal, with bellows on the top. The charcoal is put upon the hearth, where the ore is; laying dry sticks upon the top, which they call their white coals. There is a sink on the side of the hearth, into which the lead runs, that holds about a hundred and a half. Then it is cast into sand, forming what are called sowes. They have a bar to stir the fire, a shovel to throw it up, and a ladle, heated red-hot, to cast out the melted metal. Once melting is enough: and the best, which is heaviest, melts first.

There is a flight in the smoke, which falling upon the grass, poisons the cattle that eat of it. They find the taste of it upon their lips to be sweet, when the smoke chances to fly in their faces. This brought home and laid in their houses, it kills rats and mice. If this flight mix with the water in which the ore is washed, and be carried away in a stream, it poisons cattle that drink it, even after a course of three miles. What of this flight falls upon the sand they gather up to melt in a flag-hearth, and make shot and sheet-lead of it.

Of Osteocolla, &c. near Frankfort on the Oder; also, an uncommon kind of Snow. By J. Chr. Beckman. N° 39, p. 771.

Osteocolla, or glue-bone, grows in a sandy yet not gravelly soil, and not at all as yet known in any rich or clayey ground. It shoots down 10 or 12 feet depth under ground; where the branches most commonly grow straight up, yet sometimes also spread sideways. The branches are of unequal thickness, like plants growing above ground, some of them thicker, some slenderer; and the farther they are distant from the common stem, the smaller they are; the stalk being thickest of all, usually equalling the thickness of an arm or a leg, and the branches of the thickness of a little finger. Where the osteocolla is found the sand is every wh e yellowish, and there appears a whitish fatty sand, which if it be dug into has under it a dark fatty, and (how hot and dry soever the other sand be) a somewhat moist and putrid matter, like rotten wood; which matter spreads itself here and there in the earth, just as the osteocolla itself does, and is called the flower of the osteocolla. The osteocolla being thus found, is quite soft, yet rather friable than ductile. So that to get out of the ground a whole piece of it, with its branches, the sand must be very carefully removed every way from it, and then let it lie so a while; its quality being such that remaining exposed to the sun for about half an hour or longer, it grows hard, as it is sold in the shops. This substance seems to be a kind of marl, or to have great affinity with it; of which we here also have great store, yet not near those places where I have found osteocolla. It requires time to come to maturity; which appears from hence, that in the very same place where I dug some of it the last year, I this year found others; yet with this difference, that those were grown hard, after the manner before described, but these remain still soft and friable, though now in the fifth month.

I shall further observe, that on the 1st of March last, there fell an unusual kind of snow, which I considered with more than ordinary attention. It had none of the ordinary figures, but was made up of little pillars, whereof some were tetragonal, some hexagonal, with a neat basis. On the top they were somewhat larger, as the heads of columns are. Considering the whole shape, we thought fit to give it the name of *Nix Columnaris*.

Extract of a Letter written by an observing Person to a Friend of the Editor, concerning the Virtues of Antimony. N° 39, p. 774.

I found that a boar, to which I had given an ounce of crude antimony at a time, became fat a fortnight sooner than one without antimony on the same food. Antimony will recover a pig of the measles;* by which it appears to be a great purifier of the blood. I knew a horse that was very lean and scabby, and could not be fatted by any keeping, to which antimony was given for two months together every morning, and that upon the same keeping he became exceeding fat. One of my own horses having had the farcins, and being cured, had notwithstanding extreme running legs; so that he passed the course of farriers twice without being cured, but on my giving him antimony one week only he was presently healed.

The manner of using it is this: Take one dram of crude antimony powdered for one horse, and when you give him his oats in the morning, shake it upon his oats in a little heap in the middle: if he be hungry, and you keep off his head from every other part of the oats, he will snap it up in his mouth at one bite when you loose him. Some horses like it much, others refuse it after the first; if so, cover it with oats thinly, or make it into balls.

An Account of some Books. N° 39, p. 779.

I. Olai Borrichii,† Medici Regii, et in Acad. Hafn. Prof. publ. De Ortu et Progressu Chemiæ Dissertatio, 4to, Hafniæ.

* Commonly so called; but probably a leprous or scrophulous affection.

† Olaus Borrichius was born at Ripen in Jutland, in 1626. He studied at Copenhagen under Wormius and Bartholine, and afterwards travelled into Holland, England, France, Italy and Ger-

This author's principal intention in his book, is to inform the curious of the origin and progress of chemistry; how it sprang up and flourished in Egypt; passed thence into Greece, Italy, Arabia, China, Spain, France, and all Europe. And because Conringius and Ursinus have called in question this progress, he endeavours to remove the objections which they have urged against it.

II. An Idea of the Perfection of Painting: originally written in French by Roland Freart Sieur de Cambray, and translated by J. Evelyn,* Esq. F.R.S.

This excellent idea is drawn in such a manner that it is demonstrated from the principles of art, and by examples conformable to the observations which

many. He took his doctor's degree at Angers. On his return to his native country in 1666, he was appointed to the professorships of philology, chemistry and botany, in the university of Copenhagen, was made physician to the king, and had other honours conferred upon him. Besides the work here mentioned, he wrote a treatise De Hermetis Ægyptior Sapientia, 4to, 1674, (a very learned performance, wherein he further vindicates the merits of the Egyptians in respect to science and inventions, and particularly in respect to medical and chemical science, from the attacks of Conringius) another treatise De Usu Plantar. Indig. in Medicina, 8vo. 1688; a Conspectus Chemicor. illustr. published after his death in 4to. 1697; various Dissertationes Academicæ, and several communications on medical subjects inserted in the Acta Medica Hafniensia. Add to these his classical work De Poetis Gr. et Lat. He died, after undergoing the operation for the stone, in 1690, aged 67; bequeathing a large sum of money for founding a medical college, to be provided with a botanic garden, a chemical laboratory, and a library, and endowed with a fund for the maintenance and education of a number of poor students. Borrichius was not only celebrated as a physician and a chemist, but held also a very distinguished rank among the scholars and critics of his days. At the same time he possessed the knowledge and talents requisite to form the magistrate and the statesman; the functions of both which he exercised with advantage to his country, in the honourable situations which he filled in the supreme court of justice and in the chancery.

* Mr. Evelyn was a very ingenious writer, particularly skilled in botany, natural history, and the fine arts; and had the honour to be one of the original members on the first establishment of the Royal Society. He was born in 1620, and died in 1706, in the 86th year of his age. Though Mr. E. was a very studious man, he was not of so recluse a character as to neglect public affairs; for on the retiring of Richard Cromwell he laboured zealously in bringing about the restoration; and on the king's return he was honoured with particular marks of his attention. In 1664 he was appointed one of the commissioners for the care of the sick and wounded: also, after the fire of London, one of the commissioners for rebuilding St. Paul's cathedral, and he paid considerable attention to that great work. About 1669 he was named one of the commissioners of the new board of trade. At the accession of James the IId, he was made one of the commissioners for executing the office of lord privy seal; and after the revolution he was appointed treasurer of Greenwich hospital. At his entreaty it was that lord Howard presented the Arundel marbles to the university of Oxford. Mr. Evelyn was author of a number of ingenious works, some of which are the following: In 1662 appeared his *Sculptura*, or the History and Art of Chalcography and Engraving in Copper, with an ample enumeration of the most renowned masters and their works; to which was annexed the mezzotinto manner of engraving, communicated to him by Prince Rupert. In 1664 came out his great work, Sylva, or a Discourse on Forest Trees, (lately edited with valuable notes by Dr. Hunter of York). And in 1697 appeared his Numismata, or Discourse on Medals. He wrote also several other books, political and philosophical, and some papers in the Phil. Trans.

Pliny and Quintilian have made upon the most celebrated pieces of the ancient painters; paralleled with some works of the most famous modern painters, Leonardo da Vinci, Raphael Urbino, Julio Romano, and N. Poussin.

Those principles of art constantly observed by the ancients in their works, are here enumerated to be five: 1. Invention or the history. 2. Proportion or symmetry. 3. Colour (wherein is also contained the just dispensation of the lights and shades). 4. Motion, in which are expressed the actions and passions. 5. The regular position of the figures of the whole work: of which the invention and expression are more spiritual and refined; the proportion, colouring, and perspective, the more mechanical part of this art.

III. Stereometrical Propositions, variously applicable, but particularly intended for Gauging, by Robert Anderson.* Printed in small 8vo. 1668.

This little work is an elaborate treatise on the measurement of all kinds of solids that can be generated by the rotations of circles and the conic sections, or the ellipse, parabola and hyperbola; namely, all spheres, spheroids, conoids, and spindles, &c. with their segments, zones, ungulas, &c.

IV. Elaphographia sive Cervi Descriptio Physico-Medico-Chymica, Auth. Joh. Andrea Graba, Med. Doct. Jenæ, 8vo.

In this small tract are delivered, from the best writers on this subject, and the author's own practice and observations, the nature, qualities, and uses of the stag. In it is particularly considered the longevity of this animal,† and its cause conjectured at, viz. the plenty of a balsamic preservative salt, with which it is said nature has stored this above many other animals: Then the successive growth and annual casting off of its horns, together with the causes thereof; but the author chiefly and largely insists on the uses of the several parts of a stag, which he finds to be very many, and of divers kinds, viz. ornamental, mechanical, culinary, and medicinal. He thinks all the parts of this animal, even the excrementitious parts, are endued with medical virtues; but it is to the volatile salt and spirit extracted from the horns and blood that he assigns the principal uses in physic; commending them as penetrating, opening, attenuating, abstersive and discussing.

* Mr. Robert Anderson was an ingenious mathematician, who was much noticed and encouraged in his studies by the mathematical Mr. John Collins, so often mentioned in the labours of the Royal Society. Mr. Anderson was not in the profession of mathematics, or other branches which he cultivated and improved, but was a silk-weaver by trade, and must have been a person of some consideration and substance, as he was able to make, at his own charge, some thousands of experiments with cannon, for improving the art of gunnery; which he did in a considerable degree; as appears by the treatises on that art which he published, as deduced from those experiments; viz. The genuine Use and Effects of the Gun, in 1674; To hit a Mark, in 1690; and, To cut the Rigging, &c. in 1691.

† The asserted longevity of this animal is a mere fable.

Among the many medical prescriptions set down here, the author gives us the podagric unguent of the so much famed Franciscus Jos. Borrhi, made up of almost all the parts of a stag: which how far it deserves commendations, must be learned from experience.*

The Variations of the Magnetic Needle predicted for many Years following. By Mr. HENRY BOND. *N° 40, p. 789.*

The doctrine of the magnet and magnetical motions is yet so obscure, that what hitherto has been discoursed and written upon that subject, proves very unsatisfactory. An intelligent mathematician and teacher of navigation in England, Mr. Henry Bond, having formed to himself an hypothesis of the variations of the needle, has thence calculated the following table; showing how the variations of the magnetic needle will fall out for many years to come; which variation he conceives is now westward, and to have been so for some few years past; whereas they were formerly eastward. This philosophical prediction is here made public, that inquisitive men may every where from time to time make observations accordingly, either to verify or to invalidate the proposed theory.

Years	Variation West	Years	Variation West	Years	Variation West	Years	Variation West	Years	Variation West
1668	1° 56′	1678	3° 46′	1688	5° 19′	1698	6° 52′	1708	8° 17′
1669	2 7	1679	3 50	1689	5 29	1699	7 1	1709	8 25
1670	2 18	1680	4 0	1690	5 39	1700	7 10	1710	8 33
1671	2 28	1681	4 10	1691	5 48	1701	7 19	1711	8 41
1672	2 38	1682	4 20	1692	5 57	1702	7 28	1712	8 49
1673	2 49	1683	4 30	1693	6 7	1703	7 36	1713	8 56
1674	2 59	1684	4 40	1694	6 16	1704	7 45	1714	9 4
1675	3 9	1685	4 50	1695	6 25	1705	7 53	1715	9 11
1676	3 19	1686	5 0	1696	6 34	1706	8 1	1716	9 17½
1677	3 30	1687	5 10	1697	6 43	1707	8 9		

* It is now known that the volatile salt and spirit extracted from the horns of the stag are in no respect different either as to chemical properties or medical virtues from the volatile salt and spirit extracted from the bony parts of other animals. The stag, therefore, need no longer be regarded as an animal furnishing peculiar materials for the use of the healing art. It would be no recommendation in these days of the above-mentioned unguent, that it was "made up of almost all the parts of a stag!"

Extract of a Letter written by M. Louis de Bils, to D. Tobias Andreæ of Duisburg; concerning the true Use of the Lymphatic Vessels, &c. N° 40, p. 791.*

In this extract we have an account of this author's eccentric ideas concerning the structure and uses of the lymphatic system. As these ideas are for the most part mere reveries, they are now justly exploded. It will not be necessary therefore to occupy the reader's attention with an analysis of them.

Of the Tides at Bermudas, also Whales, Spermaceti, strange Spiders Webs, some rare Vegetables, and the Longevity of the Inhabitants. By Mr. Rd. Stafford. N° 40, p. 792.

The water about our island (Bermudas) does not rise above five feet; and that but at one season of the year, viz. between Michaelmas and Christmas; at other times not above three feet. It is high-water when the moon is about an hour high; and the like after her setting. It flows in from the north-west, and runs to the south-east nearest; and in that part of the land which lies most to the north-west, it is high-water soonest. But the tide does not always ebb and flow directly that course quite round about the coast; which may be owing to some points of land or shoals that may divert its north-west and south-east course.

We have hereabout many sorts of fishes; and among them great numbers of whales, which in March, April, and May frequent our coast. Their females have abundance of milk, which their young ones suck out of the teats, that grow by their navel. They have no teeth, but feed on moss, growing on the rocks at the bottom during these three months, and at no other season of the year. When that is consumed, the whales retire. These we kill for their oil. Spermaceti whales have been driven on the shore here: which sperma, as they call it, lies all over the body of those whales. These have divers teeth, which may be about as large as a man's wrist. They are very fierce and swift, also

* Louis de Bils or Bilsius was a Flemish nobleman, who had an enthusiatic passion for anatomical pursuits, on which he devoted much time and lavished considerable sums of money. He wrote several anatomical treatises in the Dutch language; (they were afterwards translated into Latin) of which the most celebrated is that which bears the quaint title of Bloodless Anatomy; wherein he pretends to have invented a new method (which he kept a secret) of preserving or embalming dead bodies, and of performing dissections without effusion of blood. Of bodies or mummies thus prepared he once possessed a large collection, on which he set a very high price; but in process of time they became putrid, and he died of a consumption, occasioned (it is said) by the fetor emitted from his decaying mummies. Thus ended the boasted mystery of the anatomia incruenta. After the author's death it ceased to hold a place in the number of useful or even curious inventions.

very strong, being inlayed with sinews all over their body, which may be drawn out thirty fathoms long.

As to the age of our inhabitants here, some live to a hundred years and something upwards; many live till they are near a hundred, but few above: and when they die, it is age and weakness that is the cause, and not any disease that attends them. The air here is very sweet and pleasant, but the diet coarse.

Here are spiders that spin their webs between trees standing 7 or 8 fathoms asunder; which they do by darting their web into the air, where the wind carries it from tree to tree. This web when finished, will snare a bird as large as a thrush.

The houses are thatched with the leaves of the palmetto; some of which are eight or ten feet long and near as broad.

Of the polishing of Telescopical Glasses by a Turn-lath; also of an extraordinary Burning-glass at Milan. N° 40, p. 795.

There is an artist at Paris who polishes optic-glasses on a turn-lath, with the same ease as he turns wood.

Signor Settalla, at Milan, causes a burning-glass of seven feet in diameter to be made. He pretends to make it burn at the distance of fifty palmes, which is about 33 feet.

Concerning Cochineal. N° 40, p. 796.

It is generally believed that the cochineal * comes out of a fruit called the prickle-pear, bearing a leaf of a slimy nature, and a fruit blood-red and full of seeds, which give a die almost like to brasiletto wood, that perishes in a few days by the fire: but the insect engendered of this fruit or leaves, gives a permanent tincture, as is generally known.

There grows a berry in the Bermudas and New-England, called the summer-island-redweed, which is as red as the prickle-pear, giving much the same tincture, out of which berry come worms, which afterward turn into flies, somewhat larger than the cochineal-fly, and feed on the berry. It is said to yield a colour not inferior to that of the cochineal-fly, and as to medicinal virtue much exceeding it. It might be useful to try whether this Bermuda berry might not grow in England; also whether out of the berry of brasiletto wood the like insect might not be obtained in respect of colour or tincture? and whether a fading colour yielded by certain vegetables, might not be fixed by causing such a fermentation in the concrete, as may engender insects giving

* Coccus cacti, of which a description is given in the 52d volume of the Transactions.

the tincture of its original, which will hold in grain? In order to breed insects out of herbs, dry them, for so they yield the best tincture; otherwise stamp them and let them dry till they will suffer no more juice to run from them; this in the sun or in a proportionable heat; or if dried, infuse them with water in a heat for 24 hours; then evaporate the water till the infusion be as thick as a syrup, without straining them from their fœces; take this mass and put it into an earthen or wooden vessel covered with straw, or something of that nature, that it lie not too close, and so proportion the quantity to the vessel, that the air may come about and into the mass, yet not too much. Then set this vessel in a ditch or pit made in the earth in a shady place, and put about it some wet leaves, or some such putrifying matters, and over it a board, and on that some straw or the like; and it will produce first a shelly husky worm, and then a fly of the tincture of the concrete, but durable and somewhat higher. As for berries, stamp and boil them, evaporating them to the consistence of a rob, and then use them as the former. As for woods, infuse them in water, being first pulverized, and boil out their tincture; then evaporate the water to such a thickness as the other, and treat them in the same way. The flies will play about the sides of the vessel and the surface of the matter; which taken, are killed in a warm pan or stove, and so dried and kept.

[The whole tenor of this inaccurate and ill-written paper betrays great ignorance respecting the generation of insects. The coccus insect is not *engendered by* the juice of the cactus plant, but is merely *nourished by it*.]

Queries concerning Vegetation. N° 40, p. 797.

What vegetables are there which having the wrong end of them set downward into the ground, will yet grow; as it is said elders, willows and briars will? Whether the branch of a plant, as of a vine or bramble, being laid into the ground, whilst yet growing on the tree, and there taking root, being cut off from the tree whilst so growing, will shoot out forward and backward? In tapping, cutting, or boring of any tree, whether the juice that vents at it comes from above or below? What part of the juice ascends or descends by the bark? Whether what so ascends, does so by the outward or inward part of it? Whether, if a zone of about two or three inches be cut off about the bottom of a branch, that branch will die or cast its leaves, or bleed out a juice from the upper or lower part of the bark so cut, or be apt to shoot out leaves or branches or knobs either above or below that baring? Of what use is the pith? Whether the juice ascend or descend by it? And what effects will follow if the trunk be bored to the pith, and a peg driven hard into the hole of the pith both above and below? This to be tried in the most pithy plants. Whether the points or ends of the roots being cut off, the roots will bleed as copiously as branches of the trunks do when bored? What side of the tree affords most sap? Of what age trees afford most sap? What are the best seasons of the year for taking the sap of every kind of tree in greatest quantity; and how long that season lasts? Whether the sap comes more copiously at one time of the day or night than another? Whether trees afford any considerable juice in the fall? What effect copiousness or scarcity of rain has on the sap of trees? Whether the nature of a tree may be changed by applications of juices or liquors to the roots or other parts? Whether a tree whose root is covered from rain and not watered, if the branches of it be exposed to the air, will grow? Whether inoculated roots of a tree will grow? How short the arms of the roots of a tree may be cut, and the tree still grow? How deep the several kinds of trees are to be set in the ground to grow? Whether a seed being planted either way, it will grow equally? Whether the stem of a tree being set in the earth, and the root turned up into the air, the tree will grow?

Extract of a Letter written by Dr. Fairfax to the Editor, concerning a Bullet voided by Urine. N° 40, p. 803.

G. Eliot of Mendlesham in Suffolk, a pale, middle aged, full bodied woman, sorely afflicted for some years with a torment of the bowels, was prevailed upon by a neighbour who had suffered much in the like case, to swallow two fit bullets, which gave her immediate ease; but afterwards her pains returned and increased, and she having many conflicts for about 15 years, then applied herself to my apothecary, Mr. Gibson of Stow-market, who administered to her in the fit a dose of Lady Holland's powder, which she took in posset drink in the morning, was moved gently by it in the afternoon, spent that night in torture of body with vomitings, and next morning, during the use of the chamber-pot, together with the urine there came that from her, which giving a twang against the side of the vessel, surprised her with wonder what it should be; and the urine being poured off gradually, there was left in it a heavy and (to appearance) gravelly stone of a colour between yellow and red, near as big as one's thumb's end (as she confidently asserts to me;) but making use of a hammer, and knocking off the outer parts of its crust, they came at a bullet inclosed in it of a kind of brazen colour on the outside; but cutting a little with a knife it proved lead within: which being discovered could easily be accounted for. Asking her if no inquiry had been made of such a bullet's coming from her before, she told me that some days after she took them the stools had been slightly examined, but finding neither, they gave over search. Being farther asked about the size of the bullet, she told me it was apparently larger when she took it, than when she voided it. The state of her body in reference to the stone being inquired into, she said, that she had, before and since that befel her, been a voider of abundance of red gravel, and particularly about three years after she took them, she voided a considerable reddish stone. When I asked her about the manner of affecting her body at the coming forth? She answered, it was much like a common fit of the stone, only it held her longer (lasting some weeks) bowed her sadly forward, (as a stone often does in the ureters,) provoked to vomitings, and particularly she felt it crowd lower and lower from the kidney to the bladder in the left ureter. Asking her farther whether she was sure it came by the passage of urine and not by stool, she assured me she was not mistaken in that. And indeed the gravelly coat which the bullet has shows sufficiently whereabout it was lodged. Inquiring also whether the other bullet was come from her? She said no; for aught she knew it was still in her body. And as to her state since this evacuation, she said, that she has had ever since stone-colic pains, but none in so high a degree as before.*

This is the plain relation of the matter of fact. The main use I would make of the instance (if it be worth mentioning) is to strengthen a conjecture I have had a long time, of some other passage from the stomach to the bladder, besides what anatomists have hitherto given accounts of. For that this bullet never came at the bladder through the veins, arteries, lymphatics, &c. (the only vessels that can be charged with it) is, I think, beyond dispute. If it shall be said that nature, when put to shifts, finds out strange conveyances to rid the body of what is extraneous and offensive to it, I readily grant it, because many instances are known making that good; yet I think it not so pertinently urged, forasmuch as some other instances seem to side with it, which cannot be taken off by the same evasion; viz. Many do find that drinking four or five glasses of rhenish (for instance), within less than a quarter of an hour they shall have a strong inclination to make water, especially if the body has been agitated. Now that it should pass through the lacteals, veins, heart and arteries, and be strained from the blood in so short a time, is to me scarce conceivable.

But surely this shorter passage (wherever it is) is as natural as that by which it should have gone, had it staid longer in the body: Not to say how little it savours of the rankness of the kidney, and how much it resembles that which it was before it was taken into the body.† And methinks the conveyance of

* If the bullet in this case was really voided from the urethra, it must have made its way through the coats of the intestinal canal (in some part of the lower tract thereof;) and, insinuating itself over the uterus, have penetrated into the bladder. It is highly improbable that it should get from the intestines into either of the kidnies, and from thence to the bladder.

† This conjecture of a short road for fluids to the urinary bladder (without passing through the kidnies) although it has met with advocates among some celebrated modern physiologists, is not countenanced by anatomical researches.

the milk into the breast has much affinity with this of the urine into the bladder; the sudden pressing whereof into the paps after the nurses drinking ordinary milk could no more be explained by the ordinary doctrine of circulation than this of the urine into the bladder, till the shorter cut was hit upon by the ductus thoracicus; though ordinarily it may be strained in from the arteries, as the serum also in the kidneys; only in a milk-flood nature finds some other channel there, as here in a water-flood. Lastly, Sometimes things are shed forth at the nipples, almost as surprising as this we have spoken of at the neck of the bladder.

Account of some Books. N° 40, p. 805.

I. Joh. Hevilii Cometographia. Dantzick, An. 1668.

In this curious and learned volume the illustrious author has with great industry endeavoured to explain the whole nature of comets, their place, parallaxes, distances from the earth, beginning and end, the several appearances of their heads and trains, with their admirable motion; and all this by means of one constant hypothesis, viz. their motion in a straight line, and their generation from vapours issuing from the sun and planets; by which he judges that all the phænomena and questions touching comets hitherto known, may be rationally and conveniently explained and demonstrated: All illustrated by 38 schemes in folio, engraven by the author himself; as the whole book has been printed at his own charges. To which are added both a particular explication of the comets which appeared An. 1652, 1661, 1664, 1665; and a history of all the comets recorded by historians, philosophers and astronomers, from the Noachical deluge unto this day, enriched with the author's notes and animadversions, and a general table, representing as it were in one view the most remarkable particulars observed in all comets, viz. concerning the time of their first apparition, their duration, place, motion direct or retrograde, slow or swift; the size, figure and colour of their heads, and the magnitude, shape and position of their tails.

II. Renati Descartes Epistolæ; Pars I et II. Londini, An. 1668, in 4to.

Though some few of these letters were by the author himself written in Latin, yet the far greater part of them having been written in French, they are now come abroad all translated into Latin, for the benefit of those that are unskilful in the other language. They contain many philosophical questions and matters of all sorts, and an explication of many difficulties to be met with in the other works of the illustrious author; and were written to some of the most eminent persons for knowledge and learning of this age. They relate to a great variety of subjects, geometrical, arithmetical, musical, optical, mechanical, physiological, medical, metaphysical and moral.

There is a third part of the same author's letters yet remaining untranslated, which is likely to follow very shortly, with some other tracts concerning Man, and the Union of the Rational Soul with the Body; whereof the former was written by Descartes himself, the latter by the ingenious D. de la Forge upon Cartesian principles.

III. Scrutinium Chymicum Vitrioli, Auth. Joh. Georgio Trumphio, Jenæ, 1667.

This author endeavours in this small tract to show the nature, difference, choice, qualities and medical virtues of vitriol, together with the various ways of preparing both dry and liquid medicines out of that mineral. He describes the method of preparing vitriol used at Goslar in Germany, his native country. This process is now so generally known, that it cannot be necessary to insert the account of it here.

IV. Francisci de le Boe Sylvii* Praxeos Medicæ Idea nova, Lugd. Batav. 12mo. 1667.

In this treatise the author endeavours to assign the nature, causes, symptoms, and remedies of every disease. He introduces many speculations concerning fermentation; the noxiousness of all such things as either destroy or dull the acid spirit of the body in the work of nutrition; the dominion of the three humours in the body of animals, viz. the gall, the pancreatic juice, and the saliva, and their mixture, either immediate or mediate, with the blood returning to the heart; as also their great influence when they are vitiated in

* F. de le Boe Sylvius was descended from an ancient and noble family, and born at Hanau in 1614. After studying at Leyden, he went to Paris, and from thence to Basil in Switzerland, where he took his doctor's degree. He practised first at Hanau, then at Leyden, and afterwards at Amsterdam, until the year 1658; when he was appointed to the professorship of physic in the university of Leyden. Here he continued teaching with almost unprecedented celebrity until the time of his death, which happened in 1672, when he was in the 58th year of his age. Sylvius was an expert anatomist, and bestowed great attention on the dissecting and examining of bodies dead of different diseases. These dissections he regarded as so important towards an accurate knowledge of the seats, progress, and termination of diseases, that he caused a dissecting room for the examination of such patients as die of remarkable disorders, to be annexed to the hospital. He was one of the earliest defenders of Harvey's discovery of the circulation of the blood. He was moreover well versed in the chemistry of his days; but it was by his medical theories, at that time almost universally adopted, that he acquired so great a degree of fame. They produced among physicians a revolution of opinion, and made him the founder of a new medical sect. He taught that certain acidities and fermentations, and particularly the effervescence of the pancreatic juice with the bile, were the chief causes of diseases, and that the proper remedies for counteracting these diseases were absorbents, alkaline salts, both fixed and volatile, and opium. These theories, so little reconcileable to the condition of the living body either in health or disease, are now justly exploded. Besides the work here mentioned, Sylvius wrote various physiological and medical tracts, which after his death were printed collectively, under the title of Opera Medica, 4to, Amsterdam 1679, and Genev. folio, 1680.

disturbing the effervescence of the blood, as well as their power and virtue when they are tempered together in a due proportion, to cause a regular motion in the blood, and to convert meat into good nourishment: also about the change of the chyle into blood, and where that change is begun, where advanced, and where perfected: concerning the alteration made in the whole body of animals by the spermatic aura, as to their voice, fatness, sweetness, &c. About respiration, and how that may cease for a while in syncope and hypochondriacal suffocations without death; concerning sneezing, the hiccup, yawning, and their causes: the alteration which the blood of the left ventricle receives in the lungs by the inspired air, and the saliva or some other glandulous liquor: of the pulse; of the plenty of animal spirits and its cause; of the return of feverish fits by intervals, together with the cause thereof, &c.

Tides observed in Hong-Road, four Miles from Bristol. By Captain SAMUEL STURMY. *N°* 41, *p.* 813.

I have observed that our annual spring tides happen in March and September, either at the tide next before the sun's ingress into the equinoctial points of Aries and Libra, or the next tide after, according as the moon is then near her full or change: and then it rises in height about $7\frac{1}{2}$ fathoms, or 45 feet; the lowest neap-tides rising in height 25 feet. We observe also, that the lowest neap makes the highest spring, unless the north-east winds, by blowing hard, keep back the tides; and the contrary winds (south-west) if they blow hard, make here the highest. Concerning our diurnal tides, we observe, that from about the latter end of March till the latter end of September, they are about 1 foot 3 inches higher in the evening than in the morning; that is, when high water happens after the sun is past the meridian, or in the tides between noon and midnight: But from Michaelmas till Lady-day we find the contrary, the day tides being in that season higher by 15 inches than the night tides, or the tides between midnight and noon. And this proportion holds in both, after the gradual increase of the tides from neap to the highest spring, and the like decrease of their height till neap again. As for the highest menstrual spring-tide, it is always the third after the full moon or change-day, if it be not kept back by north-east winds. I have observed several times, that it flows here on the change-day, when the moon is east-south-east, the tide flowing in for the space of 5 hours, and ebbing 7 hours. There is some difference in reckoning the tides by the moon's bearing on such a point of the compass, on the full or change-day; for about that time only will the rule hold good. But from the change to the quarters, and from the full to the quarters again in the neap-tides, it does not flow here so long by two points of the compass. The water neither flows nor ebbs equal spaces in equal times; but its velocity is greatest at the beginning of the flood and ebb, and so gradually decreases till high or low water. This is observed in spring-tides only, which are as here set down in the following table, which I have made from my observations of our tides here.

The Tide-Table for Quarters of Hours.

Flowing of 5 Hours.								Ebbing 7 Hours.			
Time.		Height.		Time.		Height.		Time.		Height.	
hours.	quars.	feet.	inches.	hours.	quars.	feet.	inches.	hours.	quars.	feet.	inches.
0	1	2	7½	3	0	2	3	0	1	2	7½
0	2	2	6	3	1	2	2	0	2	2	6
0	3	2	6	3	2	2	1	0	3	2	6
1	0	2	6	3	3	2	1	1	0	2	6
1	1	2	6	4	0	2	1	2	0	9	0
1	2	2	5½	4	1	1	9	3	0	8	0
1	3	2	5	4	2	1	8	4	0	6	9
2	0	2	5	4	3	1	8	5	0	5	0
2	1	2	3	5	0	1	8	6	0	4	0
2	2	2	3	in 5 hours.		44	1	7	0	3	0
2	3	2	3					in 7 hours.		45	10½

The usual number of tides from new moon to new moon, or from the full to the full, is 59. In the Severn, 20 miles above Bristol, near Newnham, 160 miles from the river's mouth (Lundy,) the head of the flood, in spring-tides, rises in height like a wall near nine feet high, and so runs for many miles together, covering at once all the shoals which were dry before; at which time all vessels that lie in the way of these head tides, or boars, as they are popularly called, are commonly overset, or carried upon the banks; and the head of the tide being past, such vessels are left dry again. It flows there but 2 hours and 18 feet in height, and it ebbs ten hours. The reason of the said boar is doubtless the straightening and shoaling of the river in that place, it being there but half a mile broad; as it is but 20 perches over three miles higher, running tapering to Gloucester.

Observations in a Voyage from Spain to Mexico, particularly of the Minerals in that Country. By an English Gentleman. N° 41, p. 817.

Nature has so prodigally enriched this country of Mexico with all sorts of minerals, both perfect, imperfect, and mixed, that she almost overwhelms the observation of the most diligent and curious naturalists. I have conversed with the most skilful miners in those parts, but I found them to know of and care for little about minerals, except gold and silver. I was once desired to visit a famous cave there, some leagues from Mexico, on the north-west side of the city, beyond the lake: this was said to be gilded all over with a kind of leaf-gold, which had deluded many Spaniards with its promising colour, for they never could reduce it into a body, either by quicksilver or fusion; though the fame ran, that the ancient Indians knew how to make use of it, and that the great Montezuma had borrowed thence a considerable part of his treasure. I rode thither one morning, taking one Indian only for my guide. I found it situated somewhat high, in a place very convenient for the generation of metals. I went in with my candle lighted, but could not make the Indian follow me, being afraid of spirits and hobgoblins. The light of the candle soon discovered to me on all sides, but especially above my head, a glittering canopy of these mineral leaves; at which I greedily stretching forth my hand to reach some parcels of it, there fell down so great a lump of sand on my head and shoulders, that not only it put out my candle, but almost my eyes also. And calling out with a loud voice to my Indian, who remained at the mouth of the entry, there rebounded within those hollow caverns such thundering and redoubled echoes, that I admired it, and the Indian imagining by those tumultuous voices that I was wrestling with some infernal ghosts, soon quitted his station, and thereby left a free passage for some rays of light to enter, and to serve me for a better guide; my sight mean while being not a little endangered by the corrosive acrimony of the mineral dust. Having got my candle lighted again, I proceeded in the cave, and heaped together a quantity of the mineral mixed with sand, and scraped also from the surface of the earth a quantity of the same kind of glittering leaves, none of which exceed the breadth of a man's nail; with the least handling they divide themselves into many lesser spangles, and with a little rubbing they leave the hand all gilded over like gold. I began first to make experiment on the sand, which had been the matrix of the mineral; and there I tried first the ordinary way used in the Indies on such occasions, which was, to observe the colour of the fumes yielded from the spangled sand in a strong reverberating fire; but here little could be observed, by reason of the adust drying of the sand, not able to afford any visible fumes fit for such a discovery. I then proceeded to another way—to boil it in water, and having poured that off, to observe the alcali left after the water's evaporation. By this means I discovered, that it abounded rather in sulphureous unctuousness, than saline acrimony. Finding this, I applied first the quicksilver, mingled with the ordinary magistrals, as they call them, used in that country, to curb and break the force of these sulphureous impediments. But perceiving these to be of no effect, I assisted the quicksilver with the caput mortuum of vitriol and saltpetre, kept as a secret among the miners, but with as little signs of the mercury's operation as before. Then I boiled my mixture over the fire, a way found out in Peru in such difficult cases, but all to no purpose; so froward a matter it was, that it could not be brought to receive mercury, either by fair means or by foul. Then I devised a way to torment it with a corrosive of ordinary separating water, impregnated with common salt, and it made a dissolution, like that of gold; which, thus dissolved, I showed to a mineralist, who had been versed all his life-time in the separatory art of gold and silver; and he would not believe but that it was true gold. But having steamed away the aqua fortis, I found my hopes turned into a dirt something yellow, out of which, with distilled vinegar, enforced with its own tartareous salt, I extracted a tincture more curious than useful.

I shall only subjoin the grand use of mercury in separating silver in the Indies, when that metal is generated, as commonly it is, in certain rocky stones, abounding with bituminous and corrosive mixtures, so as to be impossible to free it entirely from its corrupt matrix, by the violent way of melting, whatever auxiliary ingredients may be added, as lead and artificial salts, and the like, because those sulphureous and vitriolic compounds, in the way of fusion, melting together with the silver, sublime part of it away in a volatile fume by their corroding acrimony, calcining and vitrifying the other part, and robbing the artificer of half his gain. In this case the use of quicksilver is found most advantageous, which is in this manner: Having reduced the ore into small pieces, they calcine it first in a reverberating oven, yet with a moderate fire, for fear of fusion, and driving away into the air part of the treasure; this calcination serves chiefly to free the mineral from what may hinder the operation of the quicksilver; and it serves also to discover, by the colour of the fumes it yields, what corrosive mixture chiefly abounds in it, besides that it renders the ore more tractable and pliant under the mill-stone, which is to reduce it to a small flour before the application of the mercury. This is chiefly used in those silver veins that are hard and dry; for such as are softer, abounding in oleaginous sulphurs, before burning are first ground into powder in such mills as are often seen in glass-houses: and then they receive a gentle calcination, mixing with them ingredients suitable to the peccant humour, if I may so speak, of the ore. As if, for example, the metal be sulphureous and antimonial, rust and dross of iron is found to be very proper: if martial, and abounding in iron, then sulphur and antimony reduced to powder is used to good purpose. Sulphur has a particular force to soften and dissolve iron. The ore being ground, calcined, and nicely sifted, they divide it in several heaps, and then, by lesser essays, they find out how much silver is contained in every heap; where it is very common to find only 6 ounces in 100 pounds, sometimes 12, but if it yield 18, it is esteemed a very rich vein; yet sometimes there are found great masses all of pure silver, called Virgin metal. Having discovered the quantity of silver in each heap, they proportionably besprinkle them with quicksilver at several times, stirring the ore about, to mix the mass well together. I find they have only conjectural signs to know when the mercury has entirely performed its office in separating all the silver from the heterogeneal substances. When by the colour of the mercury, coagulated by the silver in clear massy lumps, they conjecture the work done, they wash it by means of three vessels, standing in order one under the other, so that the matter in the first and highest vessel being washed and stirred about, all the dust of the heterogeneous minerals, that do not incorporate with the mercury, is carried away with the water into the other vessels, and from thence quite thrown out by the continual current of the water; in the mean while, the silver in clotted lumps, called pellas, is by the weight of the mercury depressed down to the bottom of the tubs or vessels.

The mercury with the silver is taken out of the vessels, and diligently squeezed in coarse and strong linen, and even beaten with a beetle, by which the quicksilver is separated, as much as may be, from the silver. And this mass is afterwards reduced, in moulds of the shape of the Indian pine-apple, into a pyramidal or conical figure, which they call Pineas de plata, and thus fashioned for the easier placing them round about the ridges of a large earthen vessel, like a blind alembic; about the top of which, a fire being made, all the rest of the mercury abandons the silver, and falls to the bottom. The silver is melted down with the liga, as it is called, which the king of Spain allows, by which he returns to the people in copper that fifth part which they allow him of all the silver.

To conclude, to give you some of my thoughts concerning the so much discoursed of transmutation of metals; concerning which I am of opinion, that that change is erroneously apprehended by many, imagining that the whole imperfect metal is totally transformed into the more perfect, by the substance mixed with it; whereas the mixture added to the melted metal, joins itself, as

I conceive, to those parts which, being homogeneal, symbolize together with the nature of the more perfect, whereby the pure metalline parts are separated from the other heterogeneal impure sulphurs, which together with other causes hindered nature in the mine from concocting that substance into the more perfect metal.

Observations in Jamaica. By Mr. Norwood, *Jun.* N° 41, *p.* 824.

Aligators are shaped and they walk like lizards. Those of a full growth have teeth like a mastiff, and a mouth a foot and a half wide. The smell of them is so strong that it is felt at a great distance. They may be mastered and killed by any one dextrous and skilled in the way of doing it; which is, that a man be armed with a long truncheon, and fall upon them side-ways; for doing it front-ways they are too nimble for the assailant, and may by leaping upon him, which they can do the length of their whole body, overpower him; but if he strike them with his club on their shoulder, and behind their fore-feet, and lame them there, the beast being thereby rendered unable to move, is easily subdued.

Tortoises die if their blood be heated; as their blood must not be hotter than the element they live in. The chegoes are not felt till a week after they have got into the body. They breed in great numbers, and inclose themselves in a bag, which when observed, there are certain skilful persons, who with little pain take them out; taking care to extract the whole bag entire, that none of the brood, which are like nits, be left behind.

The shining flies are a kind of cantharides, looking green in the day time, but glowing and shining in the night, even when they are dead; this relator affirming, that he has applied them dead to a printed and written paper in the dark, and read it.

The Manchinel apple is one of the most beautiful of fruits to the eye, the most agreeable to the smell, and pleasant to the taste, and thence called the Eve-apple; but if eaten, it is certain death. The wood of it yet green, if rubbed against the hand, will fetch off the skin, or raise blisters; and if any drops of rain, falling from this tree, light upon one's hand or other naked part of the body, it will also have the same effect.

An Account of two Books. N° 41, *pp.* 833, *&c.*

1. Tractatus duo, prior de Respiratione, alter de Rachitide, A. Joh. Mayow,* &c. Oxon. 1668, in 8vo.

* As an account of the life and opinions of Dr. Mayow was published only a few years ago by a

The author in the former of these tracts having first given an account, how the air by its elastic force is inspired, and upon the dilatation of the chest caused by the intercostal muscles drawn upwards, rushes into the lungs, which are thereby expanded, being nothing else but a body made up of very thin little membranes, in the form of innumerable small bladders; delivers his thoughts of the use of respiration, waving those opinions that would have respiration serve either to cool the heart, or to make the blood pass through the lungs out of the right ventricle of the heart into the left, or to reduce the thicker venal blood into thinner and finer parts; and affirming, that there is something in the air absolutely necessary to life, which is conveyed into the blood; which, whatever it be, being exhausted, the rest of the air is made useless and no more fit for respiration. Where yet he doth not exclude this use, that, together with the expelled air, the vapours also steaming out of the blood are thrown out.

And inquiring, what that may be in the air so necessary to life, he conjectures that it is the more subtle and nitrous particles with which the air abounds that are communicated to the blood through the lungs; and this aërial nitre he makes so necessary to all life, that even plants themselves do not grow in earth deprived thereof, which yet, being exposed to the air, and afresh impregnated by that fertilizing salt, becomes fit again to nourish those plants.

And considering further, what part this nitrous air acts, and what operation it performs in the animal life, he is of opinion, that this nitre, mixed with the sulphureous parts of the blood, causes a due fermentation, which he will have raised, not only in the heart alone, but immediately in the pulmonary vessels, and afterwards in the arteries no less than in the heart. Examining also the reason why death so suddenly ensues upon respiration suppressed, the blood being then not yet unfit for motion, he inquires yet after another use of respiration, which makes it so very necessary to life. And considering with himself that the life of animals consists in the distribution of the animal spirits, for the supply of which is required the pulsation of the heart, and the afflux of the blood to the brain, it seems to him that respiration is highly necessary to the motion of the heart, forasmuch as the heart is one of the muscles, the motion of every one of which absolutely requires this aërial nitre, so that without the same, even the beating of the heart cannot be performed.

But here he declares that he does not see how that explosion, by which the

physician now living, we deem it unnecessary to insert in this place a biographical notice of this distinguished chemist and physiologist. We shall only remark, that in his writings are to be found the primordia of some of the most important theories and experiments of modern chemical philosophers.

muscles are so suddenly inflated and contracted, should proceed from the arterial blood and the nervous juice. He considers rather that the nitrous particles proceeding from the inspired air, do by the afflux of the arterial blood every where flow between the fibres of the muscles, and lodge therein; and that the animal spirits made up of a very volatile salt, and not much differing from the distilled spirit of blood highly rectified, do as often as they are sent from the nerves for motion, meet with the former nitrous and differing particles; by which mixture of a kind of volatile spirit of blood, and a saline liquor united together, is caused that sudden explosion, and consequently the inflation and contraction of the muscles. To which ebullition, he says, the blood may perhaps also contribute something, forasmuch as its sulphureous particles, conjoined with the nitre inspired, may render that juice nitro-sulphureous, and yet more explosive. And thus he thinks the motion made in the heart, (a muscular substance) to be done no otherwise than that in other muscles. Whence he concludes, that upon the suppressing of respiration, when that darting nitre so requisite to all motion is deficient, the cardiac nerves convey their influx in vain, so that the pulsation of the heart ceasing, and consequently the afflux of the blood to the brain, death must needs follow; but yet that the animal may live a while without respiration, forasmuch as the blood contained in the vessels of the lungs, and impregnated with air enough, may suffice to maintain for some few moments the motion of the heart.

And thus much of the first tract; the other, treating of the rickets, examines in the first place wherein nutrition consists, and finds that the nervous juice performs not alone the whole office of that operation, in regard that besides it the blood diffused through the arteries has no small share in that work, seeing that the nervous liquor mixed with the blood causes a certain effervescence, whereby the matter fit for nutrition is precipitated, and that for want of this nervous liquor the blood in this distemper of the rickets, though it be laudable enough, yet being destitute of its own ferment, is not able to excite heat in the parts, nor to execute the office of nutrition. So that the rickets, in the opinion of this author, are a disease caused by an unequal distribution of the nervous juice, from whose either defect or superabundance some parts defrauded of nourishment are emaciated, others being surcharged, grow into a disproportionate bigness. Proceeding to assign the cause of this inequality in the distribution of the aliment, he finds it not in the influx of the brain, but in the obstruction of the spinal marrow, whence it happens, that, this high way of the passage of the spirits being dammed up, the parts to be sustained and cherished by that nutritious juice, must needs languish and fall into an atrophy and the highest consumption.

Having assigned this cause, he endeavours thence to deduce all the appearances and symptoms peculiar to this disease, and suggests that upon that ground it ought to be the main intention of the physician to remove such obstructions, and to strengthen the nerves: subjoining a general method for curing this infirmity, and specifying the principal remedies both internal and external to be used therein; among which he chiefly recommends the spirits of sal ammoniac, hartshorn, blood, urine, soot, as such, that by the high volatility and subtleness of their parts are able to dislodge those obstructions which cause this disease.*

II. A Discourse concerning Physic, and the many Abuses thereof by the Apothecaries. London, An. 1668, in 8vo.

In this discourse there is nothing sufficiently interesting to require notice.

A New and Universal Method for working Convex Spherical Glasses on a Plane. By S. Mancini. N° 42, p. 837.

This method is found in an Italian book entitled L'Occhiale all' Occhio, overo Dioptrica, Prattica del Carlo Ant. Mancini, in Bologna 1660, in 4to. It treats of light; of the refraction of rays; of the eye and the sight; and also of the considerable helps that may be afforded to the eye, to make it see what is almost incredible. Besides which there are delivered in it the practical rules for working spectacles for all sorts of sights, and especially telescopical glasses for observing the planets and fixed stars by sea and land; and others to magnify the smallest of near objects thousands of times. There is also among the rest, a particular way, called new and universal, for making convex-glasses on a plane for all practicable lengths of diameters of spheres, without other dishes or concave moulds; which is as follows:

To give a spherical figure to a plane by a plane, which at first sight may seem a paradox, by moving one plane on another by a circular motion, proceed in this manner: Let the piece to which the glass to be wrought is fastened be adjusted in the head of a pole, which is to be of such length as the semidiameter of the sphere of the lens requires; and on the stool or form where

* In the disease termed rickets there appears to be a deficiency of ossific matter (phosphate of lime). Hence, in addition to the usual strengthening measures, such as a nourishing diet, country air and chalybeates, some physicians have of late recommended the internal use of phosphate of lime joined with phosphate of soda, washing the surface of the body at the same time with potash dissolved in water. Perhaps however the latter part of the treatment here mentioned will be regarded by some as too chemical; since if we improve by the first mentioned remedies the general state of health, and with it the processes of digestion, nutrition and secretion, a healthy ossification will necessarily follow.

you intend to work, let there be put a plane of iron or other metal horizontally; and perpendicularly over this plane let the pole be fastened to the ceiling of the room, if it be high enough, otherwise to another steady fastening lower than the ceiling, in this manner: about the head of the said pole let there be fastened a frame, made of two concentric rings or hoops, so that the one be moved within the other on two poles, and this other on other poles, moveable between two small arms fixed to the ceiling: which frame you may imagine like that by which the mariners compass is kept horizontal, or that which they use in Italy for carrying oil-lamps by night horizontally: or the same may be done with a ball moveable within two circles fastened on the top of the pole. All which will be better understood by the figures, where, in fig. 9, pl. 7, T is the lens, cemented to the piece E, fastened to the pole S, which passes through the centre of the inner circle B, moving on the pivots I, H, (fig. 10,) in the outer circle A; and this is fastened in a frame on the pivots L, M, in the arms C, D, (fig. 9,) in a wall, or above in the ceiling as was said; and above this frame let a pin be put through the upper pole, to hinder its getting out of the circle B, and that it may be raised a little, but not to be made lower by the workman. Or else, let the pole S be thrust into the ball O, N, (fig. 11,) moveable within the two circles P, Q, very well fixed to the two arms Z, Z; and let those two circles be made parallel, to prevent the ball from getting out. But the office of these two circles may be performed by one alone, but larger, in the manner of a socket, which may gird about such a part of the circumference of the ball in the middle as not to let it slip out. It is enough, if the ball do but move freely in it; yet so as always to touch it: which also is to be observed in the ball with two circles, by that means to keep always the centre when it moves just in the same situation. Let the plane of the iron, or other metal, F, (fig. 9,) on which the glass is to be ground, be placed level on the form G, to do which I have practised the following contrivance: let there be prepared two square planks of wood, F, R, equally thick, long and broad; but in the undermost let there be fixed a square ruler firm and solid, of a length equal to the thickness of both the planks; and in the upper plank let there be a square hole or groove so fitted, as that the plank may steadily slide on the ruler; and to such rulers, which may be called the regulators of the two planks, let there be made a ledge to keep the board more steady and firm upon it. Further, let these two planks have two gutters, R, U, going across from end to end; into which may pass two wooden wedges like Y, of which may be made four, to put one of them against another in the gutters. And then placing the plate F, T, level on the planks F, R, take a pendulum or other levelling instrument, and fit it on the said plate, and adjust it by the wedges to a level position.

The use of this instrument is very easy, since it is sufficient to guide with your hands the turn-tool fastened to the pole on the plane, where the sand is spread; making such turns as usual in this kind of work; and continuing so till the glass has taken its spherical figure. It may be polished on the same plane, applying to it the paper smoothly cemented on. But it is to be observed that the polishing by this instrument is very long and tedious; so that I would advise, after the glass is wrought to the perfect figure on the plane, to make use of certain gutters* proportionable to the sphere, whose semidiameter is represented by the length of the pole abovementioned; using the rules known and observed in the grinding of convex-glasses.

Though this contrivance be ingenious, yet skilful artists are of opinion that it will be very difficult to put it into practice with glasses of any considerable length.

Extract from the Giornale de Letterati, concerning two Experiments of Transfusion of Blood. N° 42, p. 840.

The following experiment was made at the house of Signior Cassini in Bologna, May 8, 1667, viz. The carotid artery of a lamb was opened, and the blood was let run as long as it would into the right branch of the jugular vein of another lamb, from which there had before been drawn as much blood as it was judged could be supplied from a lamb of the like size, whose blood should be let out till it died. After this, two ligatures were made pretty near one another in the vein of the lamb that had received the blood; and this vein was quite cut through between the two ligatures. This done, the lamb was untied, and without any appearance of feebleness went about, following those that had made the operation. It lived a long while after, and its wound being healed up, it grew like other lambs. But the 5th of January 1668, it died, and its stomach was found full of corrupt food. Its neck being dissected to see what had happened to the vein cut through, it was found that it had joined itself to the next muscle by some fibres, and that the upper part of that vein had a communication with the lower, by the means of a little branch, which might in some manner supply the defect of the whole trunk.

There was made another experiment the 20th of May last at Udina, at the house of Signior Griffoni, by transfusing the blood of a lamb into the veins of a spaniel, of a middle size of that kind, 13 years old, which had been deaf for

* These gutters are thus described: A polisher must be made in the form of a gutter, excavated its whole length; which may also be hollowed spherical by means of a wooden mould, turned of a spherical figure by a gage, fixed on a mandril, and made to turn round.

above 3 years, so that whatsoever noise was made, he gave no sign of hearing it. He walked very little, and was so feeble that being unable to lift up his feet, he could only trail his body forward. After the transfusion practised upon him, he remained for an hour upon the table, where he was yet untied; but leaping down afterwards, he went to find his masters, who were in other chambers. Two days after he went abroad, and ran up and down the streets with other dogs, without trailing his feet as he did before. His appetite also returned to him, and he began to eat more and more greedily than before. But what is more surprising is, that from that time he gave signs that he began to hear, returning sometimes at the voice of his masters. The 13th of June he was almost quite cured of his deafness, and appeared without comparison more jocund than he was before the operation. At length, the 20th of the same month he had wholly recovered his hearing, yet in such a manner that when called, he turned back as if he that had called him had been very far off, though not always so.

Description of a New Microscope. By EUSTACHIO DIVINI. *N° 42, p. 842.*

Eustachio Divini has made a microscope of a new invention, wherein instead of an eye-glass convex on both sides, there are two plano-convex glasses, so placed as to touch one another in the middle of their convex surface. This instrument is peculiar in this, that it shows the objects flat and not crooked, and although it takes in much, yet it magnifies extraordinarily. It is almost 16½ inches high, and adjusted at four different lengths. In the first, which is the least, it shows lines 41 times larger than they appear to the naked eye; in the second 90 times; in the third 111 times; and in the fourth 143 times. Whence it may easily be calculated how much it magnifies surfaces and solidities. The diameter of the field, or the subtense of the visual angle measured upon the object-plate, in the first length is 8 inches 7 lines; in the second 12 inches 4 lines; in the third 13 inches; and in the fourth a little more than 16 inches.

In viewing with this microscope the small grains of searced sand, they perceived an animal with many feet, its back white and scaly, but less than any of those hitherto observed: for although the microscope showed every grain of sand as large as an ordinary nut, yet this animal appeared no larger than one of those grains of sand seen without a microscope.

Testis Examinatus. *N°* 42, *p.* 843.

This is the title of a printed page, formerly (viz. Anno 1658) at Florence, by Vadlius Dathirius Bonglarus, and now by reason of the great scarcity of the original, inserted here.

There having been at this period of time considerable dissensions among anatomists, concerning the structure of the testicles; some asserting them to be glandular, others that they were composed of a pulpy or parenchymatous mass, &c.; this author undertakes to settle this controversy, by demonstrating, from a careful examination of the testicles, that they are made up of a congeries of minute vessels.* These are represented in two figures, which accompany these observations, one of the figures exhibiting these vessels as they appear in the human testicle, the other exhibiting the appearances thereof in the testicle of a boar, fig. 3 and 4, pl. 8. A A, Both testicles cut through the middle. B B, The tunica albuginea. C, The insertion of the vasa præparantia into the tunica albuginea. D D, The ductus Highmorianus, running through the middle of the testicle, clearly seen in the boar, but not so in the human subject. Perhaps the linea fibrosa of Riolan? E E E E, The vasa præparantia passing through the tunica albuginea, and joined to the duct by a semicircular communication. F F F F, The substance of the human testicle, not glandular but vascular, in such manner, that the whole body of the testis is nothing but a congeries of vessels. In the boar there intervenes between the vasa testicularia **** a thin strip of flesh, (a thin partition of cellular membrane) f f f f.

G G, Small tubes arising from the duct, at the place where it passes from the tunica albuginea into the caput testis.

H H, The beginning of the epididymis, which is not glandular, (as Highmore supposes) but (as Riolan asserts) is composed of a congeries of vessels. Hence we see that the epididymis arises from the small tubes above mentioned, and these tubes from the duct; that the semen is first secreted in the vasa testicularia composing the body of the testis; that it next distils (is conveyed) from the vasa testicularia into the duct; and that it afterwards passes from the duct through the small tubes into the epididymis, in the convolutions of which it undergoes its ultimate elaboration.

I I, The other portion of the epididymis in like manner evidently vascular; so

* See the account of De Graaf's and Van Horne's treatises on the male organs of generation at pp. 241 and 242 of this Abridgement. Also Dr. Clarck's letter, p. 249—250.

that neither the substance of the epididymis nor of the testis itself, in the human subject, is glandular.

K K, The vas ejaculatorium, a direct continuation of the epididymis.†

An Account of two Books. N° 42, p. 845.

I. A Continuation of New Experiments Physico-Mechanical, touching the Spring and Weight of the Air, and their Effects; the first part, &c. by the Honourable Robert Boyle, Fellow of the Royal Society. Oxford 1668, in 4to.

The illustrious author of this book has again furnished the philosophical world with a set of very material and pregnant experiments, which are partly improvements of the former of this nature, partly superadded new ones; concerning which, he declares, that in great part he aimed thereby to show, that these very phænomena, which the school philosophers urged as clear proofs of nature's abhorrency of a vacuum, may not only be explained but actually exhibited, some by the gravity, and some also by the bare spring of the air; which latter he now mentions as a distinct thing from the other, not as if it were actually separated in these trials, since the weight of the upper parts of the air does, as it were, bend the springs of the lower, but because that having in the formerly published experiments, and even in some of these, manifested the efficacy of the air's gravitation on bodies, he thought fit to make it his task in many of these, to show that most of the same things that are done by the pressure of all the superincumbent atmosphere acting as a weight, may be likewise performed by the pressure of a small portion of air, included indeed, but without any new compression, acting as a spring.

II. Hydrologia Chymica; or, the Chemical Anatomy of the Scarborough and other Spas in Yorkshire, &c. by W. Sympson. London, 1668, in 8vo.

Owing to the very imperfect state of chemistry at this period of time, the account here given of the Scarborough water is not such as can be in the least degree interesting to the chemical reader of the present day.

† The course which the semen takes is, to denote the parts through which it passes by the terms employed in modern anatomy, as follows: It is secreted in the tubuli sive canales seminales (of which the lobuli of the testicles are composed); from these canals (which terminate in the rete vasculosum of Haller) it passes into the vasa efferentia (which issue from the aforesaid rete vasculosum) and is carried by them (the vasa efferentia testis) to the coni vasculosi of the epididymis; then along the canal of the epididymis; from whence it is conveyed by the vas deferens to the vesiculæ seminales; and lastly (dum coitus fit) it is discharged from these reservoirs (the vesiculæ seminales) by their excretory duct (ductus excretorius sive ejaculatorius seminis) into the urethra.

Experiments and Observations on Vegetation and the Running of Sap, &c. By Dr. J. Beal and Dr. E. Tonge. N° 43, p. 853.

Dr. B. says it may be difficult to enumerate all the vegetables that will grow the wrong end set downwards in the ground. To mention some, there are elders, briars, saloes, willows, the black elder, vines, and most shrubs; two or three of their joints being covered in the mould, and the stem cut off near the overmost joint, which should be half covered in the earth, and the mould somewhat raised as it grows. Dr. Tonge agrees, saying, that currant trees, and such like, as are of a soft wood, and quick growers, seem most apt to this improvement.

Dr. B. observes, that the branch of a plant, being laid in the ground, whilst yet growing on the tree, and there taking root, being cut off whilst so growing, will grow on both ends, if it be well rooted, and the like care taken of the last knot or joint as was before prescribed. Dr. T. says, that layers of those trees, mentioned in the former query, will grow on both ends, and aptly parted, when they have spread roots both ways, make two plants out of each layer.

Dr. B. says, in the tapping of trees, the juice certainly ascends from the root, and after it is concocted to partake of the nature of the plant, which feeds as well on the air as the juice furnished through the root, it descends to the orifice whence it issues. Ratray, the learned Scot, affirms, that he had calculated experimentally, that the liquor, which may be drawn from the birch in the spring time, is equiponderant to the whole weight of the tree, branches, roots, and all together. One experiment I must here mention. When both my hands were manacled for many years, and sometimes my arms also, with deep corroding tetters, to the blush of my many friendly physicians, and in despite of many of the best medicines and purgations, all was suddenly healed, and has so continued these 20 years, by the application of the gum of plum-trees dissolved in vinegar. I must not forget to add, that I applied vine-leaves, and sometimes opened raisins to draw a moisture from those tetters some few days before I used the gum.

Dr. T. is of opinion that sap always rises, and never properly descends, having only a kind of subsiding or recidivation, which he cannot call a circulation, nor resemble to the motion of liquors in a pelican; but rather to the sinking of liquors in an alembic, whilst the thinner parts are forced over the helm; yet somewhat imitating the motion of blood in animals, forasmuch as it continually supplies the want and expense of sap in the exterior parts, from the stock of the sap in the trunk, root, and branches. He understands it thus; That the sap necessary to the growth of the leaves, fruit, and upper branches, being dispensed and converted into the form necessary for those purposes, when the tree is fullest of sap, in such manner that the sap in the innermost coats feeds the innermost, and the sap of the outward coats the outward parts of fruits, &c. that which remains in the body between the several coats, and between the bark and body, begins to condense there also, first into a jelly, and after into wood, bark, roots, &c. according to the several places to which it subsides. And because it condenses faster in some parts than in others, according as they are higher or lower, the sap condensed above or below filling less room, must needs cause the sap which is not yet condensed in appearance to descend or subside, and to sink as it were lower and lower in the pores of the timber and bark. The trees observed to run are, the vine; the birch plentifully, at body, branches, and roots; the walnut-tree, at the roots and pruned branches; some willows and sallows, and some sorts of maple; the sycamore, which is the greater maple, at a gash made on the bark of its body, and at the root and branches; the poplar and asp; to these add the whitting, or quicken-tree, in Latin, Fraxinus Sylvestris, and by some Fraxinus Cambro-Britannica, which in its season, as some affirm, will run plenteously, and whence they would have us expect a sovereign drink against some stubborn distempers, especially such as are scorbutical and splenetic. I have kept (says Dr. T.) some of the juice of the berries (which, being expressed, ferments of itself) these two years in bottles, and it has now the taste of an austere cyder: And I suppose from its grateful smell, that it may be kept till it ripen and become a strong vinous liquor. It is the household drink of some families in these parts about Wales and Herefordshire, and some out of curiosity have brewed ripe berries with strong beer and ale, and kept it till it transcended all other beer in goodness. Dr. Tonge's attempts on the poplar, asp, elm, oak, ash, elder, whitting-berry or quicken-tree, thorn, buckthorn, tile, nut, sloe, briar, bramble, &c. have not succeeded; and he doubts that they, and all apples and pears, have some degree of gumminess in their juices, so that they will not run. Dr. B. says that apparently the sap rises by the inward bark, where the quick begins, and where the graft first incorporates. He farther remarks, there are circles observed in trees, which are the distances of those films or coats by which the tree receives its yearly increase in thickness. Through these, looking full of circular pores, the sap seems to ascend in the same manner between coat and coat, as between the bark and the body; and probably between the two outermost of these coats, as large a quantity of sap as between the bark and body.

The bark is two-fold, outward and inward. The outward is dry, and in some trees rough. The inner is probably a superadded new coat of that year's growth, or something like it, between the nature of wood and bark. The sap rises within and without that superadded coat.

Dr. B. says, if a circle be drawn round about any common English tree, as oak, elm, poplar, &c. by incision to the timber, how thin soever the knife be, so that no part of the rind or bark to the very solid timber be uncut, the tree will die from that part upwards, except the ash. To get the gum of plum-trees, I have sometimes wrenched the branch till the solid timber has cracked, and the rind forced open in some parts; so leaving it to grow, but forced to continue in a posture somewhat wreathed, it yielded me store of gum next summer.

Dr. T. remarks that a branch, whose bark of the breadth of about 2 or 3 inches is taken off round towards the bottom, in some trees, and particularly the lime-tree, will live and bear leaves for many years, and grow as other branches by means of the sap ascending through all the pores of the inner coats.

He further observes that piths are of a very different nature and substance. In the walnut it is a multitude of films manifestly distant from one another. In others, as in elders and briars, it is a continued, soft, loose, dry substance.

Also, that the points or ends of the roots being cut off, they will in proportion bleed as copiously as the branches, and probably more; certainly longer, because there is greater plenty of juice ascended above them than the branches, and consequently more will issue by them than by any part of the tree, higher than they are.

That from the latter end of January to the middle of May, trees will bleed. Those that are said to run first, are the poplar, asp, abel, maple, sycamore; some, as willows and the birch, tried by myself, are best to tap about the middle of the second season; and the walnut towards the latter end of March. They generally bleed a full month in the whole. Mr. Midford of Durham, a very expert gatherer and preserver of saps, affirms that the saps of the poplar and asp rise so briskly in January, that they will bleed before the end of that month. The sycamore will run in hard frost, when the sap freezes as it drops.

The best time of the day for tapping is about noon. In the latter season, when sap is not very plenteous in trees, they will neither run morning nor evening, nor probably at any time of the night; but when they are very full of sap, and emptied but by small vents, the sap may run night and day till exhausted; but never in large vents. And perhaps this observation may give light to that opinion which holds that the ascending of the sap depends on the pressure or pulsion of heat striking the earth, and thereby driving the moisture of the earth into the root.

Trees afford no sap at all, that has been observed, in autumn. And the quantity of sap depends on that of the rain.

Answers to Queries and Observations in the East Indies. By Sir PHIL. VERNATTI, *President in Java Major.* *N*° 43, *p.* 863.

The greatest length of time that pearl-divers in these parts can continue under water, is about a quarter of an hour; and that by no other means than custom: for pearl-diving lasts not above six weeks, and the divers stay a great while longer under water at the end of the season than at the beginning. Here at Batavia is an expert diver, who draws wages for nothing else but diving for anchors, guns, &c. lost in the road. I have seen him several times go down, holding my breath as long as I could, and he stayed ten times as long under water as I could hold my breath. But he will not go down unless you give him a whole pint of strong-water before.

The oil drawn out of the roots of cinnamon-trees, and resembling camphire, is thus: the roots being dried, bruised, and steeped in water, it is then drawn over by an alembic.

The lignum aloes is part of a living tree, but commonly found when it is withered. The tree itself is of a white soft wood, giving a milky juice, which is so venomous, that if, in cutting the tree, any of the milk drop on the eye, it blinds it; if on any other part of the body, it becomes scabby and a noisome sore. The lignum aloes or calambac is found within the white-wood, but not every where. When the tree decays, the white-wood soon withers and becomes worm-eaten; and the milk soon dries up, that it may be easily rubbed asunder with the hands. The best is found in the midst of the tree nourished by the heart-root, which goes straight down into the ground. The wood smells rank like human excrement, and grows naturally in the isles of Solor and Timor.

There are serpents in these parts, which have a head on each end of their body, called capra capella. They are accounted sacred by these people, and fortunate to those in whose house and lands they are found; but pernicious to those who do them harm.

The General Laws of Motion. By Dr. JOHN WALLIS. *Translated from the Latin.* *N*° 43, *p.* 864.

1. If an agent A produce the effect E, then the agent $2A$ will produce the effect $2E$; $3A$, $3E$, &c. and, universally, mA will produce the effect mE, putting m for the exponent of any ratio.

2. Therefore, if the power or force V move the weight P, then the force $m\,V$ will move $m\,P$, other circumstances being alike, as suppose through the same space in the same time, that is, with the same celerity.

3. Also, if the power move the body through the length L in the time T, then in the time $n\,T$ it will move it through the length $n\,L$.

4. Therefore, if the power V, in the time T, move the body P, through the length L, then the power $m\,V$, in the time $n\,T$, will move $m\,P$, through the length $n\,L$; and consequently, as VT, the product of the force and time, is to $P\,L$, the product of the weight and length, so is $m\,n\,V\,T$ to $m\,n\,P\,L$.

5. Because the degrees of velocity are proportional to the lengths or spaces run over in the same time, or, which is the same thing, reciprocally proportional to the times of running over that length; it will be, as $\frac{L}{T} : C :: \frac{m\,L}{n\,T} : \frac{m}{n}\,C$; that is, the degrees of celerity are compounded of the direct ratio of the lengths and the reciprocal ratio of the times.

6. Therefore, because $V\,T : P\,L :: m\,n\,V\,T : m\,n\,P\,L$, it will be, as $V : \frac{P\,L}{T} :: m\,V : \frac{m\,n\,P\,L}{n\,T}$; that is, $V : P\,C :: m\,V : m\,P\,C = m\,P \times C = P \times m\,C$.

7. That is, if the power V can move the body P with the celerity C, then the power $m\,V$ will either move the body P with the celerity $m\,C$, or the body $m\,P$ with the same celerity C, or lastly, any body with such a celerity that the product of the body and celerity be $m\,P\,C$.

8. And on this principle depends the construction of all machines for facilitating motions, viz. that in whatever ratio the weight is augmented, in the very same ratio the celerity must be diminished; so that the product of the celerity and the mass, to be moved by the same force, may be still the same, as suppose $V : P\,C :: V : m\,P \times \frac{1}{m}\,C = P\,C$.

9. If the body P, moved by the power V, with the celerity C, impinge directly against a quiescent body $m\,P$, (not obstructed) they will move on together with the celerity $\frac{1}{1+m}\,C$. For, because the power is the same, the greater the mass that is to be moved, so much the less must be the celerity; namely, as $V : P\,C :: V : \frac{1+m}{1}\,P \times \frac{1}{1+m}\,C = P\,C$. Consequently the impetus of the one body (that is, the product of the mass and velocity) will be $\frac{1}{1+m}\,P\,C$, and that of the other $\frac{1}{1+m}\,m\,P\,C$.

10. If a body P, moved with a force V, and celerity C, be impinged on by another body $m\,P$, moving the same way, but with a greater celerity $n\,C$, (and having therefore the force $m\,n\,V$), then they will both move on together, with

the celerity $\frac{1+mn}{1+m}C$. For $V : PC :: mnV : mnPC :: V + mnV = \frac{1+mn}{1}V : \frac{1+mn}{1}PC = \frac{1+m}{1}P \times \frac{1+mn}{1+m}C$. Consequently the impetus of the preceding body will be $\frac{1+mn}{1+m}PC$, and that of the following or pursuing body $\frac{1+mn}{1+m}mPC$.

11. If bodies moving contrary ways strike each other directly, as for instance, the body P, with the force V, and the celerity C, moving towards the right hand, and the body mP, with the celerity nC, and consequently with the force mnV, towards the left hand, then the celerity, impetus, and direction of both of them, after the impact, will be thus determined. The body moved towards the right would communicate to the other, supposing it were at rest, the celerity $\frac{1}{1+m}C$, and consequently the impetus $\frac{1}{1+m}mPC$, towards the right; and itself would retain that same celerity, and consequently the impetus $\frac{1}{1+m}PC$, towards the right, (by art. 9). And, after the same manner, the body moved towards the left hand would communicate to the other body, supposed at rest, the celerity $\frac{mn}{1+m}C$, and consequently the momentum $\frac{mn}{1+m}PC$, towards the left hand; also itself would have that same celerity, and consequently the impetus $\frac{mn}{1+m}mPC$, towards the left. Since, therefore, motion is made by both the bodies, the impetus of the body which at first moved towards the right, will now be the aggregate of the impetus $\frac{1}{1+m}PC$ towards the right hand, and $\frac{mn}{1+m}PC$ towards the left; consequently the motion will in reality be made either towards the right or left, according as the one or other of these is the greater, and with an impetus equal to the difference of the two; that is, putting the sign $+$ for the right hand, and $-$ for the left, the impetus will be $+\frac{1}{1+m}PC - \frac{mn}{1+m}PC = \frac{1-mn}{1+m}PC$, and the celerity $\frac{1-mn}{1+m}C$; which will be towards either right or left, according as 1 or mn is the greater. In like manner, the impetus of the other body, which at first moved towards the left, will be $+\frac{1}{1+m}mPC - \frac{mn}{1+m}mPC = \frac{1-mn}{1+m}mPC$, and its celerity $\frac{1-mn}{1+m}C$; which will be towards the right or left, according as 1 or mn may be the greater of the two.

12. But if the bodies should neither move straight in the same direction, nor in directly opposite directions, but impinge obliquely; then the foregoing calculus is to be regulated according to the degree of obliquity: observing that the impetus of a body impinging obliquely, is to that of its direct impulse, *cæteris*

paribus, as radius is to the secant of the angle of obliquity; which is also to be understood when the body falls, not perpendicularly, but obliquely on the surface of the body which is struck, as well as when the directions of their motion cross each other obliquely. This consideration, rightly applied to the foregoing calculus, will determine what will be the celerity, the impetus, and the direction of bodies thus impinging obliquely; that is, with what impetus, with what celerity, and towards what parts they will reflect from each other, which impinge in this manner. And the ratio is the same between the gravity of bodies descending obliquely, to that of such as fall perpendicularly; as I have elsewhere demonstrated.

13. If the striking bodies be not absolutely hard, as is above supposed, but elastic, yielding to the stroke, and then restoring themselves to their figure again by an equal force, the bodies, instead of moving on together, may in that case recede from each other, and that more or less in proportion to the restoring force; namely, if the impetus from this force exceed the progressive impetus.

In accelerated and retarded motions, the impetus for every moment of time, is that which answers to the acquired degree of velocity at each of these moments. When the motion is in a curve, its direction, in each point of the curve, is the same as the direction of the tangent to that point. And if an accelerated or retarded motion be made in a curve, as in the vibrations of a pendulum, then the impetus for each point is to be estimated both according to the degree of acceleration, and to the obliquity of the tangent at that point.

The Law of Nature in the Collision of Bodies. By Dr. Christopher Wren.* *Translated from the Latin.* N° 43, *p.* 867.

The proper and most natural velocities of bodies, are reciprocally proportional to those bodies. Therefore the bodies *R*, *S*, having their proper velo-

* Dr. (afterwards Sir) Christopher Wren, was one of the most extraordinary characters ever known, possessing the extremely rare qualification of uniting both theory and practice in a very eminent degree, being highly accomplished in the mathematical and philosophical sciences, as well as in the theory and practice of architecture. He was born in 1632, and had made great advances in the mathematics at 16 years of age. Being an Oxford scholar, he was one of those learned men who first associated together there for their mutual improvement in natural and experimental philosophy, and which at length produced the Royal Society, of which he was an original and, all his life, one of the most distinguished members. In the Society he gradually rose to the highest honours, and occupied the president's chair for two years, from 1680 to 1682. He made a multitude of ingenious and useful communications to the Society, as well of writings as of machines and instruments. He became successively professor of astronomy at Gresham college and at Oxford, making great improvements in that science. Soon after the great fire of London in 1666, from his

cities, also retain proper velocities after collision. And the bodies *R*, *S*, having improper velocities, are restored to an equilibrium by collision: that is, as much as *R* exceeds, and *S* is deficient of their proper velocity before impulse, just so much by the impulse is taken from *R*, and is added to *S*: and contrarywise.

Therefore the collision of bodies having their proper velocities, is equivalent to a balance oscillating on its centre of gravity. And the collision of bodies having improper velocities, is equivalent to a balance upon two centres, which on each side are equally distant from the centre of gravity: the balance beam being produced when necessary.

Therefore there are three cases of the improper motions of equal bodies. But of unequal bodies with improper motions, whether in the same or contrary ways, there are in all ten cases; five of which arise by conversion.

R, *S*, are the two bodies, either equal or unequal, *R* the greater, and *S* the less (*fig.* 12, 13, pl. 7); *a* is the centre of gravity, or the handle of the balance; *Z* the sum of the velocities of both bodies. Then,

Re / *Se*	given velocity of body	*R* / *S*	before impulse.	Or *So* / *Ro*	given velocity of body	*S* / *R*	before impulse.
oR / *oS*	required velocity of body	*R* / *S*	after impulse.	*eS* / *eR*	required velocity of body	*S* / *R*	after impulse.

The rule is, *Re*, *Se*, make *oR*, *oS*; *Ro*, *So*, make *eS*, *eR*.

Read the syllables, though disjoined, *Re*, *Se*, *oR*, *oS*, or *Ro*, *So*, *eS*,

skill in architecture, he succeeded Sir John Denham in the office of surveyor-general of the king's works; and from this time he had the direction of a great many public edifices, by which he acquired the highest reputation; as in the theatre at Oxford, St. Paul's cathedral, London, the Monument, the churches of St. Stephen Walbrook, St. Mary-le-bow, with upwards of 60 other churches which that dreadful fire had rendered necessary; also Chelsea college, one of the wings of Greenwich hospital, the modern part of Hampton Court, &c. He was one of the commissioners who, on the motion of Sir Jonas Moore, surveyor-general of the ordnance, was appointed to find out a proper place for erecting the national observatory; and he proposed Greenwich Park, which was approved of; the foundation stone of which was laid the 10th of August 1675, and the building was presently finished under the direction of Sir Jonas, with the advice and assistance of Sir Christopher. He became architect and commissioner of Chelsea college; also principal officer or comptroller of the works in Windsor castle; and he sat twice in parliament, as a representative for two different boroughs. He died in 1723, at 91 years, and was interred with great solemnity in the cathedral of St. Paul's. As to his person, Sir Christopher was of a low stature, and thin frame of body; but by temperance and regularity he enjoyed a good state of health, to a very unusual age. In his manners, he was modest, devout, virtuous, and very communicative of his knowledge, of which he possessed an extraordinary fund.—Sir Christopher never printed any thing himself; but several of his works have been published by others; some in the Philos. Trans. and some by Dr. Wallis and by other friends. His draughts and posthumous works were published by his son.

e R in the line of any case; and of these, that which is written in the scheme in the Hebrew manner, or from right to left, shows a motion contrary to the other motion, which is denoted by the syllables written in the Latin way, or from left to right. A syllable conjoined denotes the rest or quiescence of the body.

$$\text{Calculation.}\ \left\{\begin{array}{l|l|l} R+S:S::Z:Ra & Re-2\,Ra=o\,R & So-2\,Sa=e\,S. \\ R+S:R::Z:Sa & 2\,Sa\pm\ Se=o\,S & 2Ra+\ Ro=e\,R. \end{array}\right.$$

Nature observes the rules of specious (algebraical) addition and subtraction.

An Account of two Books. N° 43, p. 868.

I. Historia Cœlestis; Ex Libris et Commentariis MSS. Observationum Vicennalium Tychonis Brahe,* Dani, Augustæ Vindelic. An. 1666, in folio.

* Tycho Brahe was a celebrated Danish astronomer, in the 16th century, and famous for having given name to a new, though erroneous, system of the planets. Tycho was born, of a noble family, in 1546, and having studied Latin, from 7 to 12 years of age, under private tutors at home, he was then sent to study philosophy and rhetoric at Copenhagen. While there, the great solar eclipse, Aug. 21, 1560, happening at the precise time it had been foretold by the astronomers, he began to consider astronomy as something divine, and from that time devoted his attention chiefly to the celestial science, purchasing and privately studying every book relating to it that he could procure. After four years stay at Copenhagen, he was sent, in 1562, to Leipsic to study the law, where his acquirements gave clear indications of extraordinary abilities: taking every occasion however of improving his astronomical knowledge; and having there procured a small celestial globe, he took opportunities, when his tutor was in bed, to examine the constellations in the heavens, learning their names from the globe, and their motions from observation. After a course of three years study at Leipsic, he returned home, in 1565; in which year, being of an irritable temper, a quarrel arose between him and a Danish nobleman, when they fought, and by a stroke of a sword Tycho lost great part of his nose; which he ever after supplied so well by a substitute of precious metal and wax, that it was not perceived. About this time he began to apply to chemistry, proposing nothing less than to obtain the philosopher's stone. But becoming disgusted to see his favourite objects of study despised, he left his country and resided some years in different parts of Germany. In 1571 he returned to Denmark: and was favoured by his maternal uncle, Steno Billes, a lover of learning, with convenient accommodations at his castle for making his observations, and building a laboratory. And here it was that he discovered, in 1573, a new star in the constellation Cassiopeia. But soon after, marrying a country girl, beneath his rank, it occasioned such a quarrel between Tycho and his relations, that the king was obliged to interpose to reconcile them. In 1574, by the king's order, he read lectures at Copenhagen on the theory of the planets. Soon after this he again travelled through Germany, and had thoughts of removing his family and settling at Basil: but the king, Frederick II. unwilling to lose a man capable of doing so much honour to his country, promised to enable him to pursue his studies to his satisfaction, and bestowed on him for life the island of Huen in the Sound, ordering an observatory and laboratory to be built for him there, with a supply of money for carrying on his designs. Accordingly the observatory was founded in 1576, under the name of Uranibourg, with an ample pension from the king. This situation Tycho enjoyed for about 20 years, pursuing his observations and studies with great diligence; keeping always about him ten or twelve young men, who assisted him in his observations, and whom he instructed in astronomy

These observations of the noble Tycho, as they were procured and preserved by the Emperors, Rudolph II. Ferdinand II. and III; so they were lately by the command of his Imperial Majesty Leopold made public. They are ushered in by a liber prologomenos, compendiously representing the observations made from the time of the very infancy of astronomy unto that of its restoration by the illustrious Tycho; and reduced into seven classes, viz. 1. The Babylonian observations; from An. before Christ 721, to An. 432. 2. The Grecian; from An. before Christ 432, to the beginning of the vulgar Christian account. 3. The Alexandrian; from An. Christi 1, till An. 827. 4. The Syro-Persian; from A. C. 827, to 1457. 5. The Norimbergian, from A. C. 1457, to 1509. 6. The Borussian; from A. C. 1509, to 1529. 7. Mixt observations; from A. C. 1529, to 1582. In which year, 1582, begin the observations of Tycho, contained in 20 books, and made in as many years, ending An. Chr. 1601,

and mathematics. And here it was that Tycho received a visit from James the 6th, king of Scotland, afterwards James the first of England, who had gone into Denmark to espouse the princess Anne of that country. On this occasion James made Tycho some noble presents, and wrote a copy of Latin verses in his praise.

Tycho's tranquillity however in this happy situation was at length fatally interrupted. Soon after the death of king Frederick, by the aspersions of envious and malevolent ministers he was deprived of his emoluments, and obliged to quit his favourite Uranibourg, in 1596. He removed to Copenhagen, with some of his instruments, continuing there his observations and his experiments, till the same malevolence procured from the new king, Charles the 4th, an order for him to discontinue them. Tycho then turned his views to Rudolph, emperor of Germany, an encourager of learning, to whom he soon after repaired. That prince received him at Prague very graciously; accommodated him with a convenient situation, and a noble revenue, promising also to settle a fee for his descendants. Here then Tycho settled in the latter part of 1598, with his own family and his scholars, and among them the celebrated Kepler, who had joined him. But he did not long enjoy this happy situation; for he died about three years after, viz. in 1601, of a retention of urine, in the 55th year of his age; exhorting his scholars to attend closely to their exercises, and particularly recommending to Kepler to complete the Rudolphin Tables, which he had constructed for regulating the motion of the planets.

Tycho was author of many important works and improvements in astronomy. He was of a pious and devout disposition, which led him, to support the credit as he thought of the scriptures, to invent a new system of the planets, that might give stability to the earth, and motion to the sun. He was of a very irritable temper: a mere trifle put him in a passion; and against persons of the first rank, whom he thought his enemies, he openly discovered his resentment. He was very apt to rally others, but soon provoked when the same liberty was taken with himself. He was also very credulous with regard to judicial astrology, and superstitiously anxious about presages: if he met an old woman on first going out of doors, or a hare on the road in a journey, he immediately turned back, from the persuasion it was an ill omen: And during his residence at Uranibourg, he retained an idiot in his house, whom he placed at his feet at table, and fed himself; carefully noting all that was uttered by this madman, believing that every thing spoken by such persons had some secret important meaning.

which was the end of Tycho's life: Of which time yet there being wanting one year, viz. 1593, of the Brahean observations, that is supplied by the Hessian; and by a catalogue of the fixed stars, made and digested by the authority and care of that renowned prince for learning and magnanimity, William, Landgrave of Hesse, and by the labours of Rhothmann and Birge. To all these is added a continuation of such astronomical observations as were made from the time of Tycho's death, to An. 1635, by Mæstlin and Schickard.

II. R. P. Andreæ Tacquet* e Soc. J. Opera Mathematica. Antwerp, 1669, in fol.

These works contain—1. Eight books of astronomy, wherein the author has explained the whole doctrine of that science. It is remarkable that though he knows no argument, demonstrating the rest of the earth and motion of the sun; yet the authority of holy writ, now seconded by that of the sacred congregation of the cardinals, put it out of doubt. This author asserts, that the comets and new stars, that have appeared since 1572, have been far above the moon; and that Riccioli about this controversy seemed too favourably inclined to Claramont, asserting the contrary. Concerning the cause of the secondary light of the moon before and after the new, viz. the obscure part of her appearing like kindled glittering ashes, our author assigns it to be the sun's rays reflected from the bright hemisphere of the earth to the darker portion of the moon, and thence again directly reflected to the earth destitute of the sun's light. This phenomenon he says is learnedly explained in Philos. Optica Nic. Zucchii, from p. 247 to p. 260.

The author has not framed or annexed any tables to his book, although he abundantly shows how they may be computed: referring his reader to those of Tycho, Reinhold, Longomontan, Kepler, Lansberg, Wendelin, Bulliald, Petavi, Reiner, Riccioli; to which may be added those of Duret, Billy, Street, which last fixes the nodes and aphelions, and Wing's, now in the press.

2. Of practical geometry, 3 books.—In the first the author handles the construction of the tables of sines, tangents, and secants; the resolution of right lined triangles; the mensuration of the heights and distances of objects, as well unaccessible as accessible.—In the second book, he handles the dimension of plain surfaces, either regular or irregular.

In the third book the author treats of the measure of solids. And among

* Tacquet was one of those learned Jesuits who chiefly cultivated the liberal sciences in the 16th and 17th centuries. Besides this collection of his works, he had before published himself, Elements of Plane and Solid Geometry, also Arithmetic, both in 8vo. He was indeed a very laborious and voluminous writer; and died in 1660. In matters of astronomy, his fear of the church censures seems to have hindered him from more effectually defending the Copernican system of the world.

these, measures such solids as are contained in the mundane bodies, as the surface of the whole earth; where he is pleased to conclude, that at the day of judgment a less portion of it than England will serve to hold all its inhabitants and their infants, that ever have been, or in likelihood may be hereafter, till then, supposing the world should last 10,000 years!

3. Of optics, 3 books.—In the first he handles the simple and direct appearances of objects, meaning such appearances as are not liable to reflection or refraction. In the second, he handles the theory and practice of the perspective or scenographic projection, or transcription of a given magnitude into a plain, which cuts the optic pyramid; wherein he explains the direct appearance, and the monstrous deformation of an object, which at a certain place shall appear beautiful. In the third, he treats of the astronomic projections of the sphere, and thence derives the triple astrolabe, and shows their uses with the conveniences or inconveniences of each projection. It is observed that the horizontal projection is as ancient as Ptolemy, and all the four quadrants of several contrivances, published by Mr. John Collins, are derived from the western side, or the continuance thereof, admitting but a mere mutation of the names of circles, and a projecting of more parallels.

4. Of catoptrics, 3 books—In the first of which the author treats of catoptrics or reflection. In the second, of the affections of plain glasses simply, or of many such placed either in a parallel or inclined position to each other. In the third, of curved glasses; and therein first the chief affections of convex spheric glasses; afterwards of concave spheric glasses; lastly, of burning glasses of several kinds. The death of the author prevented him from writing of the dioptrics, which was very far advanced by Des Chartes, and has been since further promoted by De Beaune, Honorato Fabri, Manzini, and in the century of optic problems of Eschinard; and we may hope that ere long the learned Mr. Barrow will enrich the world with his labours of this and other kinds; also Mr. James Gregory, the author of Optica Promota, has a treatise of this subject in good forwardness for the press.

5. Follows the author's treatise of Military Architecture or Fortification.

6. Follow his Annularia et Cylindrica; the first four books whereof were first published in 1651.

Angeli not only answers what is objected by Tacquet against Cavaliri's Indivisibles, but shows what famous authors he has on his side, who have derived many excellent inventions from this method of indivisibles, viz. Beaugrand, Rocca, Magiotti, Van Schooten, Rich. White, Bulliald, Torricelli, who calls Cavaliri's first book the Ocean of Indivisibles, and the Fountain of Inventions. Of which doctrine he renders many excellent examples. After many other objections and observations, the relator adds: All which is not recited here to disparage our author, but to take off the prejudice which he may beget in his readers against the method of indivisibles, which has been owned by other famous men, besides those already named, viz. by Mengoli, who from the excellencies of this method, Archimedes' method, and Vieta's Specious Algebra, composed his Geometria Speciosa; by Antimo Farby, *alias* Hon. Fabri, in tract De Linea Sinuum et Cycloide; by Pascal, *alias* Dettonville; by Descartes himself, vol. 3 of Letters, who says, that by it he squared the Cycloid; and lately by the excellent Sluse, &c. 2. To remove the other prejudice that may be against this author as defective: for the 5th book Cylindricorum et Annularium, now printed with the rest, the prefacer asserts to be first extant in 1659. The author divides this fifth book into six parts: In the first he demonstrates, that if any plane surface have a rotation about its axis in any situation whatsoever, and at any distance whatsoever or none, it produces a round solid equal to an upright solid, whose base is the generating figure, and height equal to the circumference described by its centre of gravity. This universal rule was invented by Guldin, and is the basis of most of his doctrine; but he could not demonstrate the same, though it was much desired. In like manner, if any perimeter have a rotation about its axis, in any situation whatsoever, it generates a round surface, equal to a right surface, made by the same perimeter as a base (which may be evolved and made a plane surface) whose height is the way or circumference described by its centre of gravity. These being two admirable universal rules in geometry, the reader will find the same (with many others) demonstrated by Dr. Wallis in his treatise De Calculo Centri Gravitatis, which, together with his other tracts, De Motu, Statica, Mechanica, are now at the press in London. The same rules are likewise demonstrated in Geometriæ pare Universali Jacobi Gregorii Scoti, Patavii, 1668. The methods of these learned men are different, and good arguments might be given, that they have not communicated nor seen the works of each other.

Guldin, l. 1, c. 12, shows a mechanical way to find the centre of gravity of a surface or curved line, by two free suspensions, from the points of which, perpendiculars being drawn, do cross each other at the centre of gravity. This we mention to keep the reader from taking the centre of gravity of a curved line as such, which is intended in this second rule, to be the same with the centre of gravity of the figure thereby terminated in the first rule. 3. Considers the affections of round solids, generated from a parabola, in ten propositions; whereof the 21st and 23d give the hoof, required by Angeli, which was formerly cubed by Greg. de S. Vincentio. In the 27th proposition he gives the proportion of the parabolical conoid to the spindle made of the same parabola by rotation about its base, to be as the base of the parabola is to $\frac{16}{15}$ of the axis; showing, that Guldin erred through forgetfulness. In proposition 29 he shows, that the parabola bears such a proportion to a circle described about the base as a diameter, as the axis of the parabola doth to that circumference of a circle, whose radius is equal to the distance of the centre of gravity of the semi-parabola from the axis. 4. Contains divers endeavours and manifold new ways towards the obtaining the quadrature of the circle in 12 propositions. 5. Contains ten propositions, from 41 to 51, in the 42d whereof he finds a sphere equal to an hyperbolical ring solid; whence divers ways are opened towards the attaining the quadrature of the hyperbola: and he finds a sphere equal to a ring made by the rotation of a segment of an hyperbola, and of the segment of a circle thereto annexed, described about the base of the hyperbola as a chord line: Then he absolutely cubes certain hoofs cut out of an hyperbolical cylinder, and thence derives other ways towards the obtaining the quadrature of the hyperbola. 6. Delivers 3 theorems, showing the proportion between an hyperbola and a circle: which are conceived to be wholly new.

But these theorems suppose the quadrature of both figures known, viz. that of a circle, in requiring the length of the circumference of a circle, described by the centre of gravity of an hyperbola; which centre cannot be found, without giving the quadrature or area of the hyperbola: which has been most happily performed by M. Mercator in his Logarithmotechnia, and further advanced by Dr. Wallis, in N° 38 of these Transactions; and by Mr. Gregory also further promoted and otherwise performed, in his Exercitationes Geometricæ, where he shows the same methods and approaches to be likewise applicable to the circle.

*A Continuation of Observations on Vegetables. By Dr. E. Tonge. N° 44, p. 877.**

If no rain come to the roots of trees, nor other moisture, they will not grow, but if the points of the roots only be watered, though all the rest remain dry, as it happens naturally in fir trees, they may grow very well. For the points of the roots shoot out yearly a sharp-pointed tender part, somewhat like the sharp bud on the end of a sprig, by which the root not only enlarges itself in the earth, as the branch does in the air, but also receives its nourishment. And that tender part moves towards the best moistened and the tenderest earth; so that to promote the growth of trees, it is very effectual to loosen the earth of trees about the points of the roots.

The roots of plum and lime trees inoculated upon, will shoot out their buds.

* See No. 43, p. 304.

To make a successful trial, let the root be bared in the fall of the leaf, taken out of the earth, and at convenient distance from the body of the tree, bowed, and raised a foot above the earth, and then the points and fibres of the root carefully laid about with fresh earth, and watered till they take well, and till the root raised in the air have a bark like that of a branch of a tree, which probably it will get in the next season of inoculation. The inoculation itself is made on the part raised, after the ordinary way. When it is done, let it be carefully covered with some soft wax, to defend it from the rain.

The arms of the roots of trees are to be cut for the advantage of their growth, according to the proportion they have to their head and body; or according to the design you have to increase wood or fruit. For such roots as are more outward feed the wood, but such as are inward the fruit.

The depth of trees to be set, should never be below the reach of the sun's heat, nor the goodness of the mould, and rather too shallow than too deep; forasmuch as they are apter to sink lower, than to raise themselves upwards, if they be out of the convenient reach of the sun's heat, the cause of pulsion and nourishment.

The seeds of the fir, pine, &c. which bring up the shells of their seeds on the heads of the first shoot, will either not grow at all, or difficultly, if the blunt end be put downwards, because in that posture it must turn itself, before it can emerge into the air; for the root is shot downwards at the sharp end. But it may very well grow if set horizontally.

Some young plants, if their heads be kept moist, will live all winter, if it be mild, though their roots be in the air, as I tried in seedlings of apples and crabs. The roots, set afterwards in the spring, grew and lived. The reason why some plants grow in sticks, may be the softness of such wood, apt thereby to receive nourishment like a root, and to shoot out roots and fibres from themselves. But in slips taken from firmer wooded trees, as bays, a moist temperate season is to be observed, and a stone, or chip of wood to be closed to the end of the slip, and set in the earth with it, which helps its rooting.

The sap of a large walnut in the latter season of its running, *i. e.* when it yields no sap any longer in the body or branches at any time of the day, runs longer at the roots on the south or sunny side, than on the north or shady side.

As plenty of rain can cause no more sap than the pores of the root, body, and branches will admit; which must stay some time to be digested, and converted into nourishment: so too much cold rain may, by over-cooling, hinder the sap, by abating from the degree of heat necessary to the pulsion of sap into the root, and to the digestion in the tree, which is also in watering. On this ground it seems probable, that drawing sap constantly from trees every year, will not hinder their growth in body, branches, leaves, nor fruit, to any great prejudice; for pulsion will still supply juice into the emptied pores, till their capacity be filled.

It is possible also, that trees may grow better, and give more fruit, if the right art of drawing sap be found out for that end; as some persons grow fatter by often bleeding. If plenty of sap drawn from trees hinder at all, it seems probable, that it will hinder growth of fruit, leaves, or uppermost shoots in tops of trees, and yearly shoots in extreme parts. And hence we have a probable reason of suckers robbing fruit, viz. because till the whole tree be filled with sap, the fruit cannot be served in the uttermost branches.—Wherefore not only suckers, but all superfluous not-bearing branches are to be carefully cut away before, or at the entrance of the spring.

An Extract of a Letter of Mr. JAMES GREGORY *to the Publisher, containing some Observations on M.* HUYGENS' *Letter, printed in Vindication of his Examen of the Book entitled Vera Circuli et Hyperbolæ Quadratura. N° 44, p.* 882.

See N° 37, p. 268 of this Abridgement.

Extract of the Anatomical Account, written and left by the celebrated Dr. HARVEY,* *concerning* THOMAS PARR, *who died in London at the Age of* 152 *Years and* 9 *Months. N° 44, p.* 886.

This account is annexed to a book, lately published in Latin by Dr. John Betts, one of his majesty's physicians in ordinary, and fellow of the London

* It is not possible to do justice to the memory of the great Harvey within the limits of a note. His life will perhaps be inserted in the miscellaneous volume intended to be added to this Abridgement. In the mean time, in place of a biographical sketch, we shall lay before our readers a summary account of the circulation of the blood, as explained and demonstrated by him in his immortal work, entitled Exercitatio Anatom. de Cordis et Sanguinis Motu, first published in 1628; though he had announced, several years antecedent to this publication, the leading facts belonging to this important discovery, in the lectures which he delivered before the college of physicians. In the above-mentioned treatise (the abstract of which here given is taken from the account of Harvey prefixed to the edition of his works by the London college) he shows, by experiments made on living animals, that the motion of the heart is performed by the contraction of its muscular fibres; that the auricles contract first, and thereby propel the blood into the ventricles; then the ventricles contract, whereby the blood is driven into the arteries; being prevented from returning into the auricles by the situation and connexion of the valves. Now as by the repeated contractions of the ventricles more blood is constantly propelled into the arteries than can be supplied by nourishment thrown into the veins

College of Physicians; in which treatise the author endeavours to show, that milk, or something analogous to it, is the universal nourishment of all living creatures, and the immediate and whole matter of blood; whence, and from the three parts thereof, viz. the butyraceous, serous, and caseous, and their various concoction in the stomach, and constitution in the veins, he would deduce the different nature of the humours and spirits composing the blood; as from the different quantity and quality of these he would derive the whole business of health and sickness, and the method of cure.

But as to the observations made by Dr. Harvey upon the person and dissection of Thomas Parr, it is noted:

(as appears upon calculation), and as moreover the arteries cannot receive blood through any other channel but the veins; it follows either that the veins must be quickly emptied, and the arteries on the contrary every moment more and more distended, which however is not the case: or that the blood must flow back again from the arteries into the veins, by certain secret passages, or by pores of the flesh, or by mutual anastomoses of the arteries and veins. He demonstrates that the last-mentioned communication takes place in the lungs. Again; as along the course of the arteries more blood is sent from the heart to all parts of the body than is necessary for the nourishment of those parts, he infers that the superfluous blood is returned by the veins (that they may not be left empty) from this fact, that no blood is found in the veins if the great artery be tied. On the other hand, if a ligature be passed round the vena cava at the place where it joins the right auricle, it will immediately become distended in a very surprising manner. Moreover, it must be evident to every one (he observes) who considers the situation and connexion of the valves, that the blood passes from the smaller branches of the veins into their trunks, and from thence to the heart. The true movement of the blood being thus discovered, Harvey was enabled through it to account for the distribution of nourishment and warmth to every part of the body, and to throw great light upon many obscure points relative to the animal œconomy, both in health and disease. No doctrine could be supported by proofs more simple aud decisive: nevertheless they were disputed by several of his contemporaries, and particularly by Riolan, to whom he made an able reply. Others, instead of combating the truth of his discovery, endeavoured to rob him of the merit of it, by pretending that it was known to Aristotle and Galen among the ancients, and to Servetus, Columbus and Cœsalpinus among the moderns. Some indeed of the last mentioned authors had made considerable progress towards a just conception of the blood's motion, intermingled, however, with the errors imbibed from the ancients abovementioned; but none of the passages quoted from their writings show that they taught or understood that the blood moved in a regular uninterrupted course from the heart to the arteries, and from them through the veins back again to the heart. As for what relates to the story of Pauli of Venice having been acquainted with the circulation of the blood before Harvey published his account of it; this circumstance has been cleared up in Dr. Clarck's letter, inserted in the preceding part of this Abridgement, p. 248. Thus does our countryman, the immortal Harvey, remain in full possession of the honour of a discovery which has led to elucidations of some of the most important phænomena of animal life, and is the main clue by which we have been conducted to a more accurate knowledge of diseased actions, and to more simple as well as more rational modes of counteracting them. Harvey was born at Folkstone in Kent in 1578, and died in 1658, having completed his 80th year. The London College of Physicians published his works in 4to, 1766. Among these his Exercitatio de Generatione Animal. holds the next place after his treatise on the heart and circulation above-noticed.

1. That he was a poor countryman of Shropshire, whence he was brought up by the Right Hon. Thomas Earl of Arundel and Surry,* and that he died,† after he had out-lived nine princes, and during the reign of the tenth, at the age of 152 years and 9 months.

2. That being opened after his death (Nov. 16) his body was found yet very fleshy, his breast hairy, his genitals unimpaired, serving not a little to confirm the report of his having undergone public censures for his incontinency; especially seeing that after that time, viz. at the age of 120 years, he married a widow, who owned, Eum cum ipsa rem habuisse ut alii mariti solent, et usque ad 12 annos retroactos solitum cum ea congressum frequentasse. Further, that he had a large breast, lungs not fungous, but sticking to his ribs, and distended with much blood; a lividness in his face, as he had a difficulty of breathing a little before his death, and a long-lasting warmth in his arm-pits and breast after it. His heart was great, thick, fibrous, and fat. The blood in the heart blackish and dilute. The cartilages of the sternum not more bony than in others, but flexile and soft. His viscera very sound and strong, especially the stomach; and it was observed of him that he used to eat often by night and day, though contented with old cheese, milk, coarse bread, small beer, and whey; and, which is more remarkable, that he did eat at midnight a little before he died. His kidneys covered with fat, and pretty sound; only in the anterior surface of them there were found some aqueous or serous abscesses, whereof one was nearly as large as a hen's egg, with a yellowish water in it, having made a roundish cavity, impressed in that kidney; whence some attributed a suppression of urine, which took place a little before his death; though others were of opinion, that his urine was suppressed upon the regurgitation of all the serosity into the lungs. Not the least appearance was there of any stony matter either in the kidneys or bladder. His bowels were also sound, a little whitish without. His spleen very little, hardly equalling the bigness of one kidney. In short, all his inward parts appeared so healthy, that if he had not changed his diet and air, he might perhaps have lived a good while longer.

3. The cause of his death was imputed chiefly to the change of food and air; for leaving a clear, thin, and free air, he came into the thick air of London, and after a constant, plain, and homely country diet, was taken into a splendid family, where he fed high, and drank plentifully of the best wines, whereupon the natural functions of the parts of his body were over-charged, his lungs ob-

* He was brought up to town in order to be shown to the king (Charles I.)

† November 14, 1635.

structed, and the habit of the whole body quite disordered, upon which there could not but soon ensue a dissolution.

4. His brain was found entire and firm; and though he had not the use of his eyes, nor much of his memory, several years before he died, yet he had his hearing and apprehension very well, and was able even to the hundred and thirtieth year of his age to do any husbandman's work, even threshing of corn.*

An Account of two Books. *N°* 44, *p.* 888.

I. De Viscerum Structura Exercitatio Anatomica Marcelli Malpighii. Bononiæ, 1666, 4to.

This book contains five dissertations: Of the liver, the exterior part of the brain, the kidneys, the spleen, the polypus of the heart. Concerning the liver, he, 1. gives a summary account of what has been said of it. 2. He relates what himself has observed in that viscus, in all sorts of living creatures, finding it to have lobes, and to be a gland of that kind which by anatomists is called conglomerate in contradistinction to the conglobate. 3. He examines the reasons given by Dr. Wharton† against its being a gland. 4. He assigns its office and use, making it no other than that it separates the gall, which being conveyed into the intestines, he asserts to be subservient to digestion.

Concerning the exterior part of the brain *(cerebri cortex)* he first inquires into the nature of its substance, and finds it a congeries of glands, more conspicuously so in boiled than in crude brains, and most discernible in fishes and birds, where he alleges an observation of a stone found in the brain, which was formed like the fruit of mulberries, conglobated and made up of many small kernels or grains, of ash-colour, probably thus formed by the petrified cortex of the brain, and so retaining the natural shape of the glands thereof. Next he solves the arguments of the above-mentioned Dr. Wharton produced in his book De Glandulis, against that opinion. Further, explaining the vessels of the brain, and their process, he affirms, that the whole substance called the medulla of the brain and the after-brain, is a heap of fibres or vessels, which, from the stock or trunk of the spinal marrow, by many windings and *crinkles*, form those cavities and involutions to be found there, and are at last deeply implanted in the very glands of the brain; where he teaches, that the whole work of separa-

* The account of this dissection is inserted in Harvey's Works, edited by the London College of Physicians.

† Thomas Wharton ranks among the best English anatomists of the 17th century. He wrote a treatise on the glands, entitled Adenographia, published in 1656. The salivary ducts which have been named after him, were known (as Haller remarks) to the ancients.

tion and depuration is performed by the inward structure of the glands of the brain, the juice passing immediately out of them into the hollow and fistulous fibres, to be conveyed by a continued course into the subjacent parts to execute its several offices, as is performed by the little tubes or pipes of plants; adding, for the illustration of the original of the spinal marrow and the nerves, that that marrow is a bundle of nerves, which whilst it makes up the brain, divides it into two parts (by the circumvolutions of which the sides of the ventricles are formed) and terminates at last in the cortex, wherein, and in whose glandular grains the extreme roots of the nerves, in the smallest size, are implanted. After this he proceeds to the use of the cortex, and is of opinion, that by these little glands there are separated and collected those particles which nature has designed for instruments of sensation, and by which, when conveyed through the tubulous nerves,* the coherent parts are impregnated and swelled, and the animal made sensible of the operations of several objects. Moreover he offers some considerations upon Dr. Willis's opinion about the production of the internal senses by virtue of the brain's structure; and also upon his ascribing to those bodies, which he calls striata and radiosa, a twofold texture, whereof the one ascends, the other descends, for the perception of the impressions of sensible objects by the former, and the performance of motions by the latter. Lastly, he takes notice that Dr. Glisson † has derived the matter of the nervous juice through the nerves into the brain, from the glands of the mesentery, and Fortius from the mouth and intestines; whereas, since he has observed the mass of the brain made up only of a glandular cortex, and of fibres proceeding thence, together with the sanguineous vessels, and not yet found any cavities for receiving the chyle, and conveying it into every part of the brain; he therefore conceives, that all the nerves are produced out of the brain and the cere-

* Each nerve consists of a bundle of filaments. Whether these filaments are solid or hollow, is a question concerning which anatomists and physiologists have long disputed. If we may trust to microscopic examinations, the probability is in favour of the last-mentioned structure. They contain a medullary pulp of a semi-fluid consistence.

† Francis Glisson, one of the earliest members of the Royal Society, and one of the best anatomists which this country has to boast of, was born at Rampisham in Dorsetshire in 1597. He studied at Cambridge, and was afterwards regius professor of physic there, for a long term of years; till at length he removed to London, where he read lectures on anatomy before the college. During the civil commotions of those days he retired to Colchester, but finally returned to London, where he died in 1677, aged 80. Among other treatises he wrote the following, De Rachitide 1650, (wherein he gives an accurate account of the rickets, a disease at that time new); De Hepate 1654; de Ventriculo et Intestinis 1677. In his treatise on the liver (the best of his anatomical works) he gives the figure of a tube which he used for injecting the vessels.

bellum, for this end, that they may carry down the juice separated in the very glands, there wanting no sanguineous vessels, by which both sufficient matter may be furnished, and the residue of the percolated juice carried away again.

Concerning the kidneys, he first relates what has been taught of them hitherto; and then delivers both his own observations about them, by a long use of the microscope, and his deductions from them. He affirms, that he has always observed the kidneys to be also a congeries of small glands, by injecting through the emulgent artery a black liquor, mixed with spirit of wine, and by cutting the kidneys longways, and then finding, betwixt the urinous vessels and their interstices, very many of such glands which like little apples are appendant to the sanguineous vessels, turgid with that black liquor. He adds, that, after many trials, he at last found also a connexion betwixt those glands and the vessels of urine. As to the pelvis, he considers that nothing but an expansion of the ureter, as consisting of the same membrane and nervous fibres with the ureter.

In the treatise on the spleen, having premised, as before in the other parts, what has been hitherto published about it, he subjoins what himself has further observed thereon: viz. That the whole body of the spleen, however it may seem to be a substance made up of concreted blood, yet is indeed a contexture of membranes, formed and distinguished into little folds and cells; clearly to be seen by syringing air into it by the ramus splenicus, whereby the whole spleen will become so turgid, as to swell into an excessive bigness; which, if upon the exsiccation of the thus swelled part, it be presently cut, its whole mass will be found made up of membranes of the shape of the cells in bee-hives; as he affirms to have clearly seen in the spleen of a sheep and hog, and in that of a man. But then he adds, that through this whole membranous body of the spleen are copiously dispersed clusters of glandules, or, if you will, bladders, very plainly resembling clusters of grapes, appendant on the fibres and the extremities of the arteries and nerves of that body. Coming to discourse of the use of the spleen, after he has examined the various opinions of anatomists concerning it, and declared his dissatisfaction therein, together with the reasons thereof, he does with great modesty as well as ingenuity offer his thoughts about it, viz. That, considering the whole structure of the spleen, it seems to be designed for a new separation and mixture of the juices conveyed into its glands by the arteries and nerves, and then collected in the cells; whereby, and by its stay there, the blood receives such a further change, and is so much more exalted, that being conveyed by the splenetic branch into the neighbouring liver and there refermented, it acquires a disposition for an easy separation of the gall there.

II. Ephemerides Mediceorum Syderum, ex Hypothesibus et Tabulis Joh. Dom. Cassini. Bononiæ, 1668, in thin fol.

What Galilæo Galilæi undertook, after he had discovered the satellites of Jupiter, of giving an easy and sure way to know the longitudes by a careful observation of those stars; Signior Cassini seems to have now performed more fully than others, by composing certain tables, after 15 years observations made with exactness of the motion of the said satellites. These tables are contained in this book; and for verifying them, he has added the Ephemerides of those stars for the year lately elapsed, viz. An. 1668.

END OF VOLUME THIRD OF THE ORIGINAL.

Description of an Instrument invented many Years ago by Dr. CHRISTOPHER *WREN, for drawing the Outlines of any Object in Perspective. N° 45, p.* 898. *(Vol. IV.)*

See fig. 1, pl. 9, wherein A is a small sight with a short arm B, which may be turned round about, and moved up and down the small cylinder C D, which is screwed into the piece E D, at D, this piece E D moving round the centre E; by which means the sight may be removed either towards R or F.—E F is a ruler fastened on to the two rulers G G, which rulers serve both to keep the square frame S S S S perpendicular, and by their sliding through the square holes T T, they serve to stay the sight, either farther from or nearer to the said frame; on which frame is stuck on with a little wax the paper O O O O, whereon the picture is to be drawn by the pen I. This pen I is, by a small brass handle V, so fixed to the ruler H H, that the point I may be kept very firm, so as always to touch the paper.—H H is a ruler, that is always moved horizontally or parallel to itself, by means of the small strings aaaabbbb, at the end of which is stuck a small pin, whose head P is the sight, which is to be moved up and down on the outlines of any object.—The contrivance of the strings is this: The two strings aaa, bbb, are exactly of an equal length: Two ends of them are fastened into a small leaden weight Q Q, which is moved in a socket on the backside of the frame, and serves exactly to counterpoise the ruler H H, being of equal weight with it. The other two ends of them are fastened to two small pins H, H, after they have been rolled about the small pulleys N, M M, L L, K K; by means of which pulleys, if the pen I be taken hold of, and moved up and down the paper, the strings moving very easily, the ruler will always remain in a horizontal position.

The manner of using it is this: Set the instrument on a table, and fix the sight A at any height above the table, and at any distance from the frame S S S S you please. Then looking through the sight A, and holding the pen I in your hand, move the head of the pin P up and down the outlines of the object, and the point I will describe, on the paper OOOO, the shape of the object so traced.

An Observation of Saturn, made at Paris. By M. Huygens, and M. Picard.† N° 45, p. 900.*

On the 17th of August, 1668, at 11½ hr. at night, these observers, with a telescope of 21 feet, saw the planet Saturn, as represented fig. 2, pl. 9, the

* Christian Huygens, a celebrated Dutch astronomer and mathematician, was born at the Hague, of a noble family, in 1629. When very young he discovered an extraordinary genius for the sciences, and soon made a rapid progress in the mathematics, under the celebrated Schooten, professor at Leyden. As early as 1651 he gave a good specimen of his abilities in a book entitled Theoremata de Quadratura Hyperbolæ, Ellipsis, et Circuli, ex dato portionum gravitatis centro. In 1658 he published his Horologium Oscillatorium, sive de Motu Pendulorum; containing great improvements in horology, and rendering all the vibrations of a pendulum equal, by means of the cycloid. He also proposed and contrived clocks for finding the longitude, and gave instructions for the use and management of them. In 1659 came out his Systema Saturninum, explaining his discovery of the ring of Saturn, and that of one of his satellites, which he was enabled to discover by the great perfection he had given to telescopes. He wrote divers other works, exhibiting several ingenious inventions; such as, improvements in the air-pump, the art of polishing telescopic glasses, the laws of the collision of elastic bodies, concerning the honour of which discovery he had a dispute with Dr. Wallis and Dr. Wren, who had both made the same discovery about that time. In 1661 he visited England, where he became a Fellow of the Royal Society. He made several journeys into France, and at length settled entirely in that country, with a pension from the minister Colbert, and was admitted a member of the Academy of Sciences. There he resided from 1666 to 1681, when through ill health he retired into his own country, where he died in 1695, in the 67th year of his age, while his Cosmotheoros, or Treatise concerning the Plurality of Worlds, was printing. In 1703 appeared his Opuscula Posthuma, in 1 vol. 4to. But most of his works are now found in two collections, each of 2 vols. in 4to. viz. the Opera Varia in 1724, and the Opera Reliqua in 1728.

Huygens, like most true philosophers, loved retirement, and a quiet contemplative life; but he was quite free from that melancholy disposition which is often contracted in solitude. He was possessed of a fine genius; endued with great application; and manifested one of the purest tastes in mathematics of any man since the days of Archimedes.

† John Picard, a skilful astronomer and mathematician of France, was prior of Rillé in Anjou. Coming to Paris, his talents and skill in those sciences secured him a favourable reception there, and he was admitted of the Academy of Sciences in 1666, in the capacity of astronomer; and the same year, with M. Auzout, he published a new micrometer. In 1671 he was sent, by order of the king, to the castle of Uraniburg, built by Tycho Brahe, in Denmark, to make astronomical observations there; from whence, on his return, he brought the originals of those made by Tycho, which are the more valuable as they differ in several places from the printed observations, and contain a book more than had been published. Picard made several other important discoveries in astronomy; he was one of the first who applied the telescope to astronomical quadrants; he first instituted the work called Connoissance des Temps, which he conducted from 1679 to 1683 inclusively; he first observed the light in the vacuum of the barometer, or the mercurial phosphorus; he also first of any measured

middle of the globe manifestly appearing both above and below beyond the oval of his anses; which was hardly discernible the last year. The inclination of the great diameter of the oval to the equator was measured several ways, and was found of about 9 degrees, although at that time it should be only of 4 degrees, according to what M. Huygens has affirmed in his system of Saturn, viz. that the plane of the ring which environs the globe of this planet is inclined to the plane of the ecliptic but 23° 30′. But from this, and other more exact observations, M. Huygens finds that, instead of 23° 30′, the angle of the planes of the ring and of the ecliptic must be about 31°; and that not only the shape which Saturn has at present, but also all those that have been noted since the true ones were observed, do perfectly agree with the hypothesis of the ring; and particularly that of July 1664, which was made and published by Sig. Campani, wherein the great diameter is double the less. See fig. 3. pl. 9.

Extract of M. de la Quintiny's Letter, concerning his Way of ordering Melons; now communicated for the satisfaction of several curious Melonists in England. N° 45, p. 901.

Observations now of no manner of use.

An Account of two Books. N° 45, p. 903.

I. Renati Franc. Slusii* Mesolabum. Seu duæ mediæ Proportionales inter extremas datas per Circulum et per Infinitas Hyperbolas vel Ellipses, et per quamlibet exhibitæ. Ac Problematum omnium Solidorum effectio per easdem

the degrees of the French meridian, which have since been carried on both to greater extent and with more accuracy, by the Cassinis, also by Mechain and Delambre; and he died in 1682 or 1683. His works are chiefly as follow: 1. A Treatise of Levelling; 2. Practical Dialling, by calculation; 3. Fragments of Dioptrics; 4. Experiments on Running Water; 5. Of Measurements; 6. Mensuration of Fluids and Solids; 7. Abridgement of the Measure of the Earth; 8. Journey to Uraniburg, or Astronomical Observations made in Denmark; 9. Astronomical Observations made in different parts of France; 10. The Connoissance des Temps, from 1679 to 1683. All these, and some other tracts, are to be found in the 6th and 7th volumes of the Memoirs of the Academy of Sciences.

* René Francis Walter Slusius, or Sluse, was born in 1622 at Vise, in the county of Liege, where he enjoyed honours and preferment. He became abbé of Amas and canon of Liege; and acquired great celebrity by his knowledge in mathematics and physics. The Royal Society elected him one of their members, and inserted several of his compositions in the volumes of their Transactions: viz. Method of drawing Tangents to all Geometrical Curves, in vol. 7; Demonstration of the same, in vol. 8; and a paper on the Optic Angle of Alhazen, also in vol. 8. There were also published some learned letters of his, besides the Mesolabium et Problemata Solida, above described. This learned and ingenious man died at Liege in 1685, at 63 years of age.

Curvas. Accessit pars altera de Analysi, et Miscellanea. Leodii Eburonum 1668, in thin 4to.

This problem is declared to be the same with that in the geometry of the famous Descartes, viz. That ancient problem of finding two means, or of doubling the cube, which troubled all Greece. The solution of which problem in geometry may be compared to that of finding the cube root of any number proposed in arithmetic: For in arithmetic, the first of two continual proportionals between an unit and any number proposed, is the cube root of that number; and the unit in arithmetic is represented by a line in geometry, which is one of the extremes. Concerning this problem, the author declares himself to be none of those that search for that which cannot be found, to wit, to perform it by right lines and a circle. The author observes, that amongst those that solve this problem by the conic sections, very few have done it by aid of a circle and an hyperbola or parabola; by a circle and ellipsis none, that he could observe to have been published. But that he has found out not only one, but infinite such effections, and that not in one method but many; following the guidance of which methods, by the like felicity, he has constructed all solid problems infinite ways, by a circle and an ellipsis or hyperbola.—1. His general methods for finding two means, by a circle and either an hyperbola or ellipsis, are laid down in Prop. 1, 2, 16, and in this prop. 16, he shows how to do it with any ellipsis and a circle.—2. Particular effections for finding out one or both of the means, and doubling the cube in prop. 3 to 6.—3. And though all cubic equations may be solved, either by the finding of two means, or the trisection of an angle, yet he shows the extent of his method, in finding out other infinite ways for the doing thereof, from prop. 7 to 12.—4. The trisection of an angle by a circle and hyperbola, prop. 13, and by a parabola instead thereof, prop. 15. And the finding of two means by a circle and parabola, prop. 14.

In the second part of his book De Analysi, the author first gives the analysis or algebra, whereby all his general methods of finding two means were invented. And afterwards, for the advancement of geometry, gives the analysis that relates to his particular methods. After that he comes to show how the effections or delineations for cubic equations were invented; and then how those constructions for the trisection of an angle were found out.—Lastly, he comes to treat of general constructions for the resolving of all solid problems, without reduction of the equations proposed; and shows a general construction for all cubic and biquadratic equations, by means of a circle and a parabola, letting ordinates fall from the points of intersection on some diameter of the parabola, which is always parallel to the axis, whereas Descartes, letting those ordinates always fall upon the axis, was forced to prepare and alter the equations by taking away the second term, that the sum of the negative roots might be equal to the sum of the affirmative ones, as his constructions always require.

We come next to speak of the last part of the book, to wit, his miscellany, and because it falls in here somewhat properly, we therefore first mention his 4th chap. viz. De Maximis et Minimis, from which he derives this proposition: If any magnitude, or number, as the whole, be divided into such parts, that are to each other as a number to a number, the product of those powers of the parts that are of the same degree as the parts themselves denominate, is the greatest of all products of the like powers of the parts of the same magnitude when otherwise divided.

Concerning the rest of the miscellanies; our author, in chap. 1, treats of the infinite spirals, and of the measure of the spaces comprehended by them and the radius of the circle. Concerning which he observes, that Archimedes squared that spiral which was made by an equal motion both in the radius and circumference of the circle: that Stephano Angeli has done the like, when the motion in the radius is equal, but in the circumference according to any degree of acceleration; which gave him occasion to render this doctrine easy and universal by reducing it to one analysis, when the motion is accelerated according to any degree either in the radius or circumference. He applies this doctrine, in chap. 3, to another sort of infinite spirals; and in chap. 2, he treats of the measure of spaces contained by the curves and right lines, also of their centres of gravity, applying the former analysis or algebraic calculation thereto. Chap. 5 treats of the primary conchoid of Nicomedes: which point he determines by the intersection of a parabola, whose axis is situated in the same line with that of the conchoid; or by a cubic parabola, whose axis is parallel to the base of the conchoid, and vertex the same with the pole of the conchoid; and hence invents innumerable other conchoids of like properties, and finds the curve passing through those points of flexure that are made by infinite conchoids described about the same common pole and base, which in the common conchoids he finds to be the perimeter of the cubic parabola here mentioned: But in his own new conchoids, it is the ancient cissoid, extended beyond a quadrant and running asymptotic: And he finds also the round solids made by the rotation of these infinite curves, and of the cissoid line about their base lines or asymptotes, equal to finite solids.

Chap. 6. The author considering that Vincenzo Viviani, in his book De Maximis et Minimis, found that if there were innumerable parabolas described, having the same axis and vertex common, if from any point in that axis, the shortest lines were drawn to those parabolas, all those points of incidence would fall in an ellipsis; and the author's analysis taught him, that the proposition was universal wherever the point be assigned, from which the shortest lines are to be drawn; which he has extended and applied to those infinite sorts of other parabolas.—Chap. 7 treats of the dimension of figures from the given centre of gravity. This he says is accurately handled by the learned already; yet he adds some easy methods of his own, which may be applied to good use; for, in any curve, if there be ordinates enough given, standing erect at an equal parallel distance, you may approach the area, and if by aid thereof you find the centre of gravity, then do you obtain the measure, either of the round solid or spindle made by the rotation of the given figure, or of hoofs raised upon it as a base.

Chap. 8. The author shows an easy way of finding the centre of gravity of an hyperbolical conoid, and that in order to the resolution of this problem: To find the locus in which are all the centres of hyperbolic conoids, which are generated from hyperbolas cut in a given right cone, and having their axes parallel to that of the cone; which he finds to be an hyperbola.—Chap. 9 treats of the centre of gravity of the lunula of Hippocrates of Chios; and shows that if Hippocrates had given that, as he did the quadrature of the lunula, he had squared the circle.—Chap. 10 treats of arithmetical problems; wherein he asserts that Diophantus was wont to solve arithmetical questions with great subtilty, but uses numbers only; whereas the same may often be more easily and universally solved by algebra: of which he gives some examples.

II. Tractatus de Corde; item de Motu et Colore Sanguinis, A. Richardo Lower,* M.D. F.R.S.

The learned author of this treatise, considering how important it was for the attaining of a full knowledge of the nature and qualities of the blood, to investigate, not only its circular motion, the origin and celerity of that motion, and the various changes thereof, together with their causes; but also to make an estimate of the quantity of that liquor emitted at every pulsation; thought it very well worth while to give, from his own excellent observations, a clear and particular account of the whole of this subject. And forasmuch as he conceives that the motion of the blood depends on that of the heart, he begins with a discourse concerning the situation and structure of the heart, to show how exactly these two are calculated for its motion, and how well adapted to distribute the blood into the parts of the whole body.

In chap. 1 he considers the diversity of the situation of the heart in different animals, and the reason thereof; proceeding to discourse of the pericardium and its use, together with the origin and use of the serum therein; and why in man only that case of the heart grows to the midriff, and what makes it to do

* A biographical notice of this celebrated anatomist has been given at p. 197 of this volume.

so; as also why the cone in a human heart bends much more to the left side than in brutes: Then showing that arteries have their rise from the heart, but veins terminate in it, and how and by what vessels the heart is nourished by the alimentary juice: treating also of the vessels of the heart, its nerves, and the various influx of the animal spirits through the nerves into the heart, according to the various shapes of animals, together with the cause thereof: Proving further, that the substance of the heart is perfectly muscular, and in perfection surpassing all other muscles of the body (where he expatiates into remarkable observations concerning muscles in general;) then descending to a minute explication of the parts of the heart, and there particularly showing the mechanical contrivance of the heart for its systole and diastole, together with an accurate description of the foramen ovale and its use in the fœtus, and the closing up of the same in animals born.

In chap. 2 he treats of the motion and office of the heart; where, as he admits not of any ferment or ebullition of the blood in the heart (which he affirms would be an obstacle to its systole, as it is needless to the diastole,) so he asserts that the motion of the heart depends not from such an ebullition (which he proves by experiments, and vindicates from objections;) but that the genuine and immediate instruments of the heart's motion are its fibres, nerves, and spirits flowing through them, the action of the heart being altogether conformable to that of other muscles: where he takes occasion to make it out, that the motion of muscles is not caused by their being inflated, nor by any explosion of the spirits passing through them, but after the manner as two men, taking one another by their hands, draw themselves close together into mutual embraces: Whence he goes on to show, that the whole motion of the heart consists indeed in the systole, that of the diastole being only a motion of restitution. Further, that there is a necessary commerce betwixt the heart and brain (the cause of all sense and motion:) but that both ultimately depend upon the stomach, as the constant purveyor and furnisher of matter for blood and spirits.

In chap. 3 he teaches with what celerity all the blood passes through the heart, and what difference there is between the venous blood and the arterial. As to the former, he calculates, that all the blood passes through the body 13 times (not six as stated in the book itself by an error of the press) in one hour. And concerning the latter, he is of opinion, that the purple and florid colour of the blood in the arteries proceeds not from its accension in the heart (if there be any such thing) but is derived altogether from the lungs, and the admixture of the air with the blood there: which he proves by considerable experiments; refuting withal the opinion of those that attribute it to the comminution of the blood in the lungs.

In chap. 4 he gives an account of the rise, progress and use of the invention of transfusing blood out of one animal into another: though in the history of this particular he commits (I know not by what oversight) a mistake, in relating that M. Denys (called by him Dionysius) arrogates to himself that invention, whereas he only tells us that some of his nation do so. Besides which we must point out another mistake in this part of the book, viz. that the author taking occasion to speak of the Philos. Transactions, calls them the transactions of the Society; which certainly he would not have done if he had only taken notice of what is said in N° 11 of the same.

In chap. 5 he treats of the chyle, and its change into blood; where he observes, that nothing passes from the spleen through the vas breve into the stomach; but that the ferment of the stomach proceeds immediately from the blood itself: Explaining further, how the separation of the chyle is performed in the intestines, and how the same, to facilitate the more its passage, is diluted and refined by the juice of the pancreas secreted into the duodenum: Rendering also the cause why all the glands in the abdomen, and in all the lower parts of the body, deposit their lymph or juice into the common great receptacle of the chyle, and why that receptacle is placed between the tendons of the diaphragm; as also why those channels which convey the chyle into the subclavian vein are double. To which he adds, that all the chyle is by the ductus thoracicus alone transmitted into the blood and heart, which he proves by several considerable experiments, with some reflections on the experiment alleged by Bils to prove the contrary. All which he concludes by showing the degrees and ways of change, whereby the chyle is at last converted into blood; and how it serves for the nourishment of the several parts of the body.

The whole receives a singular elucidation and ornament by the accurate figures annexed.

Additional Observations on Vegetables. By Dr. Tonge. N° 46, p. 913.

For completing the experiment on sap, and to discover whether it ascends more or less in the pricked circles of the body, than in those between the body and bark; let the tree, exhausted of all its sap the day before, be first pierced with an auger, only through the bark, and the quantity of sap it yields exactly measured and weighed: then at the same time let another hole be bored into the body of the tree about $1\frac{1}{2}$ inch deep, and so round on every side of the same tree, and of others of the same sort, (all exhausted of their sap the day before) some deeper and some shallower, with a good large auger; and one quite through sloping. From this experiment, after divers and various trials, may be found the difference of the sap rising on the north and south, in sun and shade, and so likewise from that which comes from the bark, and that which ascends in the inner part of the tree. The weight also may be compared of that which issues from the bark with that which issues from the body. The heart-sap may also be drawn apart, by boring a smaller auger hole in the middle of a larger, and fitting it with a long pipe adjusted into the inner orifice.

I am informed by a curious and intelligent person, that the corruption of the timber depends not so much on the time of the year and the ascent, or the plenty or scarcity of sap, as on the state of the moon or wind. And that timber trees felled when the wind is in the west, especially in the old moon, will keep them from being worm-eaten; and on the contrary, that when cut down in an east wind, the worm will seize on them, at any age of the moon. To prevent which corruption, it is advised that such timber be forthwith thrown into water.

Mr. E. Jay, an ingenious and expert planter, supposes that the fittest time to inoculate is presently after midsummer, because he says the sap descends; but I say, because it is then most plentiful and begins to jelly.

To make a barren tree bear again; nourish it with dung in trenches, and pare and renew the extremities of its longest roots, and cut off the outermost and shortest nearest the body. Hence it may seem that ploughing helps fruit-trees.

Cross hackings promote fruitfulness, and cure the phyllomania or luxuriancy of leaves; the reason of which seems to be, that outward circles and bark feed the wood, and the inner only reach out to the outermost sprigs of the last year, to which the fruit hangs. For some trees bear only on this year's shoot, and some only on that of the last, possibly some only on the third year's shoot; and cease bearing when they shoot no new sprigs. Seasonable baring the roots, called ablaqueation, has probably the same effect; because it hinders the nourishment, especially of the outward coats, and of bark, leaves and suckers: but as some suckers or shoots lately sprung in outward coats seem to rob the fruit of the risen juice, so later roots from the outward parts of the main roots rob them also of their first nourishment in the earth; they ought to be pruned, as well as all suckers and not bearing branches and sprigs, every year.

To preserve sap in the best condition for brewing; what is drawn first must be constantly exposed to the sun in glasses, or other fit vessels, till the rest be obtained and ready; otherwise it will soon contract an acidity. Then put into it so much very thin cut and hard toasted, but no ways burned, rye-bread, as will serve to ferment it; and when it works take out the bread, and bottle the liquor, stopping it up with waxed corks. If you bake sage, or any other medicinal herbs in such thin rye paste, till they be very dry, you may expect a very wholesome drink. If you put a few cloves in every glass, into which the sap runs from the tree, it will keep twelve months.

Spirit of wine ferments the juice of some berries, and possibly may not only preserve, but heighten the virtue of saps; a little being poured on the top of them in the bottles, or some other oily spirit.

A certain lady ferments it with rye-toast, not put in, but only hung over it, in such quantity and at such distance, as may give some light warmth, motion, and alteration to the surface of the liquor.

I fermented some with ale-barm, [yest] which converted my delicate birch-juice, kept in bottles, into poor small beer.

Honey will not mix with cyder, though boiled therein to make mead; but after a while the cyder lets fall the honey, and becomes simple cyder again.

Some affirm, that a decoction of the tops and leaves of birch in the sap will preserve it from souring a whole year; and that any sort of dried aromatic herbs, as sage, &c. boiled in beer, will keep it as well as hops, ling (heath), broom, or wormwood. And some have used bay-leaves in their beer and ale.

Fine light French manchet (bread), toasted, may possibly be also good for our saps.

The Connection of certain Parts of a Tree, with those of the Fruit: By Dr. JOHN BEALE. *N° 46, p.* 919.

I had an excellent summer apple, containing abundance of very pleasant juice. It was of that kind which never grows large. The body by the burthen of the fruit always wreathed towards the ground; the branches all curled, and full of knots at every turning; and these branches apt to grow, if a good knot be set in the ground, as soon as it is cut off, especially about Candlemas. This tree was hollow, and very nearly all the timber extremely rotten, from the top of the stem to the root; and every sprig, however small, appeared cork-coloured and rotten at the heart of the timber; and so it was generally all over the roots. Yet the tree bore abundantly, with alternative rests, every second or third year. The fruit had scarce any core; the kernels were very small, thin and empty, yet the branches from the knots grew well enough to replenish a nursery. This seems to indicate the correspondence between the pithy part, heart or timber, and the seeds. And to confirm this; a young tree grew like a sucker from the only sound root of the apple tree. This grew straighter than others of the same kind usually do; of which I conceive the cause to be this: suckers are commonly barren a pretty long time; and this continued barren till the stem was strong enough to bear the fruit which loaded the branches. But that which makes to our purpose, is this; all the fruit of this young tree had full and sound kernels; and though it was the same fruit growing from the root of the same tree, yet it seemed not quite so tender, delicious, and juicy as the fruit of the old tree; nor yet was the tree so fruitful. The sap in the old tree was less diverted, it seems, to sustain the life of the timber, which was now consumed, and thereby was wholly appropriated for the leaf, blossom, and the pulp of the fruit.

Perforated berbery roots bore berries that had no stones at all: and in hollowed apple-trees the kernels will be very thin, and empty skins, and uncapable of growth.

Some trees are less fruitful, or altogether barren, by the excessive growth and firmness of the timber; and these are recovered by cross deep hackings through the bark. They cleave the roots, and put a stone in the cleft, that it may not close again too hastily. If this violence be not done both to the stem and roots, the remedy may fail. We see also, that vines are less fruitful, when they are permitted to run out into many woody branches.

To show also the affinity between the sap of the bark and the pulp of the fruit; in summer time I made rests for water on the body of Kentish codlin-trees, and caused water to be frequently poured into those cavities. The effect was this, the apples grew to an extraordinary size, but were very insipid, and many of them had parts in appearance much like the pulp of lemons: some I suffered to hang on the tree as long as they would, and those became full of spots of the colour of cork, or like the rottenness of an apple.

Extract of a Letter of M. de la Quintinie, giving some further Directions and Observations about Melons. N° 46, p. 923.

Of no use in the present times.

The Laws of Motion on the Collision of Bodies. By M. Huygens, Translated from the Latin. N° 46, p. 925.

Some members of the Royal Society, being very earnest that that important subject, the laws of motion, which had been several times started among them, but often interrupted, and never sufficiently discussed, might at length be brought to a close examination; that illustrious body therefore resolved, that such of their members as applied themselves to that subject should be desired to produce their thoughts and discoveries on that head, and likewise to bring into one view what those excellent men, Galileo, Descartes, Honoratus Fabri, Joachimus Jungius, Borelli, and others had invented; that by this means, on comparing their several sentiments on this subject, that theory might be adopted which should be found to agree best with observation and experiment, carefully made and often repeated.

Upon this M. Huygens, Dr. Wallis, and Dr. Christopher Wren, members of the Society, were induced to finish and complete their several hypotheses and laws of motion, in forming of which, they had spent some time: And in a few weeks after these excellent persons transmitted to the Royal Society elegant abstracts of their theories, desiring the sentiments of that illustrious body upon them. Dr. Wallis was the first, who in a letter dated Nov. 15, and read before the Society the 29th, 1668, had communicated his principles of estimating the motion of bodies. Dr. Wren in a little time after, viz. 17th of Dec. of the same year, imparted to the Society, his law of nature on the collision of bodies; and the Society ordered, after first obtaining the permission of the authors to publish these discoveries, that they might be more conveniently communicated and more fully discussed. In the mean time, viz. on the 4th of the ensuing January, Mr. Oldenburg had a letter from M. Huygens dated Jan. 5. N. S. containing the first four rules concerning the motion of bodies after mutual impulse, together with their demonstration. And that very day on which Mr. Oldenburg received M. Huygens' letter, he sent in return a copy of Dr. Wren's theory without opening M. Huygens', which, on account of its bulk, and that gentleman's former promises, he suspected to contain something on the same matter, till he should have an opportunity of seeing the Honourable Lord Viscount Brounker, president of the Society. After which, and on comparing the rules of both, the Society found a surprising coincidence, which made them more inclinable to a publication of them; and nothing more seemed to be wanting but M. Huygens' consent, without which it was not thought proper to publish his discoveries, especially as they were not then entire. In the mean time it was ordered that they should be laid up in the public archives of the Royal Society, and at the same time that thanks be returned to the author for his frankness in communicating them; and on the 4th of February following he was earnestly intreated to publish his theory, either at Paris in the Journal des Scavans, or at London in the Philosophical Transactions; and in a little time after, Mr. Oldenburg had a second letter from M. Huygens, approving Dr. Wren's theory, without making any mention of the publication of his own, either at Paris or London. Hence it appears that M. Huygens was wanting to himself in delaying that publication, thus giving occasion to Dr. Wren, who by the sagacity of his own genius had made the very same discoveries, to claim a just title to some share of the glory of the discovery; since it is plain, that neither of them had known any thing of each other's theory, before they were laid before the Society, but that both of them by their own ingenuity produced beautiful originals.

It is indeed true that M. Huygens, when he was at London some years ago, resolved some of those cases on motion that were proposed to him; by which it plainly appears that he was then master of those rules by which he gave those solutions. But he himself must own that he did not communicate the least hint of his theory to any of the English, though he was often solicited to it, till very lately.

After having said so much in favour of truth and justice, we shall now deliver M. Huygens's rules.

Rules concerning the Motion of Bodies after mutual Impulse.

1. If a hard* body strike against another and equal hard body, at rest; after impact the former will rest, and the latter will acquire a velocity equal to that of the moving body.

2. But if the second body be likewise in motion, and moving in the same right line, after contact, the bodies will move with interchanged velocities.

3. Any body ever so great may be moved by a body however small, that strikes it with any velocity whatsoever.

4. The general rule for determining the motion of hard bodies arising from their direct impulse is this, viz.

Let A and B, fig. 4, pl. 9, be two bodies, of which A is moved with the velocity AD; and let B meet it, or let it move the same way with the velocity BD, or lastly, let it be at rest, that is, in this case, let the point D coincide with B. Dividing the line AB in C, the centre of gravity of the bodies A and B, let CE be taken equal to CD. I say, EA will be the velocity of the body A after the stroke; and EB of the body B; and each will move in that way, which is shown by the order of the points EA, EB. But if E coincide either with A or B, the body A or B will remain at rest.

5. The quantity of motion of two bodies may be either increased or diminished by their shock; but the same quantity, towards the same part, remains, after subtracting the quantity of the contrary motion.

6. The sum of the products arising from multiplying the mass of each hard body into the square of its velocity, is the same both before and after the stroke.

7. A hard body at rest will receive a greater quantity of motion from another hard body, either greater or less than itself, by the interposition of any third

* The term *hard* is not here used in its more modern acceptation, but it means *elastic;* the rules here delivered, being those that relate to the collision of elastic bodies.

body, of a mean quantity, than if it was immediately struck by the body itself; and if the interposing body be a mean proportional between the other two, its action upon the quiescent body will be the greatest of all.

In all these cases, the author, as he himself suggests, considers the bodies as homogeneous, or of the same matter, or that their mass may be estimated by their weight. He moreover subjoins, that he has observed a surprising law of nature, which he can demonstrate in spherical bodies, and which seems to hold universally in all others, either hard or soft, impinging either directly or obliquely, viz. that the common centre of gravity of two or three, or more bodies, always moves uniformly the same way in a straight line, both before and after the stroke.

On the Resolution of Equations in Numbers. By Mr. JOHN COLLINS.* *N° 46, p. 929.*

It has been observed by several persons of this country, in any equation, however affected, that if a series or rank of roots be assumed in arithmetical

* In addition to the biographical account of this author at p. 207, it is further to be observed, that he was of great benefit to the sciences in general; keeping up a constant correspondence with many of the most learned men of his time, both at home and abroad, and promoting the publication of several valuable works, which without his encouragement would never have been seen by the public; particularly Dr. Barrow's optical and geometrical lectures, also the doctor's abridgement of the works of Archimedes, Apollonius, and Theodosius; likewise Brounker's translation of Rhonius's algebra, with Dr. Pell's additions, &c. which were procured by his frequent solicitations. Some time after his death, among his papers, were found a multitude of manuscripts, on mathematical subjects, of Briggs, Oughtred, Barrow, Newton, Leibnitz, Pell, and many others. From whose letters, and those of other celebrated mathematicians, it appears that Mr. Collins spared neither pains nor cost to procure what tended to promote real science. Also many of the discoveries in physical knowledge owe their chief improvement to him; for while he excited some to disclose every new and useful invention, he employed others in improving them. Sometimes he was peculiarly useful, by showing where the defect lay in any branch of science, pointing out the difficulties attending the inquiry; at other times explaining their advantages, and keeping up a spirit and energy for improvement. In short, Mr. Collins was like the register of all the new acquisitions made in the mathematical sciences; the magazine to which the curious had frequent recourse; which acquired him the appellation of the English Mersenne. And had not some of his correspondents obliged him to conceal their communications, there could have been no dispute about the priority of the invention of a method of analysis, the honour of which doubtless belonged to Newton; as appears from the papers printed in Collins's Commercium Epistolicum; a work which was made out from the letters left in possession of this author.

Mr. Collins wrote and published himself a variety of useful works: as, An Introduction to Merchants' Accounts, &c. 1652; The Sector on a Quadrant, 1658; Geometrical Dialling, 1659; The Mariner's Plain Scale new plained, 1659; also several ingenious papers printed in the Phil. Trans. besides some useful commercial tracts, highly acceptable to the public.

progression, the resolvends as to their first, second or third, &c. differences, imitate the laws of the pure powers of an arithmetical progression, of the same degree as the highest power or first term of the equation. Ex. Gra. In this equation, $aaa - 3\,aa + 4\,a = N$,

If a be = {10, 9, 8, 7, 6} Then N, or the absolute numbers, or resolvends, will be found to be

a	N.	1 dif.	2 dif.	3 dif.
10	740			
		218		
9	522		48	
		170		6
8	352		42	
		128		6
7	224		36	
		92		
6	132			

To wit, the 3d differences of those absolutes are equal, as in the cubes of an arithmetical progression.

To find what relation those differences have to the coefficients of the equation, it is best to begin from an unit. Now in any arithmetical progression if you multiply numbers by pairs you shall create a rank of numbers whose 2d differences are equal; and if by ternaries, then the 3d differences of those products shall be equal. And how to find the greatest product of an arithmetical progression of any number of terms, having any common difference assigned, contained in any number proposed, is shown by Pascal in his Triangle Arithmetique, where he applies it to the extraction of the roots of simple powers. It is manifest how this rank may be easily carried on by addition, till you have a resolvend either equal or greater or less than that proposed.

When you have a greater and less, you may interpolate as many more terms in the arithmetical progression as you please, viz. Subdivide the common difference and render it less; then renew and find the resolvends, which are easily obtained by the powers and their coefficients, which are supposed known, and may be readily raised from a table of squares and cubes, &c. By this means you may obtain divers figures of the root; and then the general method of Vieta and Harriot proceeds more easily; and after any figure is placed in the root by

Mr. Collins's birth and early prospects, like those of many other great men, were but low and humble. He was born at Wood Eaton near Oxford in 1624, and at 16 years of age was put apprentice to a bookseller in this city; but appearing to have a remarkable turn for the mechanical and mathematical sciences, he was taken under the protection of a Mr. Marr, a person who drew several curious dials, which were placed in different positions in the king's gardens; and under him Mr. Collins made no small progress in mathematics. In the course of the civil wars he went to sea for seven years, but still prosecuted his favourite study; and on his return he assumed the profession of an accountant and civil engineer, giving his advice and directions in nice and critical cases, relating to matters of commerce, of accounts, and of engineering, till the time of his death, which happened in the year 1683, in the 59th year of his age.

means of the subsequent dividend and divisor, it may certainly be known whether the figure so assumed be too great or too little.

After one root is obtained, the methods of Hudden and others will depress the equations, so as to obtain more, and consequently all of them.

The author of this narrative considers that the conic sections may be projected from lesser circles of the sphere, and thence easily described by points; and that by their intersections some spheric problem is determined. Accordingly he found that this following problem, according to the various situation of the eye, and of the projecting plain, would take in all cases.

The distances of an unknown star being given from two stars of known declination and right ascension; the declination and right ascension of the unknown star is required.

And he observes that, admitting the mechanism of dividing the periphery of a circle into any number of equal parts, or, which is equivalent, the use of a line of cords, that this problem, wherever the eye be placed, may be resolved by plain geometry, and yet the eye be so situated as to determine it by the intersections of the conic sections; consequently these points of intersection, the species and position of the figures being given, may be found without describing any more points than those sought; and the lengths of ordinates falling from thence on the axes of either figure be calculated by mixed trigonometry; and hence likewise the roots of all cubic and biquadratic equations be found by trigonometry. For having from the Mesolabe of Sluse the scheme that finds these roots, it will then be required to fit those sections into cones, which have their vertex either in the centre, or an assigned point in the surface of the sphere, to which they relate as projected, and proceed to the resolution of the problem proposed: and how to fit in those sections, see the seven books of Apollonius or of Mydorgius, the third volume of Descartes's Letters, or Leotaudi Geometrica practica, or Andersonii Exercitat. Geometricæ.

As to the problem itself, it is determined on the sphere by the intersections of the two lesser circles of distance, whose poles are the known stars. And this problem has divers geometric ways of solution. As,

1. By plain geometry; supposing a plain to touch the sphere at the north-pole, if the eye be at the south-pole, projecting those circles on the said plain, they are still circles, by reason of the sub-contrary sections of the visual cones, whose centres fall in the sides of the right-lined angle, made by the projected meridians that pass through the known stars; and thus the problem is easily solved.

2. By conic geometry; In one case it may be done, by placing the eye at the centre of the sphere, and projecting as before; viz. when the longer axes of the

figures being produced meet above the vertex. Here the problem is determined by the intersections of two conic sections, of which a circle cannot be one, unless its centre be in the axis of the other figure. And in this second case, these points of intersection fall in the same right line or projected meridian, as they did before, but at a more remote distance from the pole, viz. in the former supposition the solar distance was measured by a right line, that was the double tangent of half the arch; here it is the tangent of the whole arch. Hence it is evident how one projection may produce another, yea infinite others, by altering the scale; and how the lesser circles in the stereographic projection help to describe the conic sections in the gnomonic projection; but to reduce the matter to one common radius, if we suppose two spheres equal, and so placed about the same axis, that the pole of the one shall pass through the centre of the other, and the tangent plane to pass through the said centre or pole, and that a lesser circle has the same position in the one as in the other; then, if the eye be at the south pole of the one, it is at the centre of the other; and any projected meridian drawn from the projected pole, to pass through both the projections of these lesser circles, the distances of the points of intersection are the tangents of the half and the whole arch of the meridian so intersected. But as to the points of intersection, which determine the problem proposed, they may be found without the aid of the former way, from a gnomonic and stereographic method of measuring and setting off the sides and angles of spherical triangles in those projections, which is necessary in what follows.

3. If the problem is to be performed by mixed geometry, as by a circle and either a parabola, hyperbola, or ellipsis, the circle may be conceived to be the sub-contrary section of a cone projected by the eye at the south-pole, and any of the rest of the sections by the eye at the centre of the sphere.

4. If by any of the conic sections however situated, the projecting plain may remain the same, but the eye must be in some other part of the surface of the sphere, and not in the axis.

An Account of Books. N° 46, p. 934.

I. Præludia Botanica Roberti Morison * Scoti Aberdonensis. Londini. 1669, 8vo.

* Robert Morison, one of the most celebrated botanists of the 17th century, was a native of Aberdeen in Scotland, where he was born in the year 1620. In this university he took his degree of master of arts. Having a strong inclination for the study of physic, and more particularly for that of botany, he went to Paris, where he obtained the degree of doctor of physic. His reputation as a botanist induced Gaston Duke of Orleans, an admirer of that study, to give him the direction of the Royal Garden at Blois. After the death of the Duke of Orleans he came into England, in the year

This prelude of this excellent botanist consists of two parts; the first gives us an alphabetical catalogue of all the plants in the royal garden of Blois in France, as the same was enriched by the munificence and encouragement of the most illustrious prince Gaston, late duke of Orleans, with 360 plants, in the space of five years, by the singular care and skill of our author; who in this catalogue has not only given a succinct description of the plants here enumerated, but also by certain marks distinguished the perennial ones from the annual; adding some general observations, collected from the garden above mentioned, very necessary and useful to all that are studious in botany.

The second contains some animadversions not inconsiderable, both on the Pinax of Caspar Bauhinus, showing his mistakes as well in the digesting as in the naming of plants; and on the three volumes of the Universal History of Plants of Johannes Bauhinus.

To which is annexed a dialogue between a fellow of the Royal Society and the author, containing an answer to several queries proposed, where is intimated

1660, and on the restoration of Charles the second, was appointed professor of botany in the University of Oxford, with a salary, as it is said, of 200 *l.* a year from the king. He was also chosen a fellow of the Royal College of Physicians. In 1669 he published his *Præludia Botanica,* and afterwards his *Plantæ Umbelliferæ.* In 1680 he published the second volume of his *Historia Plantarum,* (purposely delaying the first volume, which related to trees and shrubs.) He died before the publication of the third volume, which was completed by the care of the elder Robart, who succeeded him in the management of the Oxford Garden. His death was occasioned by an unfortunate bruise which he received from the pole of a coach, while crossing a street in London, whither he had gone in order to expedite the necessary subscriptions for the continuation of his work. He was in the 63d year of his age when this event took place. It is remarkable that the celebrated Tournefort died in consequence of a singular accident which happened to him in the streets of Paris.

Morison is said to have been of an amiable and estimable character, and of great plainness and simplicity of manners.

His botanical method or system, which was intended for a natural one, is taken from the fruit, but is, in this respect, according to the opinion of an eminent botanical critic, much inferior to that of Cæsalpinus, both in the plan and execution: it is clogged with a multiplicity of characters, and the classes are not sufficiently distinguished from each other: hence it is extremely difficult in practice, and was therefore not adopted by any succeeding writer except Robart, who in 1699 completed the Historia Plantarum, and by an anonymous author whose work appeared in 1720. Imperfect however as is this system, it has furnished many useful hints, which later botanists have not failed to improve; since Ray, Tournefort, and Linnæus have successively been indebted to the prior labours of Morison.

It is remarkable that Morison, during the investigations necessary for the continuation of his Historia Plantarum, imagined that a new plant had been discovered by the younger Robart in the neighbourhood of Oxford, and in consequence announced it in his work, with much satisfaction, as a "*nova et inaudita planta.*" On farther examination, however, it was easily proved that it was no other than the Samolus, (Samolus Valerandi of modern botany) a plant by no means very uncommon in watery situations in many parts of England.

the best general method, taken from nature itself, of digesting all plants, and reducing them to certain classes or heads, according to the difference of their seeds, pods, and flowers.

II. Cl. Salmasii * Præfatio in Librum de Homonymis Hyles Iatricæ: Ejusdem de Plinio Judicium.

This book is an introduction to a large volume, composed by the famous Salmasius, and now in the hands of Messieurs Lantin and De la Mare; the volume gives an account of the many and great mistakes hitherto made in the history of plants with respect to the naming them; in which it has come to pass, that several names being often given to one and the same plant, and *vice versa,* one and the same name to different plants, there has ensued a great and dangerous confusion in that large part of the Materia Medica, highly requiring to be rectified. Now to that work this preface prepares the way, by showing to the studious in botany and medicine, the argument, order, and usefulness of the same, interspersing the causes and origin of those many errors which both ancients and moderns have fallen into upon this subject; as also the negligence of those ancients, the progress of physic among the Romans, and the age of the chief writers on this argument; adding also the author's opinion concerning Pliny, what is to be approved, what to be condemned in him, and how far we are to proceed in the admiration of that writer.

Instructions concerning the Use of Pendulum Watches, for finding the Longitude at Sea. By M. Huygens. N° 47, p. 937.

These instructions were first published by M. Huygens, and afterwards

* Salmasius is much better known as a critic and philologist than as a writer on subjects relative to natural history and medicine. He was born at Semur (not Saumur) in 1596, studied at Paris, became a convert to the Protestant religion, and was professor of polite literature at Leyden, from whence he removed to Stockholm at the invitation of Queen Christina, but soon returned again to Holland. He was employed by Charles II. to write (1649) a defence of his father (Charles I.) and of kingly governments. This brought on Milton's famous reply, 1651. Both these tracts were written in Latin, of the knowledge and use of which, in its highest degree of purity, each party had sufficient reason to boast. But Milton's style was most admired, and this circumstance (according to some biographers) accelerated Salmasius's death, which happened while he was at Spa in 1653. This event however (it is highly probable) was chiefly attributable to the gradual decay of his constitution from excessive application to literary pursuits. This eminent French critic began to exercise his pen at a very early period of life, appearing as an author when no more than 15 years of age. A list of his writings is to be found in almost every work on biography. Although he was not a physician, he published other treatises besides that above-mentioned, on subjects relative to natural history and medicine; viz. Exercitationes Plinianæ in Solinum, 1629, (a large and learned commentary on the writings of the elder Pliny.) Interpretatio Hippocrat. Aphor. 79, Sect. iv. 1640, and De Annis climactericis, 1678. In this last treatise there are many observations on longevity, and in this, as well as in his other works, there is a vast fund of erudition.

altered and enlarged, by two eminent members of the Royal Society, as follows:

1. Those who intend to make use of pendulum watches at sea, must have two of them at least, that, if one of them should by accident or neglect, or long use, happen to fail, there may still remain one for use.

2. They must procure good information of the nature of the inward parts of the watches, the manner of winding them up, and how to set the indexes or hands having the hours, minutes, seconds, &c.

3. The watches on ship-board are to be hung in a close place, where they may be most free from moisture or dust, and out of danger of being disordered by knocking or touching.

4. Before the watches be brought on ship-board, they should be adjusted to keep mean time, the use of them being then most easy. Yet if time or conveniency for this purpose be wanting, they may notwithstanding be used at sea with equal certainty, provided you know how much they go too fast or too slow in 24 hours.

5. To adjust or regulate the watches to keep equal or mean time: take notice that the sun or the earth passes the 12 signs, or makes an entire revolution in the ecliptic, in nearly 365 days 5 hours 49 min. and that those days reckoned from noon to noon are of different lengths. Now between the longest and the shortest of those days, a day may be taken of such a length as 365 such days 5 hours, &c. the same numbers as before, may be equal to that revolution: And this is called the equal or mean day, according to which the watches are to be adjusted; and therefore the hour or minute shown by the watches, though they be perfectly just and equal, must needs differ almost continually from those that are shown by the sun, or are reckoned according to his motion. But this difference is regular, and is otherwise called the equation, a table of which for the year you must be provided with.*

By this table you will always know what o'clock it is by the sun precisely, and consequently whether the watches have been set to the right measure of the mean day or not, using the table, as follows: When you first set your watch by the sun, deduct from the time observed by the sun the equation adjoined to that day of the month in the table, and set the watches to the remaining hours, minutes and seconds, that is, the watches are to be set so much slower than the time of the sun, as is the equation of that day; so that the equation of the

* A general table of the equation of time, for every day in the year, was here inserted in the original; but, being very erroneous in every number, it is omitted, and without supplying its place with others, because every year requires a different set of numbers, for each day of the years.

day, added to the time of the clock, gives the true time by the sun. And when after some days, you desire to know by the watch the time by the sun, add to the time shown by the watch, the equation of that day; and the aggregate will be the time by the sun, if the watch has been well adjusted after the measure of the mean days; for the doing of which, this will be a convenient way: Draw a meridian line on a floor, then hang up two plummets, each by a small thread or wire, directly over that meridian, at about 2 feet from each other. When the middle of the sun, the eye being so placed as to bring both the threads into one line, appears to be in the same line exactly, then immediately set the watch, not precisely to the hour of 12, but by so much less as is the equation of that day by the table. Ex. gr. If it were the 12th of March; the equation of that day being, by the table, 8 min. 3 sec.; these deducted from 12 hours, the remainder will be 11 hours 51 min. 57 sec.; to which hours, minutes and seconds set the index of the watch: Then after some days observe again in the same manner, and likewise note the hour, min. and sec. of the watch; to which add the equation of these days taken out of the table; and if the sum make just 12 hours, the watch is adjusted to the right measure; but if it differ, then divide the minutes and seconds of that difference by the number of the days between the two observations, to get the daily difference. Suppose this second observation to have been made the 20th of March, viz. 8 days after the first, and finding that the middle of the sun being seen in the meridian in the same line with the two threads, the watch points out 11h. 51m. 7sec.; now the equation of the 20th of March, by the table, is 10m. 40sec., which being added to the time shown by the watch, gives 12h. 1m. 47sec. If this had been just 12 hours, the watch would have been well adjusted, but being 1 min. 47 sec. more than 12, it has gone so much too fast in 8 days: and these 1 min. 47 sec. that is 107 sec. being divided by 8, there comes $13\frac{3}{8}$ sec. for the difference on every 24 hours; which difference being known, if you want time, or have no mind to take the pains to adjust the watch, note only the daily difference, and regulate yourself accordingly. But if you would adjust it better, remove the less weight of the pendulum a little downwards, which will make it go slower; and then observe anew by the sun as before. If it had gone too slow, you must have removed the weight a little upwards. And this is of such importance in finding the longitude, that if it be not observed, you may sometimes in the space of three months misreckon 7 degrees or more, and yet without any fault in the watches; which under the tropics will amount to above 400 English miles.

Having shown how the watches may be adjusted at land, or how their daily difference may be known, next follows how the same may be done, when a vessel rides at anchor, it being hardly practicable when she is under sail. In the morning, when the sun is just half above the horizon, note what hour, min. and sec. the watch points at, if it be going; if not, set it a-going, and put the indexes at what hour, min. and sec. you please. Let them go till sun-set, and when the body of the sun is just half under the horizon, observe what hour, min. and sec. the indexes of the watch point at, and note them also; then reckon how many hours, &c. are passed by the watch between the observations; which is done by adding to the evening observation the hours, &c. that the morning observation wanted of 12, or 24, in case the hour-hand has in the mean time passed that hour once or twice; otherwise, the difference only gives the time. Then take the half of that number, and add it to the hours, &c. of the morning observation, and you will have the hours, &c. shown by the watch, when the sun was in the south; to which add the equation in the table belonging to that day, and note the sum. Then, after some days do the same: and if the hour of this last day be the same as noted before, your watch is well adjusted; but if it be more or less, the difference divided by the number of days elapsed between the two observations will give the daily difference.

Instead of the sun's rising and setting, you may take two equal altitudes of the sun, before and after noon, and having noted the time given by the watches at the time of both the observations, proceed with it in the same manner as above directed for observing the sun in the horizon. In either of which ways there may be some error, caused by the sun's refraction, which is inconsiderable, and therefore need not be noticed.

6. By means of these watches, to find at sea the longitude of the place where you are.

Give to each of the watches a name or a mark, as A, B, C; and before you sail set them to the time observed by the sun in the place whence you are departing, allowing for the equation of the day where you make the observation; which day you are to note, if the watches be not well adjusted; otherwise it is not necessary. Then, after being at sea, and desiring to know the longitude of the place where you are, that is, how many degrees the meridian of that place is more easterly, or westerly, than the meridian of that place where you set the watches; observe by the sun or stars, the time of the day, as precisely as is possible, and note at the same time, to what hour, minutes and sec. the watches point, which time, if the watches be not set to the right measure, is by the known daily difference to be adjusted; adding the equation of the present day, which gives the time of the day shown by the sun, at the place where the watches were set: And if this time of the day be the same with that observed where you are, then you are under the same meridian with the place where the watches were set by the sun; but if the time of the day observed where you are, be greater than that shown by the watches, you are come under a more easterly meridian; and if less, you are come under a more westerly. And counting for every hour of difference of time 15 degrees of longitude, and for every minute 15 minutes or ¼ of degree, you will then know how many degrees, minutes, &c. the said meridians differ from each other.

7. To find the time of the day at sea.—This, it has been shown above, is necessary for finding the longitude, which time you must observe as precisely as possible; for every minute of time that you misreckon makes a fourth part of a degree in longitude, which amounts, near the equator, to above 15 English miles, but less elsewhere. Wherefore, to find the time of the day with certainty, you are not to trust to the observation of the sun's greatest altitude, thence to conclude that it is just noon, or that the sun is in the south, unless, being between the tropics, you have it just in the zenith; for the sun being near the meridian, remains for some time without any sensible alteration of his altitude; much less are you to rely on the sea-compasses for finding the precise time of noon; neither are the astronomical rings, or other sorts of sun-dials, exact enough for showing the time to minutes and seconds. But it is better to observe the sun's altitude when he is in the east or west, for there his altitude changes in a short time more sensibly than before or after; and thus from the height of the pole and the declination of the sun, the hour may be calculated. Yet as this calculation is rather troublesome, and there may be some errors in taking the sun's altitude, here follows an easier way.

8. How to find the longitude at sea, by observing the rising and setting of the sun, and the time by the watches.—This way neither requires the knowledge of the height of the pole, nor of the declination of the sun, nor the use of any astronomical instruments; neither can the refractions of the sun or stars cause any considerable error; the refraction of the morning differing but little or nothing from that of the evening of the same day, especially at sea.

At the rising and setting of the sun, when half above the horizon, mark the time of the day shown by the watches. Then reckon by the watches what time is elapsed between the two observations, and add the half of it to the time of the rising, and you will have the time by the watches when the sun was at south; to which is to be added the equation of the present day by the table. And if these together make 12 hours, then was the ship at noon under the same meridian where the watches were set with the sun. But if the sum be more than 12, then was she at noon under a more westerly meridian; and if less, then under a more easterly one; and that by as many times 15 degrees as that sum exceeds or falls short of 12 hours.

It is manifest, that by this way you find exactly enough the longitude of the place where you were at noon, or the time of the sun's being in the south; which, though it may differ from the longitude of the place where you are when you observe the setting of the sun, yet you may estimate near enough how much you have advanced or changed the longitude in those few hours, by the log-line, or other ordinary practices of reckoning the ship's way; or, which is the surer way, by the degrees passed in 24 hours by a former day's observation.

You may also, instead of observing the sun's rising and setting, observe the setting first, and then next morning the rising; marking at both times the time shown by the watches; and find thence, after the same manner as before, the longitude of the place where the ship was at midnight.

Finally, you may also, instead of the rising and setting of the sun, observe before and after noon two equal altitudes of the sun, noting the time shown by the watches, and reckoning in the same manner as has been said of the rising and setting. Yet it is to be considered, that the sun's altitudes are best taken when he is about east and west, as has been already intimated. But note, that in sailing north or south, you take not the observations at the sun's rising and setting, but when he is due east and west.

9. But you may put the rule in practice, by taking two equal altitudes of some known star, that rises high above the horizon. For you will thence, according to the rule, know at what time, by the watches, the star has been in the south, and so the star's right ascension being known, as also that of the sun, you may thence easily calculate what time it then was, which being compared with the time of the watches, as before, will give the longitude of the place where you were when you had the star in the meridian.

10. If the watches that have gone exactly for a while, should afterwards differ from one another, as in length of time it may well happen; in that case it will be best to reckon by that which goes fastest; unless you perceive an apparent cause why it goes too fast; seeing it is not so easy for these pendulum watches to move faster than at first as it is to go slower. For the wire on which the pendulum hangs may perhaps, by the violent agitation of the ship, come to stretch a little, but it cannot grow shorter, and the little weight of the pendulum may perhaps slip downwards, but cannot get up higher.

11. When you get sight of any known country, island, or coast, be sure to note their longitude, as exactly as you can by help of the rules here prescribed; that thereby you may correct the sea maps; and that you may know how far you have sailed from any place, to the east or west. And if by any accident the

watches should stop, yet you may at any place, whereof the longitude is certainly known, make them go on again, and adjust them there by the sun, and so reckon the longitudes from that meridian.

12. If all the watches should stop at sea, you must, as speedily as possible, set them going again, that you may know how much you advance from that place towards the east or west; which is of no small importance, since, for want of this knowledge, you are sometimes by the force of currents so carried away, that though you sail before the wind, yet you are driven a-stern, of which there are many instances.

Extract of a Letter, written by Dr. Edward Brown from Vienna, concerning two Parhelias or Mock Suns, lately seen in Hungary. N° 47, p. 953.

I received the account of the parhelias seen January 30, 1669, N. S. about one o'clock in the afternoon, over the city of Cassovia in Hungary. It was communicated to me from a learned Jesuit, called Father Michel, now in this city. There were two parhelias, one on each side of the true sun, and they were so resplendent, that the naked eye could not bear their brightness. The lesser began to decay before the other, and then the other grew larger, and continued near two hours, projecting very long rays from it. On that side next the sun, they were tinged with a pale yellow; the other parts being somewhat obscure. There were at the same time seen several rainbows, with the segment of a large white circle, of a long duration, passing through the two parhelias and the sun, and all this at a time when the air was almost free from clouds, though here and there some very thin ones were scattered.

Of the Conferences held at Paris in the Royal Academy for the Improvement of the Arts of Painting and Sculpture. N° 47, p. 953.

These conferences are held once in a month by several able masters, making reflections and observations on the rarest pieces in the king's cabinet. M. Colbert, who takes a very particular care to make arts flourish in France, being to visit those artists some time since, and having received an account of what had been done in their meetings, expressed himself to this effect; that as it was necessary for the teaching of arts, to join examples to precepts; so he thought it proper that, from time to time, the works of the most excellent painters should be examined, and such observations made on them, as would inform others, wherein the perfection of a picture consists. Which has been ever since practised among them, as the best means to carry the art of painting to its highest pitch. Such an examination of the best pictures discloses many secrets of that art, for which there are no rules, and gives occasion to discuss many important questions, hitherto not treated of. Among the particulars which have been made public of these conferences, we may find these following:

First, A general idea of the art of painting; wherein are considered two principal parts: the one belonging to the theory, the other the practice, and the dexterity of the hand. Where it is observed, that the authors that have written on painting, have not treated of the former part, how considerable soever that be, in regard both of the design and disposition of the pieces.

Next, an account of seven conferences, six of which were made on as many pieces of Raphael, Titian, Paul Veronese, and M. Poussin, and the seventh on that of the Laocoon. Where are to be met with many curious remarks, and among many others these following:

M. le Brun considering a piece of Raphael, representing the combat of St. Michael with the devil, observes, that the expression particularly depends on the bodies which environ the figures; affirming, that it sets out the motion and action in the figure of St. Michael, who seems to have life in this piece: for, as if the air were pressed by the weight of the body descending, it causes whatever it meets with as more light, to be raised, and drives it on high with violence.

In another piece, where Titian represents the body of Christ carried to the grave, M. de Champagne the elder observes the dexterity of the master in ordering the colours and the light. To make the legs of the picture which first presents themselves to stand out, he has wrapped them about with a very white linen sheet, and has clothed Nicodemus, who holds them, with a very vivid and very clear lacca. On the contrary, to sink the rest of the body, he has so disposed the light of the picture, that the shadow of Joseph of Arimathea, who helps to support the legs, falls on its head and shoulders; which contributes to impress on the body the image of death. The arrangement of the colours is also very remarkable in the draperies. For between the green habit of Joseph of Arimathea, and the blue mantle of the Virgin, is the yellow clothing of Mary Magdalen, wherein, what is brown and dusky, is tempered, and borrows of the different colours about it; that the eye may pass by degrees from one of these colours to the other. And because the sleeve of Mary Magdalen, which is of a bright yellow, is near the habit of Nicodemus, which is also of a lively colour, the artist, to prevent those two vivid colours from entrenching on each other, has turned up Nicodemus's sleeve against the yellow, so that we pass from the shadow of one of these colours to that of the other.

The art of the picture, spoken of in the fifth conference, is no less remarkable.

In this piece, done by P. Veronese, is a woman, whose carnation colour is so fresh and bright, that it dazzles the eyes. M. Nocret examining what may cause this beauty, observes, that it proceeds in part from hence, that the artist has ingeniously drawn before her a child clothed in brown; behind her a man in black; and on her side a negro, who makes an admirable contrast with the great lustre and splendour of the carnation.

The last two conferences, on two pieces of M. Poussin, furnish, among other things, very elegant examples of different characters suiting different persons. This master being to represent many persons gathering manna, gives to them all different postures, answering to their characters. On the fore part of the picture are two youths, who, according to the genius of their age, fight about the manna. Near them are men gathering it in the mean time, and eating of it. A little farther off appears a girl, who, unwilling to take the pains of stooping, holds out her coat to receive the manna falling down, and fancies the heavens drop it down for none but herself: which well expresses, says the observer, the soft temper of that sex, which loves not to take pains, but imagines that all must come to pass as they wish. In the other piece, which exhibits the recovery of the two blind men, to whom our Saviour restored their sight, there is an old man, who comes very near, peeping and looking as if he doubted of the truth of the miracle; in which the artist has well observed the genius of aged persons, who commonly are more incredulous and diffident than others.

An Account of some Books. N° 47, p, 956.

I. Institutionum Chronologicarum Libri duo; una cum totidem Arithmetices Chronologicæ Libellis: per Gulielm Beveregium, M. A. è Colleg. S. Joh. Cant. Londini, 1669, in 4to.

In the first book, this author treats of time in general; of a moment, (de scrupulo;) an hour, a day, week, and month: and then of the several sorts of Years, the Celestial, Julian, Gregorian, Ægyptian, Æthiopic, Persian, Syriac and Grecian; also the astronomical, civil, and solar year of the Jews, and the Arabic year.

In the second, he treats of conjunctions, and eclipses, of the equinoxes and solstices, the circle of the sun, and the dominical letter; the circle of the moon or the golden number, the Roman indiction and epacts; of several eminent periods or revolutions of time, as the Metonic, Calippic, the Dionysian and Julian period; of several æras or fixed characters of time; as the Christian and Dioclesian; of the age of the world according to the account of the Grecians. Also of the Jewish æra; the æra or Ethnic account from the taking of Troy; and the Antiochenian epocha: the Olympiads and Agones Capitolini: of the Julian year, the Spanish æra; the æra of the victory at Actium; the epocha of Nabonassar; as also of the Philippean, Alexandrian, and the Yezdegirdican epocha; and of the Mahometan æra, the Hegira, or flight of Mahomet.

In the arithmetical part, the author explains the common characters of arithmetic, as he does also the Indian, Roman, Hebrew, Samaritan, Grecian, Syriac, Arabic and Æthiopic characters. And to the end of the second book he has annexed an appendix, explaining the Hebrew, Syriac, Persian, Æthiopic and Arabic words for the respective months of the year.

II. Elements of Speech: an Essay of Inquiry into the Natural Production of Letters; together with an Appendix to instruct Persons Deaf and Dumb; by William Holder, D. D. and Fellow of the Royal Society. London, by John Martyn, 1669, in octavo.

After considering the nature of language, and alphabets and other representative characters, as also the different organs of speech, the ingenious author examines how many different articulations can be made by several motions and postures of the organs in the parts of the mouth; which applied severally to the kinds of matter, may make several discriminations of sound to the ear, *i. e.* several letters. And of these (as to consonants, viz. letters made by appulse) our author finds, and has described 9. And if possibly any more may be found out, he judges it to be most likely that they will not recompense the discoverer's pains, by being of ready and graceful use, but will be fitter to be cast out among several others.

Now by these 9 articulations with appulse, there will be framed consonants spirital 9; vocal 9; naso-spirital 9; naso-vocal 9; in all 36. Then rejecting those that prove not graceful, nor easy to be pronounced, viz. 2 spiritals, 2 naso-spiritals, and 6 naso-vocals, in all 17; there will remain 19 consonants proper for use, according to the design of letters.

That which renders this piece the more commendable, is its usefulness, to which the author has excellently applied his considerations of this subject; viz. the instructing of persons deaf and dumb.

III. Gauging Promoted, being an Appendix to Stereomatical Propositions, formerly published by Rob. Anderson. Printed for Josh. Coniers, 1669, in octavo.

The Generation of an Hyperbolical Cylindroid; and a Hint of its Application for grinding Hyperbolical Glasses. By Dr. CHRIST. WREN, *LL. D. Translated from the Latin. N° 48, p.* 961.

Let (fig. 5. pl. 9.) D B, E C, be two opposite hyperbolas, having the transverse axis B C, their centre A, and one of the asymptotes G P; and through the centre draw O M perpendicular to B C. Then if the two hyperbolas revolve round their axis O M, it is plain that a body, called an hyperbolic cylindroid, will be generated by that rotation, whose bases, and all the sections parallel to them, are circles.—I say also, that if the body be cut through the asymptote G P, the section will be a parallelogram.

Let it be cut through the transverse axis by a circular section B N C, and also through O and M in equal circles, and at equal distances from the centre; and also through the axis, into the generating figure, whose half, is B D E C, and in the plane of which will be the asymptote G P, through which let the plane B D E be cut at right angles in the plane F H P; and join O H.

Because the triangle O G H is rectangular, therefore the square of O H, or O D, minus the square of O G, is equal to the square of G H; and because D O is parallel to B A, and cuts the asymptote in G, it will be (from the properties of the hyperbola, which are demonstrated in conic sections) the square of O G, together with the square of A B, equal to the square of O D; that is, the square of O D minus the square of O G, equal the square of A B, or of A N; therefore the square of G H, is equal to the square of A N; therefore G H and A N are equal, and at right angles to G A. And the same also is demonstrated of all other sections parallel to the base; consequently an hyperbolical cylindroid being cut through an asymptote, the section is a parallelogram Q. E. D.

Corol. Hence it appears, that on the surface of a cylindroid, though consisting of a double flexure, innumerable right lines may be drawn. It appears also, that this body may be otherwise generated, viz. by the revolution of a parallelogram about the axis, the angle at the axis G A O remaining the same, or the generating line H R continuing immoveable, and either generating or cutting the body.

And if a sharp and straight edge-tool have the same situation to the axis with the generating line, while the mandrel turns round; it is plain, that hyperbolas may be as accurately wrought by the lathe as circles; since nothing more is required for the formation of a cylindroid, than for that of a cylinder, except that in cylinders, the edge of the tool is parallel to the axis, but here inclined.

Therefore it is to be observed, that the species of the hyperbola is varied according to the inclination of the angle G A O; consequently, it may be so easily fitted to a given hyperbola, that there is no need of farther demonstration. But if, the angle continuing the same, the generating line approach nearer the centre, there will arise a less hyperbola, but quite similar to the former.

Experiments on the Motion of the Sap in Trees. By Mr. WILLUGHBY,* *and Mr.* RAY,† *Fellows of the Royal Society. N° 48, p.* 963.

In birch trees, the sap issues out of the least twigs of branches, and fibres of roots, in proportion to their size.

From branches that bend downward, there issues a great deal more sap, than from others of the same size in a more erect posture.

Branches and young trees cut quite off when full of sap, and held perpendicularly, will bleed. And if you cut off their tops, and invert them, they will bleed also at the small ends.

Roots of birch and sycamore cut asunder will bleed both ways, that is, from that part remaining to the tree, and from the part separated; but a great deal faster from the part remaining to the tree. But in a cold snowy day the root of one sycamore, we had bared, bled faster from the part separated, and ten times faster than it did before in warm weather.

In birches, the sap does not issue out of the bark, be it ever so thick; but as soon as you have cut the bark quite through, then it first begins to bleed.

The bark being quite pared off above a hand's breadth round, about several

* Francis Willughby, Esq. the amiable and learned author of several ingenious papers relative to natural history published in the Philosophical Transactions, and particularly distinguished by his Ornithology and Ichthyology, published after his death, by his friend Mr. Ray, was born in the year 1635, and was the only son of Sir Francis Willughby, Knight. He travelled into France, Spain, Italy, Germany, and the Low Countries, accompanied by Mr. Ray, collecting and describing with great attention and exactness the various species of animals which occasionally presented themselves. He died in 1672, aged 37; to the great regret of the republic of letters, and more especially of the Royal Society, of which he was an eminent and estimable member. His principal works are his Ornithology and his Ichthyology, both published after his death, under the care of Mr. Ray, who revised the MSS. and added such species as were not described by Mr. Willughby from the works of other authors, viz. Gesner, Aldrovandus, Clusius, Piso, &c. Mr. Ray assures us that what rendered Mr. Willughby most commendable was his eminent virtue. "I cannot say "that I ever observed such a confluence of excellent qualities in one person." Mr. Ray also informs us, that Mr. Willughby had meditated a voyage to the new world, in order to investigate the natural history of that portion of the globe.

† A biographical account of Mr. Ray will be given hereafter.

birches, much abated the bleeding of the trees above the bared places, but did not quite stop it.

The sap not only ascends between bark and tree, and in the pricked circles between the several coats of wood; but also through the very body of the wood. For several young birches being nimbly cut off at one blow with a sharp axe, and white paper immediately held hard on the top of the remaining trunk, we stuck down pins in all the points of the paper as they appeared wet: and at last, when most of the paper became wet, taking it away, but leaving the pins sticking, we found them without any order, some in the circles, and some in the wood between. And to confirm this further, we caused the body of a tree to be cut off aslope, and then cut the opposite side aslope likewise, till we brought the top to a narrow edge; ordering the matter so, that the whole edge consisted of part of a coat of wood, and had nothing of a pricked circle in it, notwithstanding which, the sap ascended to the very top of this edge, and wetted a paper laid upon it.

To find out the motion of the sap, whether it ascends only, or descends also; we bored a hole in a large birch, out of which a drop fell every 4th or 5th pulse. Then about a hand's breadth just under the hole, we sawed into the body of the tree, deeper than the hole: whereupon the bleeding diminished about one half; and having sawed just above this hole to the same depth, the bleeding from the hole quite ceased; and from the sawed furrow below decreased about half: and it continued bleeding a great while after at both the sawed furrows, the hole in the middle remaining dry. We repeated this with similar success on a sycamore.

Some trees of the same kind and age bleed a great deal faster and sooner than others; but always old trees sooner and faster than young.

A wound made before the sap rises, will bleed when it does rise.

While making these experiments, the weather changed from warm to very cold; whereupon the bleeding in the birches, which began to abate before, quite ceased. But all the sycamore and walnut trees, we had wounded, bled abundantly; some of which before bled not at all, and those that did, did so but slowly; and so continued night and day, when it freezed so hard, that the sap congealed as fast as it issued out. The cold remitting, the birches bled afresh, the sycamores abated very much, and the walnut trees quite ceased.

We pierced two sycamores on the north and south sides, and both of them from equal incisions bled a great deal faster from the north sides than the south, which agrees with the preceding experiment.

We set several willows with the wrong ends downward, and cut off several briars, that had taken root at the small ends. This 29th of May the willows have shot out branches near two feet long; and from the top of the sets, which were a yard high, the briars have also grown backwards from that part which we left remaining to the roots at the lesser ends; they have great leaves, and are ready to flower.

Extract of a Letter from Dr. EDWARD BROWNE *to the Editor, concerning Damps* in the Mines of Hungary and their Effects. Dated Vienna, April* 20, 1669. *N°* 48, *p.* 965.

Having been lately in the copper, silver, and gold mines in Hungary, I hope ere long to give you a particular account of them; presenting this in the mean time concerning damps in these mines; whereof I understand that they happen in most of them that are deep; and that they happen not only in the cuniculi or direct passages, where they walk on horizontally (by these mine-men called stollen) but also in the putei or perpendicular cuts or descents (termed schachts by the same.) They are met with not only in places where the earth is full of clay or the like substances, but also where it is rocky; and one place they showed me in the copper-mine at Herrngroundt, where there had been a very pernicious damp, and yet the rock so hard, that it could not be broken by their instruments; but the descent was all made by the means of gunpowder, rammed into long round holes in the rock, and so blown up. Another place they showed me, where there is sometimes a damp, and sometimes clear weather. When there is much water in the mine, so as to stop up the lower part of this passage, then the damp becomes discoverable, and commonly strong. I procured one to enter it, till his lamp went out 4 or 5 times, in the same manner as at Grotto del Cane in Italy.

Damps are not all of the same force, but some weaker, some stronger; some suffocate in a small space of time, others only render the workmen faint, with no further hurt, unless they continue long in the place. The miners (who think themselves no workmen, if they be not able to cure a damp, or to cure the bad weather, or make the weather, as they term it) perform it by perflation, by letting the air in and out, and causing as it were a circulation of it. In the mine at Herrngroundt they cured a bad damp by a great pair of bellows, which were blown continually for many days. The ordinary remedy is by long tubes, through which the air continually passing, they are able to dig straight on for a long way without impediment in breathing. For some cuniculi are 500 fathoms long; which will not seem strange to any one that shall see the map of the

* What the so called damps in mines are we have already explained at p. 16, vol. i. of this Abridgement.

copper mine at Herrngroundt or the gold mine at Chremnitz. And in the silver trinity mine by Schemnitz, I passed quite under a great hill, and came out on the other side. At Windschach mine by Schemnitz they showed me the place where five men and a gentleman of quality were lost; for which reason they have now placed a tube there. The like they place over all doors and over all ways, where they dig right on for a great space, and have no passage through. At Chremnitz they told me, that 28 men had been killed at one time in 4 cuniculi, 7 in each; and in the sinking of Leopold's pit, which is 150 fathoms deep, they were much troubled with damps, which they remedied in this manner:

They fixed a tube to the side of the schacht or pit, from the top to the bottom; and that not proving sufficient, they forced down a broad flat board, which covered or stopped the pit, or couched very near the sides of it, on all sides but where the tube was; and so forced out all the air in the pit through the tube: which work they were forced often to repeat. And now they having divers other passages into it, the air is good and sufficient: and I was drawn up through it without the least trouble in breathing.

But besides this mischief from poisonous exhalations, stagnation of the air, or water impregnated with mineral spirits, they sometimes perish by other ways. For there being in these mines an incredible mass of wood to support the pits and the horizontal passages, (the putei and cuniculi) in all places but where it is rocky, men are sometimes destroyed by the wood set on fire. And in the gold mine at Chremnitz the wood was once set on fire by the carelessness of a boy, and 50 miners smothered thereby; who were all taken out but one. He was afterwards found to be dissolved by the vitriol water, nothing escaping either of flesh or bones but only some of his clothes.

*A Chronological Account of the several Burnings or Eruptions of Mount Ætna.** *N°* 48, *p.* 967.

To pass by what is related by Berosus, Orpheus, and other less credible authors, about the eruptions of this mountain, both at the time of the expedition of the Ionian colonies into Sicily, and that of the Argonauts, which latter was in the 12th century before the Christian account; we shall first take notice of that which happened at the time of the expedition of Æneas, who being terrified by the fire of this then burning mountain, left that island; whereof Virgil, l. 3. Æneid. gives a notable description.

* In the future volumes of the Transactions we shall meet with several other accounts of this volcano, as vol. 4, 41, and 61.

After this we find in Thucydides, that in the 76th Olympiad, which is about 476 before Christ, there was another fire, and about 50 years after that another. Then in the time of the Roman consuls there happened four eruptions of Ætna, recorded by Diodorus Siculus and Polybius. The next was in the time of Julius Cæsar, related by the said Diodorus to have been so fierce, that the sea about Lipara, an island near Sicily, by its fervent heat burnt the ships, and killed all fishes thereabout. Another we read of in the reign of Caligula, about 40 years after Christ, which was so dreadful, that it made that emperor, being then in Sicily, to fly for it. About the martyrdom of the Romish St. Agatha it burned again very fiercely. Again it burnt A. C. 812, in the reign of Charlemagne. And from the year 1160 to 1669, all Sicily was shaken with many terrible earthquakes; the eruptions of the mountain destroyed a vast tract of inhabited land round about it, and reached as far as Catania; the cathedral of which it destroyed, and the religious men living in it. Again, in the year 1284 there happened another terrible fire about the time of the death of Charles king of Sicily and Arragon.

An. 1329, until 1333, there was another. An. 1408 another.

An. 1444 another, which lasted till 1447.

An. 1536 another, which lasted a year.

An. 1633 another, continuing several years.

An. 1650 it burnt on the north-east side, and vomited so much fire, that by the fiery torrents caused thereby, great devastation was made, as Kircher relates in his Mundus subterraneus.

The same author being in Sicily, observes, that the people of Catania, digging for pumice-stones, find at the depth of 100 palmes, which is about 68 feet, streets paved with marble, and many footsteps of antiquity; an argument that towns have stood there in former ages, which have been overwhelmed by the matter cast out of this mountain. They have also found several bridges of pumice-stones, doubtless made by the flux of the fiery torrents, the earth being very much raised since.

An Account of a Woman having a Double Matrix: Translated by the Editor (Mr. OLDENBURG,) from the French Account lately published at Paris, by M. VASSAL. N° 48, p. 969.*

This figure (see plate 8, fig. 5.) represents the two matrixes, found Ja-

* This (as Mr. Oldenburg has remarked) is not an instance of a double matrix, but a case of tubarian conception. What the French anatomist mistook for a second uterus was the Fallopian tube dilated by the fœtus lodged in it; and what he fancifully termed vasa ejaculatoria were ligaments and blood vessels. The testiculi are the ovaria.

nuary 6, 1669, by Benoit Vassal, surgeon, on opening the body of a woman of 32 years of age, of a sanguine constitution, and a masculine port. These two matrixes were so well disposed by an extraordinary contrivance of nature, that the true one had conceived eleven several times, viz. 7 males and 4 females, all born at the full time, and all perfectly well formed; but they were at last followed by a brother, yet a fœtus, that was conceived in an adjunct uterus, in a place so little capable of distension, that seeking enlargement after it had caused to the mother for two months and a half grievous symptoms, did at last, being of the age of about three or four months, break prison, and found its grave in that of its mother, by a very great effusion of blood in the whole capacity of her abdomen; which cast the mother into such violent convulsive motions for three days together, that she died of them. Whereupon the above mentioned author, after (having embalmed the parts) he had made for a whole month together the particular dissection thereof at his house, before all the most curious and knowing physicians, surgeons, apothecaries, midwives, and other searchers of nature, that are in Paris, thought good to preserve the history thereof, by committing it and the figure of the parts spoken of, to the press, together with a table for better explanation; which we think fit here to annex in Latin:

A. Pars vaginæ.—*B.* Orificium internum matricis apertæ.—*C.* Collum matricis.—*D.* Cavitas matricis.—*E.* Linea separans cavitatem matricis.—*F.* Uteri fundus.—*G.* Duo sinus, inventi in fundo uteri.—*H. H.* Crassities uteri.—*I. I.* Ligamentum latum, sive productio peritonæi lateris sinistri, continens in plicatura sua vasa deferentia et ejaculantia.—*K.* Arteria spermatica.—*L.* Vena spermatica.—*M.* Testiculus.—*N.* Verum vas ejaculatorium insertum fundo uteri per sinum ibi inventum.—*O.* Alterum vas ejaculans, quod ingreditur collum uteri, quò ejaculantur mulieres à conceptu.—*P.* Tuba uteri.—*R.* Ligamentum rotundum.—*S.* Ligamentum latum ab ea parte, ubi spurius hic uterus formatus est.—*U.* Vena spermatica.—*T.* Arteria spermatica.—*Y.* Testiculus. —*Z.* Pars Tubæ.—2. Verum vas jaculatorium, quod fundum uteri ingreditur per sinum supra dictum.—3. Alterum jaculatorium, abiens in uteri collum.— 4. Pars lacerata à fœtu aucto.—5. Fœtus eo situ, quo inventus fuit amnio suo obvolutus.—6. Vasa umbilicalia.—7. Placenta, substantiæ cuidam carnosæ adhærens.—8. Substantia carnosa.—9. Ligamentum rotundum.

An Account of some Books. N° 48, p. 971.

I. Thomæ Hobbes Quadratura Circuli, Cubatio Sphæræ, Duplicatio Cubi, confutata, Auth. Johanne Wallis S. T. D. Geometriæ Professore Saviliano, Oxon. 1669, in 4to.

A refutation, by Dr. Wallis, of Mr. Hobbes's pretended quadrature of the circle, cubation of the sphere, and duplication of the cube.

II. Historia Geral de Ethiopia a Alta, Em Conimbra A. 1660. in fol.

Passing by the account and relations, given in this book, concerning the kingdoms and provinces, comprehended under the Abyssinian empire, and its customs, government, militia, cities, revenues, emperors, religion, discoveries and pretended conversions there made by the Portuguese, together with the contradictions and disappointments, which those of religious orders have met with in that empire; we shall only take notice here of the heads it contains of a philosophical nature: concerning which there is to be found a good description of the true head and course of the river Nile, together with a plain map thereof; as also of the lake Dembea, through which that river passes, and of all the cataracts of it; and then of the climate and temperature, mountains, fertility, herbs, trees, animals wild and tame, and the dispositions of the inhabitants; besides of the Red Sea, and the reason of its appellation.

III. An Historical Essay; stating a probability, that the Language of China is the Primitive Language; by John Webb, Esquire. Printed for Nath. Brook, in London, 1669, in 8vo.

This surprising essay attempts to prove from authorities, that the Chinese have been a people ever since the flood of Noah, and before the confusion of tongues. That their language has continually in all times been preserved in written books; that the characters, wherein those books are written, are the same which from all antiquity were extracted from their original hieroglyphics; that in those characters their language has ever since consisted, and according to them is at this day spoken purely: and that by the same characters their language is generally understood through the whole Chinese world: from all which, considered together, the author concludes, that the mother or natural language of China remains in its ancient purity without any alteration; that the Chinese have subsisted 4000 years without any commixture with other nations; that commerce and conquest have had no influence to change the laws, customs, or language of that people; that the historical computation of the Chinese begins from 2207 years before Christ, which falls out with the 40th year before the confusion of tongues; that no nation in the world is comparable to them for certainty in chronology, &c.

And as to their ingenuities and arts, he notes, that the loadstone and compass have been in use among them above 1100 years, and paper and ink making, above 180 years, both before Christ; that they prepare ink of the smoke of oil; that they have been long acquainted with the invention of gunpowder and fireworks, as also with the manufacture and dyeing of silk; that their potters' mystery, and the manner of making porcelain vessels, excels all other practices of that kind; and that they themselves esteem it so much, that they will not be served in plate; that the earth for making porcelain is to be had in the province of Nankin only; but that there they cannot make any vessels of it, (whether it be from the quality of the water or wood, or temper of the fire, is not known;) but must transport it to Sinclesimo in the province of Kiangsi; that that earth is very lean, fine and shining like sand, which they temper in water, to reduce into little square lumps; that they make use of indigo or woad in painting their work blue, but that for all this knowledge there remains still something necessary to make these vessels aright, which they keep very secret, insomuch that he passes among them for one of the greatest criminals, that reveals this art to any but his own children.

Lastly, as to their art of printing, which was invented among them about 50 years after Christ, their manner is, that they cut their letters with an instrument of iron, as we do wood-prints on wood, lightly gluing the written copy thereon, whereby their books are free from erratas. They are very dexterous at it, and will cut a whole sheet as soon as a compositor with us can set one; and one man will print off 1500 in a day. They have also this conveniency, that their forms may be laid by for as many impressions as they please, and in the mean time print off no more copies than they find sale for, &c.

IV. An Examen of the way of Teaching the Latin Tongue by use alone. Englished out of French, and printed for Mr. Martyn in London, 1669, in 12mo.

Observations concerning the Bath Springs. By Mr. JOSEPH GLANVILL. *Dated Bath, June* 16, 1669. *N°* 49, *p*. 977.

1. The country round this city is very hilly and uneven. The whole tract of the country, within five and seven miles, abounds with coal mines, more or less. But there are no other considerable mines, that I can hear of, nearer than Mendip, which is ten miles hence, excepting some of lead at Berry in Glocestershire, which lies upon the north of this place, about four or five miles distant.

2. The hills for the most part afford a free-stone; and on the north-west of Lansdown the stones dug there are a sort of hard stone, commonly called a lyas, blue and white, polishable.

3. The town and baths are of very great antiquity. Besides what I find in very ancient chronicles to that purpose, one of our great antiquaries (Mr. P.) asserts, that these baths were 800 years before Christ.

4. It is affirmed here, that the town for the most part is built upon a quagmire, though the places all about it are very firm ground. Some workmen, that have been employed in digging, have found a mire ten feet deep, without the north-gate, the highest place of the town, at seven. The earth between is a kind of rubbish; sometimes they find pitching a man's length under ground, and passages for the water to pass; seven or eight feet down they have met with oyster shells.

5. The town and circumjacent country generally abound with cold springs; and in some places the hot and cold arise very near each other; in one place, within two yards, and in others, within eight or nine of the main baths.

6. The guides of the cross-bath inform me that when there is a great west-wind abroad, standing by the springs they feel a cold air arising from beneath: if the wind be at east, and the morning close with a little misling rain, the cross-bath is so hot as scarcely to be endured, when the king's and hot-baths are colder than usual. In other winds, let the weather be how it will, this bath is temperate. The springs that bubble most are coldest. The cross-bath fills in 16 hours both in winter and summer, without any difference from heat or cold, floods or drought. That of the king's in 12 or 14.

7. A man may better (ordinarily) endure four hours bathing in the cross-bath than one and a half in the others. In the queen's bath (which has no springs of its own, but comes all out of the king's) they have found under a flat stone, which upon occasion was taken up, a tunnel, and a yielding mud in and under it, into which they thrust a pike, but could feel no bottom. In the king's bath there is a spring so hot, that it is scarce sufferable, so that they turn much of it away for fear of inflaming the bath. The hottest spring will not harden an egg.

8. The Bath-water does not pass through the body like other mineral waters, but if you put in salt, it purges presently. Upon settlement it affords a black mud, useful in aches, applied by way of cataplasm, to some more successful than the very waters. The like it deposits upon distillation, and no other. Nor has any more been discovered upon all the chemical examinations that have come to our knowledge. One Dr. Astendoff found, that the colour of the salt, drawn from the king's and hot bath, was yellow; that, which was extracted from the cross-bath, white. This doctor concluded, that the cross-bath had more of alum and nitre than the hotter baths, which abound more with sulphur.* And yet that bath loosens shrunk sinews, by which it should seem it

* A water containing alum, nitre, and sulphur, would be very different from the Bath mineral waters.

abounds not much with alum. It is harsher to the taste than the other baths, and soaks the hands more.

9. A man cannot drink half the quantity of strong drinks in the bath that he can out of it; but if he has drank before to excess, it allays much, and is a great refreshment to the body. The bath provokes urine.

10. They are very useful in diseases of the head; palsies, epilepsies and convulsions; in cuticular diseases, leprosies, itches and scabs; in all obstructions of the bowels, as spleen, liver, and mesentery; and the scirrosity and hardness of those parts; in most diseases of woman; in the scurvy and stone; as to which last, while I am writing, an alderman of the city assures me, that his wife, who had been exceedingly troubled with the stone, went into the cross-bath for it, and voided there several stones as large as those of olives, and was never troubled with that distemper after. The bath is also good in cold gouts, as they call them. The same alderman tells me, that it gives him present ease, when he is troubled with the fits of it. He is accustomed to go in as soon as the fit takes him, which then goes off presently, and returns not in a considerable time after; he puts his feet upon the hottest springs in the king's bath. But it has a contrary effect in hot gouts; and some, who are troubled with that distemper tell me, that the bath puts them into a fit if they go into it without preparation; or, if they have the fit before, it inflames it more, and sends it about the body, and disables the joint so, that there is no treading on it for the present. The bath is effectual in the diseases of children, particularly the rickets, removing the humours that proceed from it. It is also good for women that are apt to miscarry, if used moderately. The bath guides go in when they are ready to lie down; and other women of the town use it ordinarily throughout their time, and are never observed to miscarry. It facilitates deliverance. Besides, it is very effectual for the strengthening of broken bones, and good in all cold and moist distempers and weakness of nerves, stupefactions, relaxations, and violent pains; in all which it gives ease, except the lues venerea: for in that (except the malignity be overcome by the methods of physic) it exasperates the pain more. It is an excellent remedy for removing the remaining weakness in gouts, as has been exemplified in old men even to the age of 83 years.

11. There is no instance of cures performed by it in former times, but we have experience of the same in ours and in some others, as in dropsies, cachexies, spleen, &c. In which cases they were shy heretofore of using the bath, for fear of confirming those obstructions, whereas it is now found that their cure is facilitated by it.*

* The asserted efficacy of the Bath waters in some of the diseases here enumerated, is contradicted

12. The bath guides live to a very great age, sometimes to near 100 years; ordinarily, if they are temperate, to 70. There are two at this time above 80, a man and his wife.

13. In the cross-bath the guides have observed a certain black fly with sealed wings, in the form of a lady cow, but somewhat bigger. They say it shoots quick in the water, and sometimes bites. It lives under the water, and is never found but in very hot weather.

14. The cross-bath eats out silver exceedingly; and I am told, that a shilling in a week's time has been so corroded by it, that it might be wound about one's finger.* The baths agree with brass, but not with iron, for they will eat out a ring of this metal in seven years, when brass rings seem to receive no prejudice at all from them.

15. When women have washed their hair with the mixture of beaten eggs and oatmeal, this will poison the bath so, as to beget a most noisome smell, casting a sea-green on the water, which otherwise is very pure and limpid. This will taint the very walls, and there is no cleansing of it, but by drawing the bath.

16. In summer the baths purge up a green scum on the top, but never in winter; but then leave a yellow on the walls.

17. The walls of the hot springs are very deep-set, and large; 10 feet thick, and 14 deep from the level of the street. The cement of the wall is tallow, clay, lime and beaten bricks. In the year 1659, the hot bath (a bath particularly so called, of equal heat with the king's bath) was much impaired by the breaking out of a spring, which the workmen at last found again, and restored. In digging they came to a firm foundation of factitious matter, which had holes in it like a pumice-stone, through which the water played; so that it is probable the springs were brought together by art; which probably was the necromancy, the people of ancient times believed and reported to have contrived and made these baths, as in a very ancient manuscript chronicle I find these words: "When Lud Hidibras was dead, Bladud his son, a great nygromancer (so it is there writ) was made king, and he made the wonder of the hot bath by his nygromancy, and he reigned 21 years, and after he died, and lies at the new Troy." And in another old chronicle it is said, that king Bladud sent for necromancers to Athens to effect this great business; who probably were no other than cunning artificers, well skilled in architecture and mechanics.†

by the accurate observations of later writers. This author does not duly discriminate between the internal and external use of these springs.

* This corrosion of silver by the Bath waters will not readily be credited by those who are acquainted with the chemical properties of these springs.

† Whoever wishes for any thing like accurate observation on the Bath waters, either in respect to

Extract of a Letter, dated September, 1668, from Mr. Muraltus, of Zurich to M. Haak, F.R.S. concerning the Icy and Crystalline Mountains of Helvetia, called the Gletscher. Translated from the Latin by Mr. Oldenburg. N° 49, p. 982.

The highest icy mountains of Helvetia about Valesia and Augusta, in the canton of Bern; about Taminium and Tavetsch of the Rhætians, are always seen covered with snow. The snow, melted by the heat of the summer, other snow being fallen within a little while after, is hardened into ice, which by little and little in a long course of time depurating itself turns into a stone, not yielding in hardness and clearness to crystal.* Such stones closely compacted compose a whole oblong firm mountain, though in summer time the country people have observed it to burst asunder with great cracking, like thunder, which is also well known to hunters to their great cost, forasmuch as such cracks and openings, being by the winds covered with snow, are the death of those that pass over them.

At the foot of these mountains crystals are with great labour dug out; they are found among other fossils of two sorts and colours; some of them are darkish and troubled, which by some are called the crystal-ore, to be plenteously found in the ascent of Mount Gothard; others transparent, very pure and as clear as Venice glass; hexangular, great and small: as in the mountains about Valesia, and the town called Urselen at the foot of the hill Schelenin they are dug out, and sold at a good rate. Of this latter kind my parents, four years ago, transmitted a very large and fair one to Milan for 80l. sterling.

Some Observations concerning Japan, made by an ingenious Person many Years resident in that Country. N° 49, p. 983.

The Japanese doubt not at all of their country being an island; though it be separated from the continent by such narrow channels that no vessel of any considerable burthen can pass them. The air is there very salubrious, but of different temperature on each side of the mountains which divide Japan. The plague has never been heard of there; but the small-pox and fluxes are very frequent. Their mountains are fertile almost to the very top. There are found almost all European sorts of fruit, peaches, apricots, cherries, prunes, apples,

their chemical composition or medicinal powers, should consult the tracts written by Dr. Falconer, and the more recent publications of Dr. Gibbes.

* This assertion, that the ice of these glaciers is gradually converted into stone, is founded in error.

pears, and particularly pippins, bon-chretien pears. Besides these, there is an infinity of other fruit; but hardly any but what are also found in some part or other of India. Silver is there in its highest perfection, but not used in trade; in which is seen nothing but gold, and some small coin of brass; which latter they spoil by refining it too much. Steel also is there very good. The temper of their metals was formerly better than it is now; but yet they make cutlasses, or short swords, exceeding good. The great mountain of Japan is higher than the Peak of Teneriff, since being above 18 leagues distant from the sea side, it may be seen above 40 leagues off at sea. There are 8 volcanoes or fire spitting mountains in Japan; and you cannot go into the champagne, but you discover some of them. There are many medicinal waters and hot springs there, which the inhabitants use in their distempers. They have particular medicines; but they let no blood. They make much use of caustics, by applying upon some nerve or other the powder of Artemisia or Mugwort, and cotton, which they set on fire. They always drink their liquors warm. There is so great a store of venison in Japan, that they care little for cattle, though there be no want of them. They employ most oxen for ploughing; and they make no butter nor cheese, nor are they lovers of milk. They have great plenty of corn and rice. The Japanese are proper enough of stature, and not uncomely in features; they have somewhat prominent bellies. They are exceeding active, and want no judgment; they are also military and valiant. No arts are to be met with among them, that are not known in Europe, except that of making lacca, of which there is some so fine and curious, that whereas in this country one may buy an ordinary small box for 3 or 4 crowns, one of the same size, when made in Japan of exquisite lacca, will sell for more than 80 crowns. The colours with which they dye their stuffs never fade. I have seen one of them which our vermilion and couleur-de-feu come not near to. It is extracted out of a flower like saffron, and one pound of it costs an incredible price. To try whether the colour will not change by lixivium or lye, they apply a hot iron to it; and if there it holds, they assure themselves of the durableness of the colour. They have mathematicians among them, and believe judiciary astrology, insomuch that the grandees undertake nothing without preconsulting those that make profession of that art. Japan yields divers sorts of good merchantable commodities; but chiefly all sorts of silken stuffs, unwrought silk, amber, precious stones, musk, copper, steel, lackwork. The country is very well peopled, and exceeding rich, being well stored with gold mines; and I have seen some of the gold ore, of which 10 ounces yielded 8 of the highest fineness, and pieces of the weight of 120 marks. Their buildings are very good and commodious. The apartments are all below on the ground, separated from one another by partitions of cartoon painted and gilt, which may be folded and removed like screens. Their floors are covered with mats, and sometimes with silken stuff, embroidered velvet, and cloth of gold. All their buildings are but one story high. They have no other conveniences to defend themselves from heat and cold, but such as are usual in Italy and Spain. They use the divertissements of comedies, which are gayer than those of Europe. The spectators are about 200 paces distant from the theatre, which being covered with a vault, makes the voice of the actors to be understood to the very end of the theatre. They love hunting and gaming, as dice, cards, chess, &c. At all times of the day, and in all their visits, they take tea and tobacco. Their language is quite different from the Chinese; but their priests and courtiers, that is, the learned among them, which bear the offices of the court, understand the tongue of Cochin-China, and by this means that of Tonquin, China, Corea, &c. They write neither from the right to the left, nor from the left to the right, but downward. Their government is despotic; the religion Pagan; the Christian hated upon no other account, but that some of those that there professed it, would persuade the Japanese to acknowledge a superiority above the royal dignity, disposing of crowns and scepters. Their morals are very good, their faults being punished as their crimes, even lying and detraction. Their left hand is the more honourable, and they take horse on that side.

Of M. de VILETTE*'s Metalline Burning Concave. N° 49, p. 986.*

M. de Vilette of Lyons, who formerly made that burning concave of 30 inches diameter, disposed of to the king of Denmark, and mentioned before in Number 6, p. 34, has made another of 34 inches diameter, which melts all sorts of metals, even iron itself of the thickness of a silver-crown, in less than a minute of time, and vitrifies brick in the same time; and as for wood, whether green or dry, it sets it on fire in a moment. The king has seen it, and its performances, with great satisfaction; and his majesty is likely to purchase it for the Royal Academy, for making farther experiments with it.

An Account of some Books. N° 49, p. 987.

I. Marc. Malpighii Dissertatio Epistolica De Bombyce, Regiæ Societati dicata.

This laborious performance (dedicated to the Royal Society) gives an account of the production, structure, food, growth, &c. of the silk-worm; together with an accurate anatomical description of all the parts of this insect.

In the anatomical observations on its structure, the author takes notice, among many other things, of its eleven rings or incisures, and of how many small ones each of them is made up; giving their shape, size, &c. Then he goes on to the wrinkles of the body, the head, eyes, teeth (cutting not by an up and down motion but a lateral one) and legs, with their different shapes, joints, claws, together with their posture and motion for spinning.

Of their internal parts, he observes the quality of the humour found in them, viz. concreting by the warmth of one's hand, and leaving a crust: next the mucous and rosy-coloured skin, supposed to be the new skin, found under the exterior. Then he describes the various muscles and fibres, both parallel and oblique, more or less, together with the insertion of the fibres in every ring, and of every ring in the cavity of its neighbouring ring, for producing the progressive motion of the animal; the manner of which is described very particularly.

He passes on to the vessels moistening all the parts, observing their branches and anastomoses; their termination in one common trunk, and the curious net-work they make. These vessels prolonged, he makes to be the lungs, whose structure for respiration he diligently describes, illustrating the same with observations made of other insects, and with some trials, showing, both that air issues out of their body, and that oily liquors will suffocate them, only by stopping the orifice of their wind pipe.

From the lungs he proceeds to the heart, which he says reaches from the head to the tail, being of a strange figure, and rather many hearts than one; whose motion of systole and diastole he describes, taking also notice how the vital humour passes from one little heart to another.

The ventricle, [stomach] he observes, reaches also from one extreme of the worm to the other; describing its substance, shape, fibres, and vessels bedewing it, together with its resemblance to the ventricle [stomach] of other insects: where he particularly notes the great voracity of the silk-worm, affirming, that it will eat as much in one day, as its whole empty body weighs.

In the sides of the belly about the ventricle he finds a woof of vessels, containing the silky juice; describing their progress from the mouth downward into the belly, and their strange flexures and meanders; whose end he affirms to have at length, after a long and patient search, found out. Of these vessels he makes a large and curious description, as also of their different juices, as the cause of the different sorts of webs and bags.

He also gives an accurate account of the fine texture of the spinal marrow and the cranium.

He notices the gradual change of the silk-worm, after it is exhausted by spinning; how all the parts are altered, and the whole disposed to assume the form of the aurelia or chrysalis, divesting itself of its coat in the space of 1 min. 10 sec. the manner of which he very curiously describes, having attentively beheld it himself. He adds, how the wings and other parts are formed for the papilio or butterfly, and how indeed the wings are latitant under the second and third ring of the worm, before it works the bag.

Of the aurelia he describes its shape and all the parts, and particularly the remaining vestiges of the silky intestines, the ventricle, and the concrete melleous juice therein, together with some though rare and scarce perceptible motion of the heart. Then, how the aurelia changes into a butterfly, and in what time, viz. in the space of ten days in summer, and in a month's time in autumn and winter. Where he adds, how the eggs begin in the females upon their change into aurelias, and how at last the butterfly breaks out by means of its claws and a sharp liquor.

To this he subjoins a particular description of the form of the butterfly, and all its parts; of the motion of its heart, of the discriminative marks between the male and female; of the curious structure of the ovarium; the parts of generation, the coitus, and the strange length of the time it lasts, the male beating his wings about 130 times in one copulation, the multitude of eggs, amounting to 300, 400, sometimes 500; and the death of the poor fly, following five days after the coitus in summer, but not before the 12th day in August.

He omits not to instruct the reader of the ways of keeping the eggs, and the manner of ordering them for hatching; where he takes notice of one kind of butterfly in Sicily, which is made twice fecund in one year, viz. in the end of April and the end of August.

He concludes with the way of winding off the bags, and informs us how many threads together will make good substantial silk; where he affirms, that sometimes he has reckoned 930 Bononian feet of silk wound off from one bag, without the exterior lanugo and the inmost last part, which both together might make a fourth part of that length more.

II. Description Anatomique d'un Cameleon, d'un Castor, d'un Dromedaire, d'un Ours, et d'une Gazelle. A Paris, 1669, in 4to.

The dissections of these animals and the observations thereon were made in the royal library at Paris.

Of the chamelion * (which they say was an Egyptian one;) they allege that there are two other sorts, one of Arabia, and another of Mexico, and observe: 1. That its contrary motions of swelling and contracting are not made as in other

* Lacerta chamæleon. Linn.

animals, dilating and presently after contracting their breast for respiration, in a constant and regular order; since they have seen it swell for the space of above two hours, during which time it would indeed contract a little, but almost indiscernibly, and also swell a little again, but with this difference, that the dilatation was more sudden and more visible, and that by long and unequal intervals; they having also observed it to subside for a long time, and much longer than swelled.

2. That the grains [or coloured eminences] in the chamelion's skin assumed various appearances, being of a bluish-grey, when the animal was in the shade motionless, and had not been touched a long while, but that the paws underneath were yellowish-white, and the space between the grains of a pale and yellowish-red; and that the said grey colouring him all over when at rest, and remaining on the inside of the skin when flayed, (which seemed to argue it was the natural colour) did, when exposed to the day light, change in the sun, so that all the places of its body, struck by that light, took, instead of their bluish-grey, a browner grey, approaching to a minim; but the rest of the skin, not shone upon by the sun, changed its grey into divers brighter colours, which formed spots half an inch big, of an isabella colour, by the mixture of the yellowish white in the grains, and the light brown in the ground of the skin; the other skin not shone upon by the sun, and remaining of a grey paler than ordinary, being like cloth mixed of wool of divers colours, the ground continuing as before. The sun ceasing to shine, the first grey returned by little and little, and being then touched by one of the company, there appeared presently many very black spots on his shoulders and fore-feet, which happened not when he was handled by those that took care of him. Being wrapped in white linen for two or three minutes, he was taken out whitish, and having kept this colour awhile, it vanished insensibly: which experiment refutes those who give out, that the chamelion takes all colours but white. Having put him on divers things of several colours, and wrapped him up in them, he assumed none of their colours but the white, neither took he this, but the first time of the trials.*

3. The structure and motion of his eyes turning two different ways at one and the same time, which yet is not true of the chamelions of Mexico.

4. His way of taking hold of the small branches of trees, like that of a parrot, who puts two of his claws before and two behind, whereas other birds always put three before and one behind.

* The changes of colour observed in the above-mentioned grains or tubercles on the chamelion's skin, would seem to depend upon the quantity of fluids contained in the cutaneous vessels; which quantity of fluids in these vessels must vary, according as the skin is more or less stretched in the emphysematous swelling, which, by a peculiar structure, this animal can produce at pleasure.

5. His having no spleen; a very little heart, and exceeding little brain, in which appeared no mark at all of any sense of hearing, this animal neither re ceiving nor giving any sound.

6. His tongue being furnished with and fastened to a long tromp, [tube] serving to launch it out, for the taking of flies,* on which he feeds, and not on air alone; the observers having found many flies in his stomach and guts.

In the castor†, or beaver, they note:

1. His two sorts of hair, one short, soft and fine, to defend him from cold, the other long and thick, to receive the mire, in which they often wallow, and to hinder it from getting to the skin.

2. His teeth, formed after a peculiar manner, exceeding fit to cut trees, which they do to build themselves lodgings to breed their young ones in; for which purpose nature has also furnished them with such fore feet as exactly resemble the hands of a man; the hind feet, proper for swimming, being formed like those of a goose.

3. His bladders containing the castoreum (distinct from the testicles) of which they found four great ones about the lower part of the os pubis, of which two were above the other two, but closely joined to one another, the two upper being likely to prepare that matter, and the two other to bring it to the perfection of more consistence and unctuousness, as also of a stronger scent and deeper yellow colour; for which purpose the two latter are of a glandular composition. But under this second sort of sacks they found yet another long one, full of liquor, more yellow and liquid, and more elaborate than that in the former, of a different smell, and like to the yolk of an egg; of which they write from Canada, that beavers use it to make themselves an appetite when they want it, and that they squeeze it out by pressing with their paws the bladder which contains it; and that the savages anoint with it the gins they set for these animals, to draw them thither.

4. His testicles not fastened to the back-bone, as several authors affirm, but on the side of the os pubis about the groin, altogether hid, and not appearing at all, no more than the penis, before the skin was removed. The penis, contrary to that of a dog, which goes from the os pubis to the navel, descended here downward to the anus, where it terminated.

5. The heart had its left auricle greater than the right (which is also found in some other animals), whereas in man it is contrary. They found no foramen ovale, which many authors assure to be in all amphibious animals, and even in

* The extremity of the chamelion's tongue is constantly moistened with a slimy or glutinous liquor, by which it is enabled to catch its prey.

† Castor Fiber. Linn.

men, that are divers, and stay long under water. But it may be, that this castor having been kept several years from going into the water, that hole had been closed.

In the dromedary it was observed that it has but two small hoofs on the end of his feet, the soles of them flat and large, being very fleshy, and covered only with a soft, thick and little callous skin, proper enough to march in the sands of Asia and Africa; that the six callosities of his legs being opened, their substance was found to be between flesh, grease, and ligament, some having a collection of a thick purulent matter mixed; that that callosity under the breast, strongly fastened to the sternum, was considerably large every way, and much suppurated; that his inward parts were like those of a horse; but that in his second ventricle [stomach] there were many square openings, being the entry of about 20 cavities, made like sacks, placed between the two membranes that compose the substance of the whole stomach, in which sacks, as in convenient receptacles, it is probable that camels do for a long while keep the water they drink in great quantity, when they meet with any, for a supply in dry and desert places;* that the lungs had but one lobe; that the heart was remarkably large, viz. nine inches long and seven broad; that, contrary to other tongues, which are every where rough from within outwards by store of small eminences tending from without inwards, a part of this tongue had them from within outwards, &c.

The bear has a very particular structure of his legs, and their substance very good to eat. Its claws differ from those of a lion; by being more equal and more compact. The teeth differ from those of a lion in this only, that they are less. The thorax consists of 14 ribs. There appeared no distinction in his guts, as in other animals; they were 40 feet long, whereas those of the lion, formerly dissected by the same observers, were but 25. The kidneys had a very peculiar structure, viz. a membrane containing 56 small kidneys, actually separate from one another, each covered with its proper membrane; here and there connected by very fine fibres; every one having a large base outwards, and straightening itself inwards; that base being in some a hexagon, in the most a pentagon, and in others square; and the whole representing as it were a ripe pine-apple: therefore probably so great, and divided into so

* Many ruminating animals have four stomachs; camels have the same, in common with them; but they have moreover an appendix to the paunch [ventriculus, properly so called] which serves as a reservoir for water, and which may be regarded as a fifth stomach, peculiar to themselves. It is provided with cavities in the manner here described, wherein the water can be retained many days, pure and unmixed with the food; and from which it can be thrown either into the paunch or be brought up again into the œsophagus by the contraction of the muscular coats of this receptacle, whenever these animals chuse.

many smaller kidneys, that it might contain and evacuate the greater plenty of serosities, to be found in a bear, because he has but little of insensible transpiration, by reason of the thickness of the habit of his body, not favourable for it. The brain they observed to be four times greater than that of the lion they opened. The eyes exceeding little, the crystalline very oddly situated, and drawn on one side of the axis of the eye.

But that which is particularly taken notice of in the description of this animal, is, 1. The strength of its temper and constitution, by which it is able, though it have but a little stomach and strait guts (among which there is no cœcum) to digest with ease all sorts of edibles, raw flesh, fish, lobsters, insects, herbs, fruits, honey ; supplying by the force of his temper the defect of a commodious structure. 2. The small capacity of its liver and spleen to receive excrementitious matter; which argues, that the action of the natural heat is so well regulated, that it is not subject to defect or excess. 3. The singular faculty of increasing to a great bulk, by which, though it be born exceeding small, it grows a very large animal, its natural moisture being so perfect, as to render the parts capable of extending themselves, and of increasing their magnitude without lessening their strength.

The gazelle or wild African she-goat (the same with the dorcas or strepsiceros) was of the size and shape of a hind, its hair fallow, except that of the belly, which was white; its eyes large and black ; the horns black also, streaked cross-ways, 15 inches long, very sharp, pretty straight, but a little turned outwards about the middle ; in part hollow, and by a sharp bone fastened to the head. Toothless in the upper jaw, as being of the ruminating kind. Very cloven footed, and small-hoofed before, but thick-fleshed on the hinder parts of the legs, like a camel.

As to the inward parts, it had a liver shaped like that of a man, divided into two lobes; and in the hollow part of the liver there were two lymphatic branches, which fastened the trunk of the vena porta to the superior orifice of the stomach. The substance of this liver plainly appeared to them glandular, each grain of it being pierced, as they thought, in the middle, by reason of a little red cleft they had, whence issued blood when pressed. And the cause why these glands seldom appear unsevered one from another, may be, that when the animal is in health they are spungy and filled out with blood, which they are not when it is sick or emaciated, &c.

III. Labyrinthus Algebræ, Auth. Joh. Jac. Ferguson. Printed at the Hague in 4to. 1667.

There do not appear to be any methods of resolving equations in this book, but what were taught by former authors.

IV. An Answer to the Hydrologia Chymica of W. Sympson; by R. Wittie, M. D. 8vo. 1669.

We did not think our readers would wish to be detained with an analysis of the hydrologia chymica (mentioned at p. 303 of our Abridgement); much less do we think they would be interested by a detail of the particulars in this reply.

The Weight of Water in Water with ordinary Balances and Weights. By Mr. Boyle. *N° 50, p.* 1001.

A glass bubble, of about the size of a pullet's-egg, was purposely blown at the flame of a lamp, with a somewhat long stem turned up at the end, that it might the more conveniently be broken off. This bubble being well heated to rarefy the air, and thereby drive out a good part of it, was nimbly sealed at the end, and by the help of the figure of the stem was by a convenient weight of lead depressed under water, the lead and glass being tied by a string to one scale of a good balance, putting into the other scale as much weight as sufficed to counterpoise the bubble, as it hung freely in the midst of the water. Then with a long iron forceps I carefully broke off the sealed end of the bubble under water, so as no bubble of air appeared to emerge or escape through the water, but the liquor by the weight of the atmosphere sprung into the empty part of the glass bubble, and filled the whole cavity about half full; and presently, as I foretold, the bubble subsided, and made its scale preponderate so much, that there needed 4 drachms and 38 grains to reduce the balance to an equilibrium. Then taking out the bubble with the water in it, by the help of the flame of a candle, warily applied, we drove out the water (which otherwise is not easily excluded at a very narrow stem) into a glass counterpoised before; and as we expected, we found it to weigh about 4 drachms and 30 grains, besides some little that remained in the egg, and some that may have been rarefied into vapours, which added to the piece of glass that was broken off under water and lost there, might very well amount to 7 or 8 grains. By which it appears, not only that water has some weight in water, but that it weighs very near, or altogether, as much in water as the self-same portion of liquor would weigh in the air.

The same day we repeated the experiment with another sealed bubble, larger than the former, being as large as a great hen-egg, and having broken this under water, it grew heavier by 7 drachms and 34 grains; and having taken out the bubble, and driven out the water into a counterpoised glass, we found the transvasated liquor to amount to the same weight, abating 6 or 7 grains, which it might well have lost on such accounts as have been above mentioned.

Observations in two Voyages to the East Indies. By Mr. RICHARD SMITHSON. *N° 50, p.* 1003.

From England to Cape Finisterre in Gallicia, in 44° north lat. the winds are as variable as with us in England; only the Bay of Biscay is more subject to storms, and the sea more rough, the waves running very high. From thence to 34° the wind is variable also, but if within 100 leagues of the European continent, it is generally inclined to north-east. From 34 degrees, towards the coast of Africa, or about the meridian of the Canaries, the wind is always about the north-east. Yet in winter, on the coast of Africa there are sometimes westerly storms that are violent, but of no long continuance. And in summer, when it is sometimes calm, the air will come variably. These north-east winds hold most commonly to 8 degrees north latitude, and then begin the tornado winds, which are most part confined between 8 and 4 degrees north latitude. They are seldom or never more southerly; but on this side the line they have sometimes been met between 11 and 12 degrees north latitude, and sometimes in 9 and 10 degrees. These tornadoes are uncertain winds, blowing from all points of the compass in the same hour, and sometimes the wind shifts thus without intermitting, and at other times it will be quite calm between every blast. They are so irregular that if 4 or 5 ships sail together, as near as is fitting for ships that keep company, at the same instant, very often, every ship will have a several and contrary wind. This place is almost always infested with dreadful thunders, lightnings and rain. And the nearer you are to the Afric shore, so much more dreadful is the thunder and rain: but the further westward you go, the thunder and rain is the less, and the winds not so uncertain; so that, if you go as far west as the meridian of the east-side of Brasil, there is little thunder, neither does the wind come down in such puffs and flaws; but between 4 and 8 degrees it is most inclined to calms, with very great and thick fogs, and the rains come not in such violent showers. This is a sure rule, that near the African shore, and as far as 100 or 200 leagues west, the north-east winds commonly incline more and more to the east; so that by the time you come to the west of the meridian of the Azores about 20 degrees, the trade or constant wind will be mostly east-north-east. Now as from 34 to 44 degrees near the continent of Europe, the winds are commonly between east and north; so after you come as far west as the meridian of the hithermost of the Azores, they are commonly between south-west and north-west. And for this reason, ships that are outward bound to the Straits keep near the coast of Portugal, but homeward bound they are many times forced to run far west to fetch a westerly wind. Likewise ships bound to Barbadoes go by the Canaries, but come home a great way to the north-west of the Azores. And the Virginia ships are twice as long in going out as they are in coming home, and many times longer: for they come home directly before the wind, but go out round about as far as the tropic, or at least to 28 degrees latitude, for the benefit of the north-east wind; and when that has carried them far west, they come back to the northward again: and then, as the westerly wind hangs more or less southerly, they have a good or bad passage. About 3 or 4 degrees northern latitude the south-east wind begins to take place, between the equator and the tropic of Capricorn. But the nearer you are to the coast of Africa, it is so much more southerly; and as you approach to the coast of Brasil, it inclines more and more easterly. And there is not only a variation in the wind in respect of longitude, but also in respect of latitude: for, near the equator, the wind is more southerly than it is in the same meridian near the tropic of Capricorn; as for example, in the great bay of Guinea, which our seamen call the Bight of Guinea, the wind is mostly south; and inclines as much to the west as to the east: but in the same meridian near the tropic of Capricorn, it is constantly between south-east by east, and south-east by south. And on the contrary, in that meridian which may be about 100 leagues to the eastward of Brasil, near the equator, the wind is between south-east and east-south-east; and in the same meridian the winds near the tropic are more variable, but most part about north-east.

In our latter voyage, after we came to 32 deg. south latitude (to which place from the line we were much becalmed) we had fair weather, and a constant wind between WNW. and WSW. all along to the Cape; in that vast space between Rio de la Plata and the Cape, the wind being all the year westerly. But about the Cape, from the end or middle of September to the beginning of April, the winds are variable as in England. The rest of the year they are westerly, with intolerable storms.

Considerations on the Variety of Slate, and a Computation of the Charges of covering Houses with them. By Mr. SAM. COLEPRESS. N° 50, p. 1009.

Among the several materials for building, that for covering is not the least to be considered: among the kinds thereof our country slate is not to be despised, either for beauty, duration, or cheapness. The first sufficiently appears by the slated houses in or about London. For the second, the most experienced helliers or slaters affirm that some sorts have continued on houses several hundred years, and are yet as firm as when first put on. And as to the third, the computation of charges may give some idea of the comparative expenses, being little more than tiling, and much less than leading. To know the goodness of any slate, take and strike it against any hard body; then if the sound be distinct and clear, that sort of stone is firm and good. Or, if in hewing it does not break before the edge of the sects, the slater's hewing instrument, you may not doubt of the firmness of the slate. But if, after it has been exactly weighed, it be put for 2, 4, or 8 hours under water in a vessel, and afterwards taken up and wiped very clean with cloths, if then it weigh more than before, it is of that kind which imbibes water, and therefore not so fit to endure any considerable time without rotting the laths and timber. These stones may be pretty well guessed at, whether they be of a close or loose texture by their colour: For the deep blackish blue is apt to take in water; but the lighter blue is always the firmest and closest. To which may be added the touch; for a good stone feels somewhat hard and rough; whereas an open stone feels very smooth, and as it were oily. Another way is to place the stone longways perpendicular in the midst of a vessel of water, no matter how shallow the water be, so it exceed half a foot in depth, and so let it remain a day, or half a day, or less. If it be a good firm stone, it will not draw water above half an inch above the level of the water, and that perhaps but at the edges only, the parts of which might be somewhat loosened in the hewing. But a bad stone will draw water up to the very top, be the stone as long as it will, all over.

It is worthy of observation, that if a side wall happen to take wet by the beating of the weather, or the like, when nothing else will cure it, our kersing with slate will quickly remedy it.

Some Observations concerning the odd Turn of some Shell Snails, and the darting of Spiders. By an ingenious Cantabrigian. In a Letter to Mr. J. Ray. N° 50, p. 1011.

I have found two sorts of shell-snails, easily to be distinguished from each other, and from all besides; because the turn of the wreaths is from the right hand to the left, contrary to what may be seen in common snails. They are very small, and might therefore well escape so long the more curious naturalists; neither of them much exceeding, at least in thickness, a large oaten corn.

The first I thus describe: the open of the shell is pretty round, the second turn or wreath is very large, and the rest of the wreaths, about the number of six, are still lessened to a point. This *turben* or conical figure is near a quarter of an inch, the colour of the shell is duskish, yet when the animal is shrunk in it, you may see light through it, and then it is of a yellowish colour. These shells are extremely brittle and tender.

Of the second sort I send you half a dozen. They seem to be much stronger and thicker shelled; they are nearly half as long again as the other, and as slender; they have the exact figure of oaten corn, being as it were pointed at both ends, and the middle a little swelled. The aperture of the shell is not exactly round, there being a peculiar sinus in the lower part of it. I think you may reckon about 10 spires, having their turn from the right hand to the left. The colour of the shell is of a dark and reddish brown.

When the snails creep, they lift up the point of their shells to a perpendicular, and extend with part of their body two pair of horns, as do most of their kind. In March they are always to be found in pairs. Aristotle affirms all these kind of creatures to be of a spontaneous birth, and no more to contribute to the production of one another than trees, and therefore to have no distinction of sex. I have no reason to subscribe to his authority, since I have seen so many of them paired, and in the very act of venery. That they engender then is most certain; but whether those that are thus found coupled be one of them male and the other female, or rather, as you observed in the Catalogue of Plants growing wild about Cambridge, that they are both male and female, and do in the act of generation both receive into themselves and emit a like penis, I leave to further and more minute discovery to determine.*

The Romans knew something extraordinary of these kind of animals, that made them so choice of them, as to reckon them among their most delicate food, and to use all care and diligence to breed and fat them for their tables, as related by Varro.

Of late, comparing Bussy's Histoire Amoureuse de Gaule with Petronius Arbiter, out of whom I was made to believe he had taken two of his letters word for word, besides other love intrigues, I found in running him over what satisfied me not a little in this very subject of snails, viz. that these very animals, as well as other odd things in nature, as truffles, mushrooms, and no doubt too the cosci or great worms in the oak, another Roman dainty, were made use of by the ancients to incite venery. You will there find, that the distressed and feeble lover prepares himself with a ragout of snails' necks (cervices cochlearum,) in which the penis is placed.

* The two snails here mentioned seem to be the *turbo bidens* and *turbo perversus* of Linnæus. Both species are generally found among mosses, on the bark, and in the hollow of old trees. With respect to the breeding of these animals, it is similar to that of most of the snail tribe, in which each individual is a hermaphrodite, but incapable of impregnating itself, so that it is necessary that two individuals should reciprocally communicate by their respective organs.

These snails are to be found frequently under the loose bark of trees, as old willows, and in the ragged clefts of elms and oaks, &c. but no where else, that I could observe.

All spiders that spin a thread, (those which we call shepherds or long-legged spiders never do,) produce these threads observable in the air in summer in such infinite quantities every where, especially towards September. I had exactly marked all the ways of weaving used by any sorts of them, and in those admirable works I had always observed that they still let down the thread they made use of, and drew it after them. At length, in close attending on one that wrought a net, I saw her suddenly in the mid-work to desist, and turning her tail into the wind, to dart out a thread with the same violence that water spouts out of a spring; this thread, taken up by the wind, was in a moment emitted some fathoms long, still issuing out of the belly of the animal; by and by the spider leaped into the air, and the thread mounted her up swiftly.

After this first discovery, I made the like observation in almost all the sorts of spiders I had before distinguished; and I found the air filled with young and old, sailing on their threads, and undoubtedly seizing gnats and other insects in their passage; there being often as manifest signs of slaughter, as legs, wings of flies, &c. on these threads as in their webs below. Many of these threads that came down out of the air were not single, but snarled and with complicable woolly locks, now more now less; on these I did not always find spiders, though many times I had found two or three upon one of them; whereas when they first flew up, the thread was always single, or but little tangled, or thicker in one place than another. I observed them get to the top of a stalk or bough, or some such thing, where they exercise this darting of threads into the air, and if they had not a mind to sail, they either swiftly drew it up again, winding it up with their fore feet over their head into a lock, or break it off short, and let the air carry it away. This they will do many times together, and you may see those that have chains of these locks or snarled thread before them, and not yet taken flight.

Again, I found, that after the first flight, all the time of their sailing they make locks, still darting forth fresh supplies of thread to sport and sail by. It is further to be noted, that these complicated threads are much more tender than our house webs.

In winter, about Christmas, I have observed them busied in darting; but few of them sail then, and therefore but single threads only are to be seen; and besides, the young ones only of last autumn's hatch are then employed, and it is more than probable that the great ropes of autumn are made only by the large ones, and upon long passages and summer weather, when great numbers of prey may invite them to stay longer up in the air.

An Account of some Books. N° 50, p. 1017.

I. Georgii Sinclari* Ars Nova et Magna Gravitatis et Levitatis. Roterodami, in 4to. An. 1669.

The subject of this book is the spring and pressure of the air, with some considerations on the weight and pressure of water; as also concerning a vacuum, the effects of pumps, and on pendulums, hygroscopes, &c.

Whether the doctrine or experiments, here delivered, be new and unheard of, as the author is pleased to think, we leave to the intelligent to judge. But we find ourselves obliged to take notice that the excellent treatise of the honourable Robert Boyle, entitled, New Experiments Physico-Mechanical, touching the spring of the air and its effects, was printed two years before that time, about which the author of this book says, in his preface to the reader, he came to London, and there committed his then unprinted papers to the censure of the Philosophical College there, meaning the Royal Society, of whom he complains in the preface, that he waited for an answer from them for almost two years in vain; adding, that he afterwards found, in divers books printed in English, many things taken out of his manuscript.

But to undeceive the reader in this particular, we shall first desire him to compare the date of the edition of Mr. Boyle's book, above-mentioned, with

* Mr. George Sinclair, or (as it is in his printed books) Sinclar, was professor of philosophy in the university of Glasgow, and author of several works on mathematical and physical subjects. He was dismissed from his professorship soon after the restoration, on account of his political principles; and again recalled to it on the change of government at the revolution in 1688, and he died in 1696.

Mr. Sinclar's publications were, 1. Tyrocinia Mathematica, 12mo. Glasc. 1661; 2. Ars Nova et Magna, &c. 4to. Roterod. 1669; 3. Hydrostatics, 4to. Edinb. 1672; 4. Hydrostatical Experiments, with a Discourse on Coal, 8vo. Edinb. 1680; 5. Principles of Astronomy and Navigation, 12mo. Edinb. 1688. Besides which, a very extravagant production, called "Satan's Invisible World discovered," has been ascribed to him: it bears the initials, G. S. of his name.

Mr. Sinclar's writings are not destitute of ingenuity and research; though they may contain some erroneous and eccentric views. His work on Hydrostatics, and his Ars Nova et Magna Gravitatis et Levitatis, and perhaps also his political principles, provoked the indignation of some persons, on which occasion Mr. James Gregory, author of the Optica Promota, &c. and then professor of mathematics at St. Andrews, animadverted on him rather severely in a treatise entitled, "The Great and New Art of Weighing Vanity, &c. under the name of Patrick Mathers, Archbedal of St. Andrews."

Considerable attention seems to have been paid by Mr. Sinclar to such branches of hydrostatics as were of a practical nature; and it has been said he was the first person who suggested the proper method of draining the water from the numerous coal mines in the south-west of Scotland. During the period he was deprived of his office, he resided about the southern and border counties, collecting and affording useful information on subjects of mining, engineering, &c. particularly he was employed by the magistrates of Edinburgh, on the then new plan for supplying the city with water, &c.

that of the time of this author's visiting London, and there exposing his MS. That done, it will easily appear which party has the priority in the explication of this doctrine of the air: where it must not pass unnoticed, that Mr. Sinclair, when he was in London, in his visits to Mr. Boyle discoursed much with him on that subject, and by his own acknowledgment, then made to that gentleman, received much light from him concerning it. Secondly, We shall take notice, that it is so great a mistake, that this author commended his manuscript to the judgment of the Royal Society, that it is not so much as mentioned any where in their register-book that such papers came ever before them, which yet is their constant and careful practice to do of all things of that nature; to which we must add, that the person with whom he left those papers of his, Sir Robert Moray, perhaps with a desire to recommend them to that illustrious body, affirms, that he did not at all judge them proper to be exhibited there, because they seemed to him to contain nothing new or extraordinary.

II. Observationes Medicæ, M. Leyser, &c. 8vo. There is nothing in these observations worthy of notice.

III. Ottonis Tachenii Hippocrates Chymicus. Venetiis, in 12mo. In this book we have an account of the chemical opinions of Hippocrates, certainly of no use in the modern improved state of chemical science.

IV. Th. Bartholini Dissertatio de Cygni Anatome, nunc aucta à Casp. Bartholino, F. Hafniæ, 8vo. 1668.

In this discourse the author chiefly observes the wonderful internal fabric of this stately bird (the swan), and more especially the admirable structure of its wind-pipe; which is so framed, that together with the œsophagus it reaches down to the sternum, into which, as a safe case, it winds itself, and being gone down to the bottom of that cavity is turned up again, and gets out of the straits of the sternum, and climbing up the intermediate clavicles, on which it leans as on a base, it bends to the thorax. But before it comes to the thorax and the lungs, it forms a kind of larynx with an os hyoides, covered with a large membrane, and resembling a musical pipe, wide above, but with a narrow slit, and strait and depressed below. Under which larynx, before the wind-pipe enters into the lungs, it is divaricated into two branches, like bronchia, thicker in the middle, but narrower where they are near the lungs: in which particular it differs from the human arteria aspera, which, it is true, is also divided into branches, but not before it enters the lungs.

After this description of the structure of this organ, he considers the fitness of its contrivance for such a respiration as was requisite for an animal that by long diving and sinking its neck to the bottom of waters was to find its food.

V. Ægidii Strauchii Breviarium Chronologicum. Witebergæ, in 12mo.

VI. Abrégé Chronologique de l'Histoire Sacrée et Prophane, par le P. Labbe, de la Comp. de Jesus. Paris, in 12mo. in 5 volumes.

Extract of a Letter containing the whole Process used in France for making Sea Salt by the Sun. Communicated to the Editor by a French Physician, residing near the chief Place where it is practised. N° 51, p. 1025.

Sir,—I send you the plan of our salt marishes, with the way of making our salt.

A A A, is the sea. (Fig. 6, pl. 9.)—I I. the entry, by which the sea-water passes into B B.—B B, the first receptacle; in which the water is kept 20 inches deep.—C C C, the second receptacle, where the water makes three turnings, as you see, and is 10 inches deep.—22, the opening, by which the first and second receptacle have communication one with another.—E E F, the third receptacle, which is properly called the marish.—*d d d d d d,* is a channel very narrow, through which the water must pass before it passes out of the second receptacle into the third.—33, is the opening, by which the water runs out of the second into the third receptacle. The pricks, you see in the water throughout the whole scheme, mark the course and turnings, which the water is forced to make before it comes to *h h h h h,* which are the places where the salt is made.—*h h h h h,* are the beds of the marish, where the salt is made; and in them the water must not be above an inch and a half deep. Each of these beds is 15 feet long and 14 feet broad.

99999, are the little channels between the beds.

88888, are the apertures, by which the beds receive the sea-water after many windings and turnings.

When it rains, the openings 22, 33, are stopped, to hinder the water from running into the marish marks, E E F. Unless it rain much, the rain-water does little hurt to the marish; and although it rain a day or a night, we do not let the water which is in the marish run out, the heat of the sun sufficiently exhaling such rain-water, if for example it be not above an inch high. Only, if it have rained very plentifully that day, no salt is drawn for the three or four next following days. But if it rain five or six days, the people are then forced to empty all the water of the beds by a peculiar channel, conveying it into the sea; which channel cannot be opened, but when it is low water. But it very seldom rains so long as to constrain men to empty those beds.

It is obvious, that in the hottest years most salt is made, and besides the heat of the sun, the winds contribute much to it, as less salt is made in calm than in windy weather. The west and north-west winds are the best for this purpose.

Our country people draw the salt every other day, and they draw out of those beds marked *h h h*, every time more than a hundred pounds weight of salt. In the hottest part of the summer salt is made even during night. The instruments used to draw the salt have many small holes to let the water pass, and to retain nothing but the salt.

According to the quality of the earth or ground of the marish, the salt is made more or less white. The reddish earth makes the salt more gray; the bluish, more white; besides, if you let a little more water run in than you ought, the salt becomes more white; but then it yields not so much. Generally all the marishes require a fat earth, neither spungy nor sandy.

And as to the whiteness of salt in particular, there are three things to be considered: First, that the earth of the marish be proper. Secondly, that the salt be made with good store of water. Thirdly, that the salt-man, who draws it, be dexterous. In this isle of Rhe some draw very dark salt, and others as white as snow; and so it is in Saintonge. Chiefly care is to be taken, that the earth at the bottom of the beds mingle not with the salt. The salt we use at our tables is perfectly white; the reason of which is this, that four or five hours before the salt is to be drawn, we draw the cream, or that salt which is formed on the top of the water. The grains of it are smaller than of the other. Generally the salt of Saintonge is somewhat whiter than ours. I do not well know the size of the grains of the sea salt made by fire; but ours is of the size of a pepper-grain, and of a cubical shape.

The marishes are preserved from one year to another by overflowing them, so that the water be near a foot high above the marishes. There are marishes that are only separated from the sea by a ditch of 20 or 30 feet broad; others are further distant, receiving the water by channels that are made according to the situation of the marishes. To preserve this ditch it is strengthened with stones from the foot to the top, as streets are paved.

Concerning the Eruptions of Mount Ætna, An. 1669. *Communicated by some English Merchants, residing in Sicily. N° 51, p.* 1028.

For the space of 18 days before this fire broke out, there was a very thick dark sky in those parts, with thunder and lightning, and frequent concussions of the earth, which the people make terrible reports of, though I never saw nor heard of any buildings cast down thereby, save a small town or village, called Nicolosi, about half a mile distant from the New Mouth, and some such other slight buildings among those towns that were after overrun by the fire. Besides, it was observed that the old top or mouth of Ætna did, for two or three months before, rage more than usual; the like of which did Volcano and Strombilo, two burning islands to the westward.

The eruption took place on the 11th of March 1669, about two hours before night, on the south-east side or skirt of the mountain, about 20 miles beneath the Old Mouth; and 10 miles from Catania. At first it was reported to advance 3 miles in 24 hours; but April 5, it scarce moved after the rate of a furlong a day; and at this degree of progress it continued for 15 or 20 days after, passing under the walls of Catania a good way into the sea; but about the latter end of this month (April) and the beginning of May, it bent all its force against that city; and passed in divers places over the walls; but its chief fury fell on the convent of the Benedictines, having large gardens and other ground between them and the wall: which when it had filled up, it fell with all its force on the convent, where it met with strong resistance, which made it swell almost as high as the higher shops in the Old London Exchange, this convent being built much after that fashion, though considerably larger. Some parts of the wall were driven in, whole and entire, almost a foot, as appeared by the rising of the tiles in the midst of the floor, and bending of the iron-bars that went cross above. But here its fury ceased the 4th of May, running forward in little channels or streams chiefly into the sea. It had overwhelmed in the inland country about 14 towns and villages, whereof some were of good note, containing 3 or 4 thousand inhabitants, and stood in a very fruitful and pleasant country, where the fire had never made any devastation before: but now there is not so much as any sign where such towns have stood; only the church and steeple of one of them, which stood alone upon high ground, still appear.

The matter which thus ran was nothing else but divers kinds of metals and minerals, rendered liquid by the fierceness of the fire in the bowels of the earth, boiling up and gushing forth like the water at the head of some great river; and having run in a full body for a good stone's cast or more, the extremities thereof began to crust and curdle, forming when cold those hard porous stones which the people call sciarri, having the nearest resemblance to huge cakes of sea-coal, full of fire. These came rolling and tumbling over one another, and where they met with a bank, would fill up and swell over, by their weight bearing down any common building, and burning whatever was combustible. The chief motion of this matter was forward, but it also dilated itself, as a flood of water would do on even ground.

About 2 or 3 o'clock at night we mounted a high tower in Catania, whence we had a full view of the mouth; which was a terrible sight. Next morning we would have gone up to the mouth itself, but durst not come nearer than a furlong off, for fear of being overwhelmed by a sudden turn of the wind, which carried up into the air some of that vast pillar of ashes, which to our apprehension exceeded twice the size of St. Paul's steeple in London, and went up in a straight body to a far greater height than it; the whole air being thereabout all covered with the lightest of those ashes blown off from the top of this pillar: And from the first breaking out of the fire till its fury ceased, being 54 days, neither sun nor star were seen in all that part.

From the outside of this pillar fell off great quantities of stones, but none very large, neither could we discern any fire in them, nor see where the fiery matter broke out, there being a great bank or hill of ashes between it and us.

At the mouth whence issued the fire and ashes, was a continual noise, like the beating of great waves of the sea against rocks, or like thunder afar off, which sometimes I have heard here in Messina, though situated at the foot of high hills, and 60 miles off. It has also been heard 100 miles northward of this place, in Calabria, whither the ashes have also been carried: And some of our seamen have also reported that their decks were covered therewith at Zante, though probably not very thick.

About the middle of May we found the face of things much altered, the city of Catania being three quarters of it compassed round with these sciarri, as high as the top of the walls; and in many places they had broke over. The first night of our arrival a new stream of fire broke out among some sciarri, which we were walking upon an hour or two before, and they were as high as to be even with the top of the wall. It poured itself down into the city in a small stream of about 3 feet broad, and 9 feet long of fire, the extremities still falling off into those sciarri; but this stream was extinct by the next morning, though it had filled up a great void place with its sciarri. The next night another much larger channel was discovered, pouring itself over another part of the wall into the castle ditch, which continued, as we were informed, some days after our departure. Divers of those small rivulets ran at the same time into the sea, and it does so still at this very day, though faintly.

Having spent a couple of days about Catania, we again went up to the mouth, where now without any danger of fire or ashes we could take a free view both of the old and new channel of the fire, and of that great mountain of ashes cast up. That which we guessed to be the old bed or channel, was a three cornered plot of about 2 acres, with a crust of sciarri at the bottom, and upon that a small crust or surface of brimstone. It was hedged in on each side with a great bank or hill of ashes, and behind and at the upper end rose up that huge mountain of the same matter. Between those two banks the fire seems to have had its passage. At the upper end in the nook upon a little hillock of crusted sciarri was a hole about 10 feet wide, whence probably the fire issued; and it might have had several other such holes, since either crusted over or covered with ashes. At the bottom of this hole the fire was seen to flow along, and below it was a channel of fire, beneath that surface of sciarri, which being cleft a-top for some space, we had an easy and deliberate view of the metal flowing along, whose superficies might be a yard broad, though possibly it carried a greater breadth underneath, the gutter sloping. What depth it had we could not guess: it was impenetrable by iron hooks and other instruments. We were very desirous to have got some of this matter at the spring head, but we could not penetrate into it. It is likely that some running may have been more yielding than we found this. From this channel, but especially from that hole above it, issued great quantities of a strong sulphureous smoke, wherewith some of our company were at first almost stifled. About once in a quarter of an hour there rose a pillar of smoke or ashes, but nothing comparable to the former; which seemed to come from the middle top of that new made mountain.

We found the people busy in barricading the ends of some streets and passages, where they thought the fire might break in; and this they did by pulling down the old houses, and laying up the loose stones in manner of a wall.

At present the fire is said to have run a mile into the sea, and as much in front. The superficies of the water, for 20 feet or more of those rivulets of fire, was hotter than to endure one's hand in it, though deeper it was more temperate, and those live sciarri still retained their fire under water, as we saw when the surges of the sea retreated back in their ordinary reverberations.

The general face of these sciarri is in some respect not much unlike, from the beginning to the end, to the river Thames in a great frost at the top of the ice above bridge; I mean lying after such a rugged manner in great flakes: but its colour is quite different, being mostly of a dark dusky blue, and some stones or rocks of a vast size, close and solid.

But notwithstanding their ruggedness, and store of fire, which we could see glowing in the clefts and cavities, we made a shift to ramble over a good part of them; as it is said also that people would do the same in its greatest violence of burning. For as those live sciarri, and those rivers of fire themselves were so tough and impenetrable as to bear any weight, so the superficies of the sciarri might be touched and handled, the fire being inward, and not to be discerned but near hand, especially in the day time: And it was a strange sight to see so great a river come so tamely forward; for, as it approached unto any house, they not only at good leisure removed their goods, but the very tiles and beams, and what else was moveable.

The whole country, from the very walls of Catania, to 20 miles on this side, is full of those old sciarri which former eruptions have cast forth, though the people remember none so large as this last, or that burst out so low. This country is notwithstanding well cultivated and inhabited; for length of time has either mollified much of those old sciarri, or new mould or ashes have overgrown them; though there still remains much country, which probably will never be made serviceable.

An Account of M. Steno's Discours sur l'Anatomie du Cerveau. Paris, 1669, 12mo. N° 51, p. 1034.

In the beginning of this discourse the author represents, that those who search after solid knowledge, will find nothing satisfactory in all that has been hitherto written concerning the brain; that all, which anatomists agree in, is only, that it consists of two substances, a white and a greyish, and that the former is continued with the nerves that are distributed through the whole body; and the latter serves in some places for a kind of cortex to the white, and in others severs the white filaments from one another. But that they are yet ignorant what those substances are, in what manner the nerves are joined in the white, and how far their extremities advance in it; from which disposition yet depends all the diversity and variety of our sensations and motions. And as for the ventricles or cavities of the brain, he affirms them to be no less unknown than its substances; some anatomists lodging in them the spirits, others making them the receptacles of the excrements of the brain; and both perplexed in assigning the source and issue of the excrements and the spirits, and the manner of the production of the latter.

Besides this, he finds a great defect in the way of dissecting the brain, and having shown the imperfection in the common ways, he proposes and recommends that, though difficult one, of continuing the filaments or threads of the nerves through the substance of the brain, to see where they pass, and where they terminate.

Next, he entertains the reader with an enumeration of the chief errors of anatomists touching the brain. And here he examines particularly the systems of Dr. Willis and Monsieur Descartes. In the former he takes special notice, that the author thereof lodges the common sense in the corpus striatum; the imagination in the corpus callosum, and the memory in the greyish substance which encompasses the white. But then he declares, that these assertions are very obnoxious; for, whereas Dr. Willis describes that corpus striatum, as if there were two sorts of streaks or rays, some ascending some descending, he finds, that a separation being made of the grey body from the white, those rays will be found to be all of the same nature, that is, they make part of the white substance of the corpus callosum, which passes towards the marrow of the back, separated in divers layers by the intervening of the greyish substance. Which being so, with what certainty can we be made to believe, that those three operations are performed in those three bodies? And who can tell us, whether the nervous fibres have their beginning in the streaked body, or whether they rather pass through the callous body into the grey substance?

In the system of M. Descartes, he finds, that that philosopher has rather devised, in his Treatise of Man, an engine that performs all the actions men are capable of, than described man as he really is; which he undertakes to prove by divers instances, taken from the Cartesian fabric of the parts of the brain: in the doing of which our author shows great dexterity, skill, and accuracy. And from hence he proceeds,

To observe the want of exactness in the cut or figures hitherto given of the brain; and although he acknowledges, that the best figures, hitherto made of that part, are those of Dr. Willis, yet he finds several faults committed here and there, and conceives there are many things to be added for making them perfect; which certainly this able anatomist will consider further, and, as he shall see cause, rectify.

Having thus discussed the way hitherto practised of dissecting the brain, and the little light to be thence derived, together with the defectiveness of the figures belonging thereto, he leaves it to the consideration of judicious men, what faith is to be given to the explanations made upon such unsolid foundations; and that done, he declares which are the only two ways of attaining a true knowledge of an engine; viz. one, by having the contrivance of it discovered by the author himself; the other, by taking it in pieces to the very least parts, and examining them all both severally and jointly.

He concludes his discourse by recommending the method which seems best and most convincing to him, for making true discoveries in anatomy; where he advises, that for obtaining the true history of the parts, we should examine and accordingly draw them in that state in which they are found naturally, without at all forcing them; thereby to find, whether the parts are indeed joined together or separated, and what situation is assigned them by nature itself. And he desires, that the anatomist would not only be intent upon the part on which he is for the present employed, but also reflect upon all the operations he has made before he came to that part, which may have caused some change or other in the same, as to its situation, connexion, &c.

Besides this exact attention on all the operations, he further counsels the change of the ways of dissecting, and deduces the ill consequences of binding ourselves to certain fixed laws of dissecting each part. Of the necessity of this change he brings manifest proofs; and having done so, he alleges the reason why he says nothing of the use of the parts of the brain, nor of the actions called animal, it being impossible to explain the motions that are made by an engine, if the fabric of the parts be not known; and those anatomists rendering themselves ridiculous, that discourse so magisterially of the use of the parts, of which they know not the structure.

Last of all, he observes, that for the acquiring of some good knowledge of the brain, there must be dissections and examinations made of as many heads as there are different species of animals, and different states and conditions of each kind; since that in the fetuses of animals it will be seen how the brain is formed; and what could not be seen in sound and entire brains may be seen in such as have been changed by sickness.

[This account of Steno's work is followed in the original by a minute, but uninstructive analysis of Dr. Wittie's Answer (mentioned in N° 49) to Sympson's Hydrologia Chymica, noticed at p. 303 of this vol. of our Abridgment.]

An Account of divers Minerals, thrown up and burned by the late Fire of Mount Ætna. By some ingenious Merchants of England. N° 52, p. 1041.

As the examination of the matters thrown up from fiery mountains may best explain the cause of such eruptions, we publish the following account received by a ship lately arrived from Messina.

First, a quantity of ashes, taken up in divers parts of and about Ætna; some at the top or mouth of the new made mountain; some a mile off, some four, some ten miles, some but half a mile distant, and others on the skirts of the said mountain; whereof the four first were found to agree well enough with their distances, but the last two to differ much both from the former and from one another; the former four sorts having been found very dry like dust, but the two latter being still very moist, though long exposed to the hot sun; besides that the last two differ from one another, as one sort of them consists of hard and small lumps, the other of very soft dirty grains, yet both moist and of a vitriolate taste.

Secondly, some of the cinders or sciarri, taken up at some distance from the mouth; and of the coarser some are black, with a crust of brimstone, some of a red hue, others finer, said to be got out of the gutters of fire at the very mouth. Both these kinds are light; but there is a third sort of stone, very solid and ponderous, which seems to be made up of a conflux of divers minerals melted together.

Thirdly, a piece of sal ammoniac, and several pieces of sandever, besides those moist vitriolate ashes above mentioned.

The fire spread about 3 miles in breadth, and 17 miles in length; the same being now quite extinct, except that in the clefts or hollows of the rocks of sciarri some still remains glowing.

Some Observations concerning the Organs of Generation, made by Dr. EDMUND KING, F. R. S. and by Dr. REGNERUS de GRAEFF, Physician in Holland; which latter occasioned the Publication of the former. N° 52, p. 1043.

The observations of the former (Dr. King) as he presented them in writing to the Royal Society, the 17th of December 1668, were as follow:

About 3 weeks since I produced the testiculos cuniculorum marium dissected in several shapes; which appeared to several of this illustrious company, as well as to myself, to be indeed made up of vessels; and I then had particularly shown them to Mr. Hook and some others, and the manner how they lay. And being desired to give in the account in writing, I cannot but affirm here, that I find the vessels in the testes of this kind of animals to lie in round folds, in the manner of the little intestines, but both ends of each roll meeting at their insertion, which seems to be made into the ductus nervosus: and every one of these little rolls are very curiously embroidered with other vessels, which I judge to be veins and arteries by reason of their reddish colour, appearing in them even to the bare eye.

These little rolls lie in ranges, having a kind of uniformity not unpleasant to behold by a good light. But I do not mean, that every one of these rolls is one entire tube, but consists of many tubes, besides the said embroidery of veins and arteries: for, when I cut one of the said rolls transverse, there seemed to me 5, 6, or more distinct tubes in one roll, contained as it were in one common membranula; but the fine texture and tenderness of them is such, that they will not admit of expansion in such a manner as some other testes will, and especially as that of a rat is said to do by Dr. de Graeff; yet if it shall appear that they are really made up of vessels, though of ever so many sorts, I humbly conceive you will not think the experiment lost, because I suppose the chief thing intended by these trials to be, that it may be well known what indeed the body of the testes is made of; whether indeed it be a congeries of vessels and liquors without any intermediate substance, as was asserted by me to many of this honourable company, several years ago, concerning most if not all parenchymous parts, which was inserted in Number 18 of the Phil. Trans. since which time I have made several experiments of the same kind, about the testes, the pancreas, &c. and as far as I have examined them, I find them to be only a texture of fine tubes or ducts, with more or less liquor, without any other substance.

But perceiving the testes of several animals to be variously composed and interwoven; I proceeded ad testiculos tauri, which I have dissected and ordered several ways; some boiled, others broiled, others infused in spirit of wine, hot and cold, &c. and upon the best examination I can make, I cannot see any of this intermediate substance, or indeed any thing else, that is not vessel or liquor.

Now in obedience to your commands, I have added another experiment, and that is testiculi humani, hoping to prove it clearly, and perhaps to put it out of dispute, that it is nothing else but a congeries of vessels of various sorts, and their several liquors; and that there is no such thing as an intermediate substance; and to demonstrate this, I think, it will evidently appear to the bare eye, by what I have here expanded,* which is the true genuine substance of the testiculi humani, I mean the body of it, after the tunica albuginea is removed, without any addition or diminution, excepting only what liquors are dried up during the time of the expansion, which could not be prevented in making such a scheme of it as this is. And this is continued from one end to the other of the glass, on which you see it exhibited in several places without breaking; which breaking yet does not at all prejudice the truth of this experiment. And although I had not time to open every part, which you see to be like that substance, yet I can order it so, as to show with ease, that that also is nothing but a congeries of vessels, as aforesaid, not yet opened.

And if it should be objected, that this may be drawn out into seeming vessels, which yet may not be really such; I answer, that these vessels have the same appearance in the body of the testis, as to denote them such, before they are drawn out; and in the extension it does sometimes so happen, that one of them will extend easily near half a yard long, before it breaks, though so exceeding delicate and tender, as you may imagine: and when it is thus extended, it has a kind of resemblance to the corrugations of the epididymis, and keeps the same figure and magnitude in the whole extent of them, as to the sight, unless they begin to dry, and then you may see them lose their gyrations upon stretching: as you may see of both sorts on the glass above mentioned.

* See fig. 1, pl. 10. which represents only the 4th or 5th part of what was exhibited of the same testis after the same manner with this on glass.

And that the greatest part of these vessels are arteries, or other vessels, that immediately receive liquors from them; I may prove, I think, from another experiment made by injection into a part of the arteria præparans, before I began to expand the body of the testis; whereupon opening the part, which I saw discoloured, I found, that many of these tubes had received some of the fine particles of that matter with which I tinged my injected spirit.

And to prevent another objection that might arise, viz. That these particles might possibly change their colour only outwardly; I used other endeavours to assure myself that the said particles were indeed included within the cavities of these tubes. In the doing of which, I moistened those two tubes with spirit of wine, to see whether that would remove or alter those particles; but finding no such thing, I pricked and opened with a fine needle part of the containing tube; whereupon I saw issue forth several of those liquid particles before mentioned: which assures me farther that this is a mere scheme or congeries of vessels.*

I have made several other experiments of this kind, about other parts of the body; not to mention the muscles, heart and kidneys, because I suppose that few men will now contend for a parenchyma in them. And as I have opportunity, I shall show, I hope, that all sorts of glands (so called) are nothing else but vessels (and their liquors) variously wrought, and receptacles of several liquors for divers uses; the difference of which alters their colour, consistence, &c. My meaning is, that there is in no reputed gland any other thing than there is in the body of the testis, viz. that it has not this or that intermediate substance, but that the liquors regularly come and go to and through them in fine tubes (in such and such heaps and figures, as may make them appear so and so formed in several parts of the body, where they are situated;) as also, that the more conspicuous vessels of the body have other vessels, that help to make up their coats, and serve for the nourishment of the same, besides such as import or export those liquors, for the conveyance of which they were designed for common use.

So far Dr. King: as to Dr. de Graeff, we shall deliver what he lately imparted to us upon this subject, in his own words, extracted out of his letter, dated Delft, July 25, 1669, accompanied cum testiculo gliris dissoluto, et transmisso in spiritu vini; represented in fig. 2, pl. 10.

Quod Clar. D. Clarck ait, se parenchyma (quod succum quendam denotare dicit affusum vel effusum et aliquomodo concretum in vasculorum et fibrillarum

* Had the art of injecting with quicksilver been then known, the fullest demonstration would have been obtained, that these vessels (concerning which there was so much controversy among anatomists at the period here mentioned) are real tubes in which the seminal liquor is secreted.

interstitiis,) in testiculis virorum et aliorum etiam animalium, testimonio sensuum ostendere posse; hoc ego, pace tanti viri dixero, nonnisi autopsiâ edoctus admittere possum. Quandoquidem sæpissimè hominum aliorumque animalium testiculos, exceptis tenuissimis quibusdam membranulis, ita dissolverim, ut ne umbra quidem talis parenchymatis remaneret; imò, quod magis est, quorundam animalium testiculos ita dissolvi, ut visus acie ne quidem membranulæ illæ conspicerentur. Et ut verba mea factis comprobem, mitto ad te Gliris testiculum meo modo dissolutum, ut videas, an glandulæ tales in testibus (quales proponit Clar. Dn. Clark in Epist. sua, 18 Maii, 1668. Transactionibus Philosophicis inserta) vel etiam parenchyma tale, quale in epist. sua 10 Maii, 1669, describit, reperiatur. In hunc ferè modum reliquorum animalium testiculos dissolvere possum, eâ tamen diversitate, ut in nonnullorum testibus aliquæ membranulæ tenuissimæ, et in quorundam, radix præterea epididymidis Highmori, *remaneant*.

So far these two industrious physicians, which though it looks very fair to evince, that the testes of animals are made up of nothing but vessels and their liquors, yet Dr. Timothy Clarck, and several other expert anatomists and physicians, still doubt whether that be so indeed, considering that not only it cannot be denied, that this curious heap of strings or supposed vessels was at first covered all over with a mucous matter (which in so fine and tender a part may well be thought to serve for a parenchyma), but also that Monsieur de Graeff must himself grant, that in the said part there are found certain small membranes besides those vessels he is asserting, such another substance being conceived to be highly necessary to serve for a medium, whereby that compounded liquor, which from the greater vessel passes into the minute arteries, nerves, and lymphæducts of the testes, may be secreted, and according to the different nature and figure of their several particles conveyed into those several small and subtile vessels.

Extract of a Letter (dated July 1669*) from Dr.* William Durston, *Physician at Plymouth, to Lord Viscount* Brouncker, *P. R. S. concerning a very sudden and excessive Swelling of a Woman's Breasts.* N° 52, *p.* 1047.

I present your lordship with a phenomenon in nature, which, for its rarity and prodigiousness, may obtain the favour of your perusal. The thing is evident, and can be attested by thousands, as well as by the lord ambassador, who was an eye witness of it, and imposed the task on me, of giving your lordship a perfect narrative, which is as follows:

Elizabeth Trevers, 23 or 24 years of age, fair of complexion, brown haired, of a healthy constitution, low of stature, of honest repute, but of mean and poor parentage, near this town, was on Friday July 3, 1669, in good health, and went well to bed, where she took as good rest and sleep as ever before, but in the morning when she awakened, and attempted to turn herself in her bed was not able, finding her breasts so swelled, that she was affrightened to astonishment. Then endeavouring to sit up the weight of her breasts fastened her to her bed, where she has lain ever since, yet entirely without pain and weakness either in her breasts or in any other part. See fig. 3, pl. 10.

This being noised abroad, several physicians and surgeons resorted to her: some proposed cutting off her breasts, which I was wholly against, advising for the present only an emollient and temperately warm fotus, and once gave her a purgative bolus, upon the taking of which she had ten motions deorsum, and the swelling somewhat abated; but the maid was so weakened upon it for two or three days after, that I durst not attempt any thing of that nature since; sed quia passa fuit suppressionem mensium per sex retro menses, diuretica nonnulla, et sanguinis menstrui prolectamenta præscripsi, intending also phlebotomy. The tubuli or pipes of the breasts are all very hard and swelled, and indeed the whole breasts seem to be nothing else but those tubuli, and little or nothing of wind or water. As near as we can guess the left breast weighs about 25 pounds, but the right somewhat less. And the skin of the back, neck, and belly, seem to be drawn towards the breasts to serve for the distension. The measures of the breasts are these:

The circumference of the right breast two feet seven inches; of the left breast three feet one inch and a half; the length of the right breast from the collar-bone one foot five inches and a half; the length of the left breast one foot seven inches and a half; the breadth of the right breast as it lies one foot one inch; the breadth of the left one foot four inches and a half.

Now what should occasion those monstrous tumours of the whole breasts, and that so suddenly in one night, keeps us in great suspense. There occurs nothing in this point satisfactory in the writings of Platerus, Rhodericus à Castro, Fontanus, Forestus, or any other of the moderns that I have seen, writing de Morbis Mulierum, suitable to what may be offered upon the data of the circulation of the blood, the lymphæducts, and the vasa chylifera thoracica, and probably some capillary vessels branching thence (in their progress to the subclavians) through the intercostal muscles into the breasts.

This narrative having been read at the R. S. the author desired to impart what he should further observe in this very extraordinary accident; he therefore wrote some while after a second letter to the Editor, (dated Sept. 17, 1669,) an extract of which is as follows:

About the beginning of this month, the woman in coughing brought up at several times some blood; but this I soon took off, and at that time there appeared several cutaneous ulcers upon her breasts and other parts, et abundè in verendis (ut à fœminis edocebar) which last I cured; but those on her breasts in part remain, and daily discharge, by the sole application of colewort leaves, much sanious matter; and the patient complaining also at that time of grievous interjuncture pains, especially upon the tibiæ, I applied Empl. de Ran. &c. and gave her three succeeding mornings a purgative medicine. The third day, it wrought sursum et deorsum pretty briskly; after which her pains vanished as well as many of those ulcuscula, and her breasts much lessened, and her pains also; and she, though drooping much before, and out of hopes of life, exceedingly revived. I consequently gave her on Sunday last a medicine which wrought upward plentifully; and she daily gets strength since, and her breasts abate. I design to salivate her, in hopes to correct that vicious ferment which is spued out of the genus nervosum into the breasts, and contributes much to those tumours.*

An Account of two Books. N° 52, p. 1054.

I. Gauging Epitomised, by Michael Dary. London, printed by W. Godbid, 1669, upon one folio page.

II. Histoire Naturelle des Animaux, Plantes, et Mineraux, qui entrent dans la Composition de la Theriaque d'Andromachus; par M. Charas.† Paris, 12mo.

As there are above 60 sorts of different drugs, which are ingredients of this no less difficult than famous and useful medicine, which was invented by Andromachus, physician to Nero, and as those drugs are subject to be sophisticated, and require different preparations, so there are few men that are sufficiently skilled to chuse aright all those ingredients, or dextrous and patient enough to prepare them well. The author of this book treats of this celebrated medicament, and not only teaches the way of composing it, but intersperses many not

* This woman died in October following, see N° 53, where there is a description of the size and internal appearances of the breasts.

† Moses Charas was in great repute in the 17th century for his pharmaceutical knowledge. When by that cruel and impolitic measure, the revocation of the edict of Nantes, Lewis the XIV. compelled many thousands of his most industrious subjects to emigrate from France, Charas, who was a Huguenot, sought an asylum in Spain. Here, however, in consequence of some freedom of speech on religious matters, he fell under the scourge of the Inquisition, and was thrown into prison, nor did he procure his enlargement, until he had solemnly renounced the Protestant faith. Besides the treatise above-noticed, he wrote Experiences sur la Vipere (of which an account will be given hereafter) 1669, Pharmacopée Galenique et Chymique, 1672; and an Account of a new Method of administering the Peruvian Bark, inserted in the Memoirs of the Parisian Academy of Sciences for 1692.

inconsiderable remarks touching the nature and virtues of all the drugs which compose it.*

A Description of Dr. (afterwards Sir) CHRIST. WREN'S *Engine for Grinding Hyperbolical Optic Glasses. Translated from the Latin. N° 53, p.* 1059.

Let there be three bodies, P, Q, R, (pl. 9, fig. 7,) fit for grinding; of which let P and Q be equal, and of the shape of a pillar, and R resemble a lens. Let P have a rotation about the axis AB, Q about CD, and R about EG. Let AB and CD be in different planes, but so posited that EG, being produced, may be at right angles both to AB and CD. Lastly, let the bodies approach to each other, as much as necessary; still however preserving the same situation and position of their axes.

I say, that by the rotation and mutual attrition of these bodies, new geometrical figures will arise, of which P and Q will be equal hyperbolical cylindroids, and R an hyperbolical conoid, given both in species and magnitude.

I have both the demonstration, and the model of the machine itself for grinding hyperbolical glasses. To give a figure of it, and to describe it minutely, would cause more trouble, both to the artist and myself, than the inventing of it. For since the geometrical principles are already explained, it will be easy to guess what sort of an instrument it is. The parts are three oblong, plain, strong, and smooth boards, laid upon each other. The lowest and middlemost sustain the unequal supporters (or handles supporting the mandril) placed alternately; which is necessary, on account of the obliquity and decussation of the two mandrils. The poppet-heads or supporters, disposed according to the length of the uppermost board, are equal to it; and the mandril is inserted into a perforation in the nearest poppet-head. I omit the several wheels, rollers, strings, weights, screws, and the other apparatus necessary for strengthening the machine, and giving it a swift motion. P belongs to the lowest board; Q to the middlemost; R to the uppermost; R is a lens of glass; Q a grinding tool that grinds the lens; P the director correcting the grinding tool; which, moving obliquely, and in a direction different from that of the lens and grinding tool, continually effaces and grinds off any defect communicated to the model by the attrition of the lens, and its own matter.

Wherefore, since the formation of the hyperbolical conoid is so simple and easy, being produced by only circular motions; and since the motion is double

* In modern pharmacy preparations less compound have been introduced in place of these heterogeneous farragoes.

and various, it is probable, that hyperbolical glasses must be derived from these principles, or none.

Some Inquiries concerning the Salt Springs and the Way of Salt-making at Nantwich in Cheshire, answered by WILLIAM JACKSON, *M. D. N° 53, p.* 1060.

1. What is the depth of the salt springs ?—The depths are various, in some places not above three or four yards; at Nantwich, the pit is full seven yards from the footing about the pit; which is guessed to be the natural height of the ground, though the bank be six feet higher, accidentally raised by accumulated rubbish or walling as they call it. In other places the springs lie much shallower; for in two places within our township the springs break up so in the meadows, as to fret away not only the grass, but part of the earth which lies like a breach at least half a foot or more lower than the turf of the meadow, and has a salt liquor, oosing as it were out of the mud, but very gently.

2. What kind of country it is where the springs are, whether hilly, &c.; and what plants grow near them ?—Generally a low ground, yet very full of collicular eminences, and various risings, to distinguish it from being all meadow. We have also a peculiar sort of ground in this county and some adjacent parts, which we call mosses, they are a kind of moorish boggy ground, very stringy and fat: which serves us very well for turfs, cut out like great bricks and dried in the sun. In these mosses is found much of that wood we call fir-wood, which serves the country people for candles, fuel, and sometimes for small timber uses. They generally seem to be places undermined by some subterraneous streams; or by the dissolution of some matter that made them equal with the rest of the ground formerly: In which conjecture I am confirmed by this, that near a place of my Lord Cholmondeley's, called Bilkely, about nine or ten years since, not far from one of these mosses, without any earthquake, a piece of ground about 30 yards over fell in, and drew in great oaks growing on it, which hung first with part of their heads out, afterwards suddenly sunk down into the ground, so as to become invisible: Out of which pit they drew brine with a pitcher tied to a cart-rope, but could then find no bottom with the ropes they had there: The pit has been since filled up with water, and now does not taste salt, but a little brackish, a very small rindlet passing through it. The nearest salt-springs to this place are at Dartwich about three miles from it. We have some hills, but not large, near our springs; which generally lie all along the river Weever, as Hankillow, Hatherton, Osterson, Bartherton; Nantwich, Weever, Leftwich, Northwich; yet there is an appearance of the same

vein at Middlewich nearer the river Dane than Weever; which notwithstanding seems not to be out of the line of the Weeverish stream; and these lie all near brooks, and in meadowish grounds. As to plants, I could observe no singularity at all; for, where the salt reaches the surface, it frets away all, and upon the turf near the old decayed pits grows the very same as in the remotest place of the meadow; only where the turf is fretted away, rushes maintain their station longest; yet they grow also in other moist grounds, so that they are no friends to the salt springs, but I perceive they resist them best.

3. Whether there be any hot springs near the salt ones? And whether the water of the salt springs be hotter or cooler than other spring water?—The water of the salt springs here is very cold at the bottom of the pit, so that when the briners cleanse the pit, they cannot abide in above half an hour, and in that time they drink much strong liquor. There are no hot springs (that I can hear of) nearer us than Buxton-well, which is about 30 miles distant.

4. Whether any shells are found about those springs, and what is the kind of earth?—I cannot hear of any shells dug up, though of late several new brine-springs have been found by sinking deep pits; yet no one knows of any shells, but rather a blackish slutch mixed with the sand, which infects the whole spring (like the scuttle-fish) black, when it is stirred, else the water runs very clear.

5. How strong the water is of salt?—Springs are rich or poor in a double sense; for a spring may be rich in salt, but poor in the quantity of brine it affords. Thus they have a rich brine in their chief pit at Middlewich, which yields a full fourth part of salt, like the rich Burgundian springs mentioned in Kircher's Mundus Subterraneus; yet this is so thrifty of its brine, that the inhabitants are limited to their proportions out of it, and their quantity is supplied out of pits that afford a weaker brine. Our pit at Nantwich yields but a sixth part; but then it is so plentiful a spring, that as they seldom make salt in above six houses at a time; this pit is judged sufficient to supply them all: besides that such quick use of it extremely strengthens the brine, perhaps to a degree little less than that of Middlewich pit: For I have found myself, that a quart of brine, when the pit has been drawn off three or four days first, to supply five or six wich-houses, has yielded an ounce and a half more of salt than at another time, when it has had a rest of a week or thereabout. But I conclude that the nearest conjecture is, that it yields one pound of salt for six pounds of brine.

On March 8, 1668, I weighed two pounds of distilled water in a narrow mouthed glass bottle, that I might make an exact mark for a quart. This bottle being filled with our brine to the very same mark, weighed (besides the tare of the bottle) two pounds three ounces and five drachms. This was taken up, when the wich-houses had only begun to work, so that the pit was but little drawn. I filled up the bottle with the same brine, and it weighed just three drachms more. This brine being boiled away without any addition or clarification, made five ounces and two drachms of salt. Five days after, when the pit had been drawn all that while for the working of the wich-houses, viz. March 13, the same bottle filled to the quart mark aforesaid with brine then taken up, weighed, beside the bottle, two pound four ounces and one drachm: the same time the bottle, filled as in the former experiment, weighed just two pounds and a half, which is three drachms more than the quart mark before; which boiled into salt made six ounces six drachms and two scruples, exceeding the former quantity of salt by one ounce four drachms and two scruples, though the brine exceeded the former in weight but four drachms. By this trial also I confuted a tradition of the briners, that the brine is strongest at spring-tides; for March 8, aforesaid, was only one day past the full, and then the brine was weaker than it was the 13th day, when it was six days past the full. So that I conclude, there could be no other reason than that the much drawing makes way for the salt-springs to come the quicker, and allows the less time for the admission of fresh springs.

6. What is the manner of their work? or what time of boiling the salt-water? Whether they use any peculiar thing to make it granulate, and if so, what that is?—Their manner of working is this: They have formerly boiled their brine in six leaden pans with wood-fire; upon which account they all claim their interest in the pit by the name of so many six leads walling; by which they each know their proportion; but in the memory of many alive they changed their six leads into four iron-pans, something better than a yard square, and about six inches deep, still fitting the content of these to that of the six leads: and of late many have changed the four iron-pans into two greater; and some wall but in one: But still the rulers gauge it to their old proportions.

They use for their fuel pit-coals, brought out of Staffordshire. These pans are set upon iron-bars, bricked in very close. They first fill their pans with brine out of the pit: which comes to them in several wooden gutters: then they put into their pans amongst the brine a certain mixture, made of about 20 gallons of brine, and two quarts of calves, cows, and chiefly sheep's blood. Of this mixture they put about two quarts into a pan that holds about 360 quarts of brine; this bloody brine at the first boiling of the pan, brings up a scum which they are careful to skim off; they continue their fire as quick as they can till half the brine be wasted, and this they call boiling upon the fresh. But when it is half boiled away, they fill their pans again with new brine out of the ship, (so they call a great cistern by their pan sides, into which their brine runs through the wooden gutters from the pump, that stands in the pit;) then they put into the pan two quarts of the mixture following: They take a quart of white of eggs, beat them with as much brine, till they are well broken; then mix them with 20 gallons of brine, as before was done with the blood; and thus that which they call the whites is made. As soon as this is in, they boil sharply till the second scum arise; then scum it off as before, and boil very gently till it corne; to procure which, when part of the brine is wasted, they put into each pan of the size aforesaid, about a quarter of a pint of the best and strongest ale they can get: this makes a momentary ebullition, which is soon over, and then they abate their fires, yet not so but that they keep it boiling all over, though gently; for the workmen say that if they boil fast here, it wastes their salt. After all their leach brine is in, they boil gently, till a kind of scum come on it like a thin ice; which is the first appearance of the salt: then that sinks and the brine every where gathers into cornes at the bottom to it, which they gently rake together with their loots: this they continue, till there is but very little brine left in the pan; then with their loots they take it up, the brine dropping from it, and throw it into their barrows, which are cases made with flat cleft wickers, in the shape almost of a sugar-loaf, the bottom uppermost. When the barrow is full, they let it stand so for an hour and a half in the trough, where it drains out all the leach brine, then they remove it into their hot-house behind their works, made there by two tunnels under their pans, carried back for that purpose. The leach brine that runs from the barrows they put into the next boiling, for it is to their advantage, being salt melted, and wanting only hardening.

This work is performed in two hours in the smaller pans, which are shallower, and generally boil their brine more away; wherefore their salt will last better, though it does not granulate so well, because when the brine is wasted, the fire and stirring breaks the cornes. But this salt weighs heavier and melts not so soon; and therefore is bought for many sales to a distance. But in the greater pans, which are usually deeper, they are above half an hour longer in boiling; but, because they take their salt out of their brine, and only harden it in their hot-house, it is apter to melt away in a moist air: yet of this sort of salt the larger the grain is, the longer it endures; and generally this is the better granulated and the clearer, though the other be the whiter. And I think it is rather the taking of the salt out of the brine before it be wasted, that causes the granulating of it, than the ale, to which the workmen impute it.

They never cover their pans at all, during the whole time of boiling. They have their houses like barns open up to the thatch with a cover-hole or two to vent the steam of the pans. Possibly tiles may do better, but nobody is yet so curious as to try, but the steam is such that I am confident no plaster will stick; and boards will warp, and their nails will rust so, as quickly to fret to pieces.

7. Whether the salt, made of these springs, be more or less apt to dissolve in the air than other salt? and whether it be as good to powder beef or other flesh with, as French salt?—This question I cannot well answer, in regard that French salt comes not to us, to compare the efficacy of the one with the other experimentally; but this I can assure for our salt, that with it both beef and bacon are very well preserved sweet and good a whole year together; and I do apprehend this salt to be rather more searching than French salt, because I have often observed that meat kept with this salt shall be more salt to the midst of it, than I have observed, when I have eaten powdered meat on ship-board, which was probably done with French salt, I then being on the south-side of England and in a Dutch vessel. It is certain that Cheshire sends yearly much bacon to London, which never yet had any mark of infamy set upon it; and hung-beef (which others call Martinmas beef) is as good and as frequent in Cheshire, as in any place; so that I conclude this salt is fully effectual for any use.

Explanation of the figures belonging to the Account of Salt-making, in Plate 10.

Fig. 4. This is the model of an iron pan of that proportion, when four are used in one house. a a, The ears to hang the pan by on the brick work. b, The several junctures of the iron plates riveted. *C C*, The breadth and length of the pan near four feet. *C* d, The depth of the sides of the pan, about six inches.—Fig. 5. a a, The hot-house between the wall and the chimney. b b, The two tunnels. *C C*, The chimney back, into which the two tunnels convey the smoke. d d d d, The four pans. *E*, The partition wall between the pans and the hot-house. f, The fire places. g g, The ash-holes. h, The hearth below. i i, The descent to the hearth.—Fig. 6. The back with its stale, with which they reach brine out of the ship to fill their pans with.—Fig. 7. a b, Several positions of the loots, with which they skim and gather the salt.—Fig. 8. a a, Two barrows newly filled with salt, set into the leach-trough to drip out the leach or leach-brine. b b, The salt heaped above the barrows, and patted down hard. *C*, The leach trough.—Fig. 9. A gutter, which they lay over from one pan to another, to run the brine into the farthest pans.

Extract of a Letter written to the Editor, from Plymouth, Nov. 2, 1669, by WILLIAM DURSTON, *M. D. concerning the Death of the big-breasted Woman, (noticed in N° 52.) together with what was observed in her Body. N° 53, p.* 1068.

Elizabeth Travers died on Thursday night, October 21. The next morning I sent for a surgeon, and some others to be present at the opening, and taking off her breasts; though we only took off the largest, which was the left, and having weighed it, we found it 64 pounds weight. Upon opening it, (which we did in several places) we could find neither water, nor cancerous humours, nor any thing vitious, more than the prodigious size; and the tubuli and parenchymous flesh were purely white and solid, and no other than what we see in the soundest breasts of women, or the best udders of other animals. She had lost her appetite and rest several weeks before, and made great complaints of her breasts from their excessive distension, and her whole body was exceedingly emaciated. I have sent you inclosed one measure, which was the breadth of her two breasts (as she was laid out on a table being dead;) I mean, from the further end of the one to the other; which you will find three feet two inches and a half; and another measure showing the dimension of the breasts longwise, viz. near four feet four inches; and a third, giving the dimension of the breadth, viz. three feet four inches and a half.

The right breast we took not off, but guess it might weigh 40 pounds. Some weeks since I began a salivation with her, which lessened her breasts in circumference some inches; but she proving not conformable, I durst not proceed to keep up the flux. But she was wonderfully revived afterwards for some time. She being weary of that course, I caused a caustic to be applied; upon which the eschar fell off, yet nothing issued out of the breast. Then I caused an incision-knife to be used, and made an incision two inches and a half deep (supposing the caustic had not wrought deep enough) but to no more purpose than the former.

An Account of some Books. N° 53, p. 1069.

I. Certain Philosophical Essays, and other Tracts, by the Honourable Robert Boyle, Fellow of the Royal Society. The second edition, enlarged. An. 1669.

This edition is chiefly increased by the addition of a very philosophical discourse on the absolute rest in bodies, wherein the noble author, with his usual modesty and acuteness, delivers his thoughts concerning the intestine motions of the particles of quiescent solids, and in so doing calls the absolute rest of bodies in question, by undertaking to prove, that some of those bodies, which we think have their parts most at rest, are not exempted from having internal motions in them; from which he thinks it probable, that in other bodies, whose solidity is confessed inferior, the component particles are not in a state of perfect rest.

II. Del' Movimento della Cometa, apparsa il mese di Dicembre 1664. Da Pietro Maria Mutoli, in Pisa, in 4to.

This author principally discourses of the motion and place of that comet, and how its odd appearances may be solved. He works not by the way of taking the several altitudes from the horizon, but by observing the position of the comet among some neighbouring fixed stars; for doing which, he says he employed only a simple thread stretched out by an arch, to make it evident whether this comet had a sensible parallax, or not. From whence he concludes that this comet was above the moon, as it had no parallax that was perceivable.

III. Erasmi Bartholini* de Cometis An. 1664, et 1665, Opusculum; Ex Observationibus Hafniæ habitis adornatum. Hafniæ, in 4to.

The author having first intimated that the more narrowly this subject about comets has been searched into, the farther have intelligent men receded from the opinion of the peripatetics concerning them; he acquaints the reader what instruments he used in making observations; among which was a quadrant of 1½ foot radius; what conveniency he had as to the place of observation; what observations he made, viz. An. 1664, December 23, 24, 25, 26, 27, 28, 29, 30, 31, and An. 1665, January 1, 3, 4, 5, 6, 7, 9, 12, 24, 31, and February 7. Also, how he found the longitude and latitude of this comet, viz. by observing its distances from two fixed stars; and having found those, how thence he found its right ascensions and declinations for every day: Besides, how he found the motion of the comet in its orb, and the place of intersection and the angle of inclination with the ecliptic. To which he adds his considerations about the place of the comet, and the parallaxes; vindicating here the noble Tycho from the accusation of Riccioli in Almag. Novo: Concluding all with an investigation of the causes of comets; where he examines whether comets be coeval with the world, or produced anew; and, if the latter, how: Insinuating that, though he esteems the latter comet to be different from the former, yet, admitting the

* Erasmus Bartholin was successively professor of mathematics and philosophy at Copenhagen; and attained the rank of counsellor of state. He died in 1698, at 73 years of age. He was author of some other mathematical and physical works, besides that mentioned in this article; as, Experimenta Chrystalli Islandici, 1670, in 4to, and De Aere Hafniensi, 1679, in 8vo.

Cartesian hypothesis, we may without any difficulty maintain that it was the same with the first.

IV. Sylva* et Pomona, by John Evelyn, Esq. F.R.S. Reprinted in folio. London.

This is the second edition of this book, very much improved and enlarged. It was the first that was written and published by the express order of the Royal Society.

In Sylva our author provides and directs for sound timber, and store of the best fuel in forests, woods, and groves. Timber is the strength and walls of this kingdom; and is serviceable for many domestic uses and curious utensils; and to advance the riches and accommodations, not only of our own country, but also of all foreign parts, as far as may mutually oblige each other by navigation and commerce. In Pomona he gives an account of fruit-trees. He has also given an instructive touch for vineyards in England (see Number 15); and for the sake of his own country has furnished us with accurate translations of the French Gardener. Also of the best writers of architecture, and of the idea of the perfection of painting (see Number 39.)

Of his own collections, he has given us an elegant history of sculpture, and of engraving in copper, and the curious arts thereunto belonging: In which the most excellent painters of this age may see themselves either duly celebrated or directed for the best, and assisted, or at least encouraged.

And, besides other treatises which are anonymous, he has lately made two considerable tracts: In one of which, for a caution to all future ages, and to denote the general aptness of mankind to be deluded and deceived, he has published a history of the three late famous impostures; the first, merely casual and innocent, in Padre Ottomanno; the second, bold and impudent, in Mahomet Bei; the third, a confident cheat, in Sabatai Sevi, the Jews' counterfeit Messiah: In the other tract, public employment and an active life are preferred to solitude.

A Letter from an Englishman at Paris to a Member of the Royal Society, concerning some Transactions there, relating to the Experiment of the Transfusion of Blood. *N° 54, p.* 1075.

Monsieur Denys, a physician, had been questioned before the Lieutenant Criminal here on account of the death of his patient (a man that had been stark mad for several years) who had expired under his hands, while he was trans-

* A new and splendid edition of the Sylva, enriched by many useful and ingenious notes, was published in 1786, by Dr. A. Hunter of York.

fusing blood into him according to the new experiment. The operation had been twice performed with good success; the patient having had thereupon a good interval of two months after the first, and all hopes of a longer after the second, had it not been for the debauches in wine and brandy, that he fell to soon after the operation. He was a Breton by birth, and the origin of his madness, love. What Mr. Denys's advocate very much gloried in, was, that (besides that the experiment had been practised with good, at least with no ill success, in England, Germany, Italy, Holland, &c. and defended in theses in almost all the universities of France) there were two persons, a man and a woman, present in the audience, who had received a benefit to admiration from the experiment, after they had been abandoned by physicians.

In justifying the introduction and use of new experiments, Mr. Denys's advocate said, that the most precious life to the state, (viz. that of his most Christian Majesty) had been saved by the administration of a lately invented emetic. This advocate was the son of Monsieur le Premier President de la Moignon. He was not long since in our court, and is I perceive well known to it, and infinitely satisfied with the civilities he had received from several persons there. Though this was his first action, yet his performance was a master-piece. The pleading for the widow plaintiff will be on Thursday next; but any odds would be laid on the defendant's side; though some partial men here are more than suspected to set on the widow.

Paris, Nov. 30, 1669.

Extract of a Letter from Dr. Durston, *dated Plymouth, Nov.* 28, 1669, *giving an Account why the late big-breasted Woman was not opened after her Death. N° 54, p.* 1077.

The reason here assigned was that a relative of the deceased would not allow the body to be opened.

An Appendix to the Discourse concerning the Salt-work, published in Number 53. *Communicated by the same Dr.* Jackson, *in a Letter, dated November* 20, 1669. *N° 54, p.* 1077.

Q. 1. Whether those salt springs yield less water and more salt, in great droughts, than in wet seasons? A. Our springs do not sensibly alter in their decrease or increase in either dry or wet seasons; for, being plentiful springs, we have always the pit full: Only this is observed by the briners, that they make more salt with the same quantity of brine in dry than in wet seasons; and more salt of the same quantity of brine at the full of the moon than at any other time.

2. How long before the spring, or in the spring, it may be, before the fountains break out into their fullest sources? A. It is not observable at all in our salt springs that the brine rises more plentifully in the spring time, than at any other season of the year: neither is there any sensible difference in the quickness of the sources as to the times of the day.

3. How much water the spring yields daily, or in an hour ordinarily, or in great droughts? Ans. Our pit is about five yards square, and of so plentiful a source, that I believe it cannot be guessed; and the rather because it seems not to run much when it is permitted to come at its full gauge, where a vent through the bank into the river is; but being drawn much, so as to sink it below its usual gauge, it so plentifully lets in, that it will serve all the houses in the town to work, without falling much lower than a yard or two at most: so that I believe, that, when it is full, its own weight balances much the influx of the springs, which are much quicker in a low pit than a full one.

4. At what distance the two richest springs of Nantwich and Droitwich are from the sea? Ans. That of Nantwich is from the sea about 30 miles. Droitwich, being in Worcestershire, is not known to me.

5. How near the foot of a hill is to those springs; and what height the next hill is of? Ans. The nearest hill to our springs is about seven miles distant from them: the hill steeper, but not much higher, than Highgate hill.

6. Wherein consist the distinctions of those sorts of salt, which are called cats of salt, and loaves of salt? Ans. As white salt is that mentioned in my former discourse, and gray salt the sweepings of such as is constantly shed and scattered about on the floor without taking much of the dirt; so cats of salt are only made of the worst of salt, when yet wettish from the pans; moulded and intermixed with interspersed cummin-seed and ashes, and so baked into a hard lump in the mouths of their ovens. The use of these is only for pigeon houses: But loaves of salt are the finest of all for table use. No difference in the boiling of these from the common way of the fine salt; but in the making up some care is used: for first they cut their barrows, they intend for salt-loaves, with a long slit from top to bottom, equally on both sides; then they tie both sides together with cords; then fill this barrow with salt boiled as usual, but in the filling are careful to ram down the salt with the end of some wooden bar, continuing this, till the barrow be filled to their minds; then place it speedily in their hot-house, and there let it stand all the time of their walling: wherefore they prepare for these loaves at the beginning of their work, that they may have all the benefit of their hot-houses; and when these begin to

slack, they take out the loaves, and untie the cords that fastened the bar, so that both sides of the same may easily open without breaking the loaf. Then they take the loaf, and bake it in an oven after the bread has been drawn. This they repeat till it be baked firm enough.

I must not omit telling you, that all the ground, where salt or brine is spilt, is, when dug up, excellent muck for grazing ground; and even the bricks, that are thoroughly tinged with it, are very good muck, and will dissolve with other muck, and fertilize land considerably (especially grazing ground) for at least four years.

On the Quicksilver Mines in Friuli. By Dr. EDWARD BROWN. N° 54, p. 1080.

The town of Idria, in the county of Goritia and Province of Friuli, is seated low, and encompassed with hills on all sides. A river of the same name runs by it, which I found small and shallow at the time when I was there; though upon plentiful rain it proves sufficient to convey down the fir-trees and other wood, required in working the mines, and for fuel: and to this end there is a work of piles made sloping athwart the river to stop the trees, which are cut down, and cast into the river above this place. What is chiefly considerable in this town, are the quicksilver mines. The entrance into these mines is not high, or upon a hill, as in many other mines; but in the town itself, whereby they are somewhat the more troubled with water, against which they are provided with many excellent engines and devices. The deepest part of the mine from the entrance, is between 120 and 130 fathoms. They make two sorts of the quicksilver: The one called Iungfraw, that is, virgin quicksilver; the other, plain quicksilver. Virgin mercury they call that which discovers itself without the help of fire; and is either plainly to be seen in the ore, or falls down in little drops in the mine, and sometimes streams out in good quantity; as about seven years ago it ran out of the earth, at first in a stream as small as a thread, but afterwards as thick as a packthread; but ceased in three or four days. That also is accounted virgin quicksilver, which having no need to pass the fire, is separated by water, first in a sieve, and afterwards in a long trough, having very small holes at one end. So that there are two sorts of virgin mercury; the one, running out and discovering itself without labour, the other requiring some method of extraction and separation, though not so high as by fire. Plain quicksilver they name that, which is not at first perceived by the eye, or falls from the ore, but is forced out by fire. And this they obtain out of the ore, or out of the cinnaber of mercury, which is dug out of this mine. The ore of this mine is of a dark colour, mixed with red. But the best is a hard stone; which they commit not presently to the operation of the fire, but powder it grossly, and work it by the sieve; that so if any virgin quicksilver be found in it, it may be separated in this manner, and what doth not pass the sieve, may be separated by fire in iron furnaces: fifty of them in a fire. The quicksilver ore of this mine is the richest of all ores I have yet seen; for ordinarily it contains in it half quicksilver, or in two parts of ore, one part of quicksilver, and sometimes in three parts of ore, two parts of quicksilver.

I went into the mine by the pit of St. Agatha, and came up again by that of St. Barbara, descending and ascending by ladders. I ascended at one of 639 staves, or 89 fathoms. This mine, I was informed, has been wrought 200 years, about the same space of time with Newsol mine, but comes much short in time of the silver mine at Schemnitz; and much shorter yet of the notable lead mines in Upper Carinthia. Some hundreds of men are employed about this mine of Idria; of which the chief officers are the prefect, the comptroller and the judge. I heard no complaint of the damps of this mine, as I have heard of divers others; yet the workmen are sufficiently injured without them: for, though they be not suddenly suffocated, yet the mercury getting into their bodies, they are languishingly destroyed by it.

In a laboratory where the quicksilver is separated by fire, I saw a heap of 16,000 retorts of iron; every one of which costs a crown at the best hand from the iron furnaces in Carinthia. There are 800 retorts, and as many recipients, employed together in drawing over the quicksilver in 16 furnaces; 50 in each furnace, 25 of a side, 12 above, and 13 below, on each side. June 12, when I was there, they carried out 40 saumes of quicksilver into foreign parts, each saume containing 315 pound weight, to the value of 400 ducats of gold. Though the conveyance be not easy, yet some is sent as far as Cremnitz in Hungary, for the use of the gold mines; and very much carried away southward. In the castle I saw 3000 saumes of quicksilver together in barrels; the quicksilver being first made up in double leather: and in another house as much rich ore as can be distilled in two years, except they have great plenty of rain to bring down the wood; but, the hills being high about them, it snows at the tops of them oftener than it rains.

The countries through which I passed are singularly well wooded, and well stored with fair trees, wherein, beside such as grow with us in England, are stately firs, larches, pines, pinasters, piceas, and that nobly crisped and well grained kind of acer, whereof viols and violins are made: of which there is also plenty in the country of Saltzburg and Carinthia. The way to this place I found difficult; for, travelling from the borders of Croatia by Lovitch, I was forced to pass over great mountains; and, coming from it, I passed over Swartzenberg, or the Black mountain, from whence I descended 10 miles in a rocky country, and far more stony than the Crau or Campus Lapidosus in Provence; and so to Aidoschini and Goritia, and leaving the Scalvonian behind, entered into the Lingua Fullana, and so on to Palma Nova in Friuli.

On the Uncommon Lake, called the Zirchnitzer Sea, in Carniola. By the same. N° 54, p. 1083.

Having crossed the river Drave, and passed mount Luibel in the Carnic Alps, by that noble passage, cut through the rocks, and vaulted like that of Pausilippo near Naples, I had a desire to take a view of the lake of Zirchnitz. With this view I went to Crainburg on the river Save, and so to Labach, the chief city of Carniola; from whence I continued my journey in Carniola between the hills and a great marsh, till I came to Brounizza, two leagues from whence and beyond the hills is seated the said lake, receiving that name from Zirchnitz, a town of about 300 houses.

This lake is near two German miles long and one broad. On the south side of it lies a great forest, wherein are many deer, and wild boars, wolves and bears. On the north side the country is flat; but the whole valley is encompassed with hills at some distance from it. This lake is well filled with water for the greatest part of the year; but in the month of June it sinks under ground, not only by percolation or falling through the pores of the earth, but retires under ground, through many great holes at the bottom of it: and in the month of September it returns by the same, and so in a very short time fills up the valley again. As the time of the water's descent is short, especially when the lake grows lower, and has for a while shown some abatement, so the ascent and return is speedy; for at these holes it mounts with such violence, that it springs out of the ground to the height of a pike, and soon covers the track of earth again. This piece of ground, in the time of the retirement and absence of the water, is not unfruitful; but by a speedy and plentiful production of grass, yields not only a present sustenance for the beasts of the field, but a good provision of hay for the cattle in the winter. Nor have the inhabitants thereabout only the benefit of the ground by these commodities, but also the recreation and profit by hunting. For at the time of the waters absence, hares, deers, boars, and other animals come into it, from the neighbouring forest and country. The lake is not only filled with water, but every year well stored with fish. The Prince of Eckenberg is lord of it and of much country thereabout: but upon restoring the waters all have liberty to fish; and the fishermen, standing up to the waist at the holes before-mentioned, intercept the passage of the fish, and take a very great number of them, which otherwise would be secure for some months under the earth, and not fail to return in September. The fish of this lake have a closer habitation than those of any other I know; for they pass some months under the earth, and a good part of the winter under ice. But beside these holes at the bottom of the lake, there are also divers caverns and deep places in the country of Carniola, even where there is no water; like those in the Peak Country, and at Eldenhole in England.

An Account of some Books. N° 54, *p.* 1086.

I. Mechanica, sive de Motu Tractatus Geometricus. Auth. Joh. Wallis, SS. Th. D. et Geom. Profess. Saviliano, &c. 1670, in 4to.

In the first part of this work are delivered the general laws of motion. Secondly, the descent of heavy bodies by gravity. Thirdly, the doctrine of the lever or balance, containing the fundamental principles of all statics; in which the author explains the geometrical considerations requisite in making both exact common scales, and the Roman statera. On this part of the lever depends the whole doctrine of the centre of gravity, and the calculation thereof; showing how by calculation to assign it, in all sorts of lines, surfaces, solids, as well such as are bounded, or take their rise from curved lines, as those that are bounded by straight lines and plains.

And from the general principles here laid down in his third part (which is to follow) he derives the doctrine of the lever, the pulley, the screw, the axis in peritrochio, or several sorts of wheel-work; and other such mechanical engines derived from these. As likewise the doctrine of percussion (on which depends that of the cuneus or wedge, with many other speculations of a like kind): and that of resilition or rebounding, which (as appears by a short specimen formerly printed in Numb. 43 of these Tracts) he derives from a repercussion, either of some other body in motion which it meets with, or from the elastic force or spring in one or both of the meeting bodies, which being compressed by the collision, endeavour to restore itself by repelling these bodies one or both ways.

II. Nathan Highmori* de Hysterica et Hypochondriaca Passione, Responsio Epist. ad Doct. Willis. Lond. 1670, 4to.

* See p. 271 of this vol. of our Abridgement.

Dr. Willis, in his Pathologia Cerebri, having undertaken to confute the causes of the hysteric and hypochondriac passions, as assigned by this author in his Exercitations, published in 1660, Dr. Highmore thought himself obliged to write this defence.

The controversy as to the former of these disorders consists in this, whether the cause of it is to be referred to the genus nervosum, and it be primarily a convulsive symptom, depending on the brain and the nerves; or whether it ought to be imputed to the blood rushing too impetuously into and stuffing up the lungs? Dr. Willis asserts the former, and objects against the latter, of which the vindication is undertaken in this epistle; wherein it is considered among other things: 1. That the hysteric passion is not always accompanied with convulsions; where also the existence of the succus nutritius (on which Dr. Willis's doctrine seems to depend) is called in question, and several arguments alleged to the contrary. 2. That if the succus nutritius in the nerves be by a violent explosion dilated, and that by means of fixed salts mixed in the nerves with acid ones (according to Dr. Willis); the same may happen upon the concurrence and conflict of the like salts in the blood.

Concerning the hypochondriac passion, the question between these two physicians is, whether the first and chief cause of that distemper be the weakness and laxness of the tone of the stomach and its fibres, whereby a vicious chyle and blood is generated, that causes such a fermentation, upon which ensue trembling and palpitations of the heart, swoonings, &c. This is affirmed, and here further asserted by our author, but denied by Dr. Willis; concerning which, the reader will find the best satisfaction in perusing the writings of both parties.

III. Nouvelles Experiences sur la Vipere, par M. Charas. Paris, 1669, 8vo.

The author of this curious book, having first taken notice of divers observables in the dissection of vipers, and among them of the salivary glands, he discovered in them as well as in other animals, and those accompanied with lymphatic vessels, passing into a greater vessel running along and under the said glands, and discharging itself into the vesicle of the gingiva, and carrying with it the salival liquor, which he makes to be the same with that yellow water in the bag, hitherto esteemed venomous, but by him reputed a mere harmless saliva.* He attempts to prove, that the bitings of vipers, at least of such as are in France, are indeed venomous, and proved actually mortal: he alleges many

* By referring to the observations of Redi and the notes thereupon (at p. 58 of this vol. of the Abridgement) it will be seen, that Mons. Charas's assertion concerning the innoxiousness of the yellow liquor contained in the vesicle or bag above-mentioned, is extremely erroneous.

experiments, made by himself in the presence of several physicians and others, in proof of this assertion; in the recitation of which he observes many remarkable phænomena, seen in the animals bitten by vipers, both without and also within them when dead and opened; particularly, that he found all their vitals and viscera fresh and in a good state, but the blood, in all of them that were opened, either coagulated already and blackish, or tending towards coagulation.

2. To confute the opinion of those (and particularly of the famous Italian philosopher Redi) who assert that the venom of these animals resides in the yellow liquor contained in the bag about the vipers' teeth; whereas this author will have it to be in their vexed and enraged spirits, which he thinks he has sufficiently proved by wounding several animals with some of the largest teeth of vipers, pulled out, and letting into wounds thus made, and rubbing with that reputed poisonous liquor of the bag; whereupon no ill effects at all have followed. Which he confirms by another trial, wherein, holding the jaws of a viper, and then thrusting its teeth into the flesh of a living animal, and letting the juice of the bag into the wound, no ill consequence appeared,* considering that the angred spirits of the viper in that forced and restrained posture were kept from passing abroad; for the emission of which he supposes the freedom of the animal is required.

3. To recommend, among divers other antidotes for the bitings of vipers, the volatile salt made of them ; the virtues of which he praises exceedingly, alleging the example of a person, who being bitten by a viper, could be saved by no other means but by several doses of this volatile salt; the preparation of which he describes at large.†

IV. Athanasii Kircheri Ars Magna Sciendi sive Combinatoria. Amstelodami, 1669, in fol.

In this voluminous work on the art of reasoning, the author first shows the theory and the rules, in the first five books, and then, in the other six, he applies these rules to practice by examples, relating to the several arts and sciences.

V. Le Systeme General de la Philosophie, par Francois Bayle, D. M. A. Thoulouze, 1669, in fol.

This small tract, consisting only of four sheets, may serve to initiate those that desire to be acquainted with the sum and import of the Cartesian philoso-

* All this is contradicted by the late observations of Fontana, mentioned in the notes at p. 58 of this vol. Charas seems (as Haller has remarked) to have employed sickly and exhausted vipers in his experiments.

† The volatile salt obtained from the flesh and bones of the viper, differs not from that procured from the flesh and bones of other animals. Such volatile salts are, however, good antidotes in these cases.

phy. It delivers these heads: the metaphysics of that famous philosopher; his logic; principles of natural philosophy in general; his doctrine concerning the productions made in the bowels of the earth, concerning meteors; sensible qualities, plants, animals, man, human passions, and ethics.

VI. Theodori Kerckringii,* D. M. Spicilegium Anatomicum, continens Observ. Anatom. rarior. nec non Osteogeniam Fœtuum. Amstelodami, 1670, 4to.

This collection consists of several uncommon and very considerable remarks, caused by the author's own observation; a Dutch stiver swallowed down, and by closing the pylorus of the stomach, killing the patient in ten days; on the contrary, a small brass coin being swallowed, was after a month's time voided by purges, and the patient saved, the coin being so worn in the stomach, that the same hardly appeared; also of a tumour on the back, resembling a sack filled with corn, formed there by the force of imagination; examples of superfœtation and ambiguous birth; of animals bred in the ear, and worms come out of the nose; of two nipples in one breast; of divers very odd monsters; of infants born with teeth; of a periodical spitting of blood; of stones growing upon the wind pipe, in the brain, and the heart, and killing the patients; of a double vena cava, of a treble ductus thoracicus; of four spermatic arteries found without spermatic veins; of a portion of a secundine, voided uncorrupted and innoxiously, four months after the production of the child; of a woman frighted by the prediction made by a beggar, of the day of her death, and dying on that very day; of a very crooked man, not above forty years of age, made straight again by purging away tough humours besieging the muscles; of a boy, and of several sheep destitute of brains, &c. &c.

The other part of this book treats of the gradual and successive growth of the bones in a fœtus; for the better observation of which, the author affirms that he has by him skeletons from the second month after the conception, to the very ninth month, assuring the reader, that he delivers nothing but what he knows by his own clear inspection; which he performs in such a manner,

* This writer on chemical and anatomical subjects was born at Hamburgh in the 17th century. He was educated in Holland, where he studied physic, and for some years followed that profession at Amsterdam; but at length relinquished it, and turned his attention to politics; in consequence of which he was appointed resident or envoy at Hamburgh, on the part of the grand duke of Florence. In addition to the work above-mentioned, we have by him Anthropogeniæ Ichnographia, 1671, 4to. and a Commentary on Basil Valentine's Currus Triumphalis Antimonii, published in the same year. He is said to have invented a method of preserving anatomical preparations by embalming them with a solution of amber. The fidelity of some of his pretended discoveries, and particularly of those which relate to the existence of a spherical ovum in the human subject two or three days after conception, has been justly suspected; and he has moreover been accused of plagiarism. Some other anecdotes recorded of Kerckringius we would hope not to be true.

that he only relates the more rare particulars, wherein the fœtuses differ from adult bodies. He deduces the increase of the bones through their several intervals of times, and gives an account how they are constituted in the first two months, in which there hardly appears any thing of bones distinguishable; then what of them, about the third, fourth, and the rest of the months, is ordinarily changed, added, or taken away, at least in the fœtus, of these climates. The sum is, that there is a transmutation of membranous parts into cartilaginous, and from them into bony ones; performed by nature with such silent steps, that the most quicksighted and the most patient eye shall never see it doing, though it may evidently see it done.

An Account of such of the more remarkable Celestial Phænomena of the Year 1670, *as will be conspicuous in the English Horizon; written by Mr.* JOHN FLAMSTEED,* *Nov.* 4, 1669. *Addressed to Lord Viscount* BROUNKER, *P. R. S. N° 55, p.* 1099.

This paper contains a calculation and prediction of a solar eclipse, and of some occultations of fixed stars by the moon, made from Street's tables, and for the town of Derby, the then residence of Mr. Flamsteed.

* Mr. John Flamsteed, the first astronomer royal of England, was born at Derby in 1646, and was educated at the grammar school of that town. When very young he showed a disposition for mathematical learning, particularly astronomy, which became his chief study and amusement after quitting that school, and by the help of Sacrobosco's book de Sphera, which had fallen in his way, the foundation of that knowledge was laid for which he became afterwards so justly famous. In 1666 he calculated a solar eclipse; which being shown to a gentleman skilled in the science, he encouraged young Flamsteed in his pursuits, and lent him several books on astronomy. Of these he made so good use, that in 1669 he sent to the Royal Society calculations of eclipses and occultations that were to happen in (1670) the year following. This communication procured him the correspondence of some learned members of that body; and the year following he paid them a visit in London, which brought him acquainted with several others, particularly Sir Jonas Moore, then surveyor general of the ordnance, by whose interest Mr. Flamsteed experienced great assistance and encouragement, and afterwards the honour of Astronomer Royal. On his return from London he visited Cambridge, where he entered himself of Jesus College, and made an acquaintance with Dr. Barrow, Mr. Isaac Newton, and other learned men there. In 1674 he was appointed the King's astronomer. The year following the Royal Observatory in Greenwich Park was begun; where, when completed, Mr. Flamsteed resided, making astronomical observations, till the time of his death in 1719, at 73 years of age. Mr. Flamsteed had entered into holy orders, but all the church preferment he received, was the small living of Burslow in Surry. Many of Mr. Flamsteed's communications are printed in several volumes of the Phil. Trans. And in the second volume of Sir Jonas Moore's System of Mathematics, is a tract of Mr. Flamsteed's, on the Doctrine of the Sphere. He was author of several other pieces, besides his great work, the Historia Cœlestis Britannica, in three volumes folio, published only in 1725.

Observations concerning the Baroscope and Thermoscope. By Dr. I. Beal, at Yeovil in Somerset. N° 55, p. 1113.*

During the latter part of the month of December, 1669, the barometer stood very high, being about 30½ inches all that time.

December 26, 1669, in the morning, the weather was colder than ever I found it, since I could take it by the measure of a thermometer, that is, since these five or six years.

To note this degree of cold more particularly, I must acquaint you, that in my staunch thermometer on that day the liquor was at 3½ inches. This morning, December 29, and one or two other mornings, it was at 3¾ inches. Most other times of these cold days, morning and evening, it was at the height of 4 inches; in ordinary brisk frosts it is at 7 inches. Yet here I must observe, that sometimes the frost dissolves, when it is at the 7th figure, and sometimes I find it at the 8th figure in a smart frost. It is warm May weather, when at the 10th figure, and not much above the 12th figure in the hottest weather of June, July and August. It is remarkable, that at the 7th inch, and sometimes the 8th in my thermometer, it should abide freezing, and the frost increase, till the liquor descended 4½ inches; and yet, that it should not ascend from the 8th inch more than 4½ inches in our hottest summer, being hung in the same place within 18 inches of the glass-window, facing the north-west, and in a little writing-room, in the second row of buildings. But now I am strongly persuaded, that the degrees of heat and cold are not exactly indicated by the inclosed spirit of wine: for, when the snow melted, and the frost was first dissolved without sun-shine, the liquor was not above the height of 5½ inches. Possibly it retains some part of the cold a while after the ambient air becomes more tepid.

* Doctor John Beal, an English divine, was a very early and useful member of the Royal Society, having been elected a fellow in January 1663. Dr. Beal was born of a good family in Herefordshire, in 1603, and was educated successively at Worcester school, Eton college, and King's college, Cambridge; after which he spent some time on his travels abroad, about 1636. He was very useful in promoting the apple orchards in his native country, and was author of a small tract on that subject, entitled, "Herefordshire Orchards a Pattern for England." He resided chiefly at Hereford till 1660, when he became rector of Yeovil in Somersetshire, where he resided till his death, which happened in 1683, at 80 years of age. Dr. Beal was made D. D. and King's chaplain in 1665. Several of his papers were printed in the Phil. Trans. and elsewhere. He was a man of excellent parts, extensive learning, and great public spirit.

Observations on the Baroscope and Thermoscope. By Dr. WALLIS, in a Letter of January 7, 1669-70. N° 55, p. 1116.

Whereas I formerly observed, that in hot weather the quicksilver in the baroscope used to rise observably, especially in sun-shine and the heat of the day; which might seem to argue the air to be thereby made heavier; I now find, having kept the same barometer for the space of five years unaltered, the case for these two years last past, to be somewhat otherwise: and that in hot sun-shiny weather the quicksilver rather subsides a little; and in extreme cold and frosty weather it rises. Which makes me judge the cause of these contrary observations to be this, viz. That the quicksilver, at its first putting into the tube or baroscope, was not perfectly cleansed from the air: which latent particles of air, by the external heat, were so much expanded as to give a greater bulk to the same quantity of quicksilver, with which it was mingled, and consequently to make it rise somewhat higher, and on the recess of the external heat, the spring again slackening, the air being more compressed, suffered the quicksilver to be again contracted into its former lesser dimensions, and so to become heavier, and not to rise so high as before, when it was hotter. But now, the air freed from its entanglement with the quicksilver, being got up into the void part of the tube above the quicksilver, acts contrarywise, that is, when it is by heat expanded, it presses downward on the quicksilver, and a little depresses it; and on the contrary, when by frost or very cold weather this air is contracted, the quicksilver, freed from that pressure, rises a little.

My thermoscope, or sealed weather-glass, has this last frost been much lower than I ever have known it on five years constant observation; which proceeds partly from the extremity of the cold more than ordinary, and partly from the inclosed liquor (being spirit of wine tinged with cochineal,) growing less spirituous. It was first made in December 1664. In the months of January and of February following, we had very smart frosts, more cold than ordinary; when yet the lowest mark to which the liquor did subside in very hard frosts and very cold wind, was at 12¼ inches: the height in summer following, 1665, was usually at 20, 21, 22, or thereabout; but in some few very hot days, at 25, 26, 26½; the whole height of the small cylindric glass, whose cavity was about ⅛ of an inch diameter, being about 20 inches; besides a small spherical bowl at the top, of about ¾ of an inch diameter; and a bowl at the bottom, which contained the liquor of about two inches diameter: the space above the liquor being at the first composure of it, void of air, save what it had out of the liquor, which being warm at the first putting in, filled the whole cavity while the glass was hermetically sealed.

The winter following, the liquor seemed to remain much about the same temper as in the next foregoing. For in December, January, and February, we had at $14\frac{1}{2}$ frost certain; sometimes at 15 or higher, and the lowest was $12\frac{3}{4}$. The height in the following summer, 1666, was usually about 19, 20, 21; the highest of all at 25.

About the end of December 1666, and the beginning of January following, it was in hard frosty weather at 12, 11, and once at $10\frac{1}{2}$, the weather being very cold, and the liquor becoming somewhat less spirituous, having evaporated some of its more subtile parts into the void cavity; and it was frost certain that winter about $13\frac{1}{2}$, sometimes at 14, or $14\frac{1}{2}$. The usual height in summer following, 1667, was about 19, 20, 21, and the highest at $24\frac{1}{2}$.

The winter following, it was scarce certain frost at 13; but yet sometimes at 14 or a little higher: the lowest, to which it descended that winter, was at 12. And the following summer, 1668, usually about 18, 19, 20, the highest of all at 22.

The next winter it was frost certain about $12\frac{1}{2}$; but sometimes at 13 or higher, the lowest of all at $10\frac{1}{4}$. And in the summer following, 1669, the highest of all not much above 20.

But now this Christmas, 1669, though I find it to be frost certain about $12\frac{1}{4}$, and sometimes at higher than 13; yet it has sometimes come lower than 8; and particularly December 26, in the morning, to $7\frac{3}{4}$. It has ever since been rising, and was on January 1, when the frost seemed first to relent, somewhat higher than 9; and is this day, January 7, about $13\frac{1}{2}$. The baroscope at 29; but for some days before it was about $28\frac{3}{4}$, (the weather having been windy and rainy,) and so it was in the frost about December 25, but then continued to rise till about January 2, to $29\frac{3}{8}$; but had been December 13 at $30\frac{1}{8}$, which is the highest I have ever known it in my baroscope; $27\frac{7}{8}$ being the lowest that I have ever observed it in, (October 26, 1665,) the most usual height being about 29 or somewhat higher.

An Account of a small Tract, entitled, THOMÆ HOBBES *Quadratura Circuli, Cubatio Sphæræ, Duplicatio Cubi, (secundo edita,) Denuò Refutata, Auth.* JOH. WALLIS, *S. T. D. Geom. Prof. Saviliano. Oxoniæ,* 1669. *N°* 55, *p.* 1121.

Since Mr. Hobbes thought himself obliged to make some reply to Dr. Wallis's confutation of what he had, not long since, published on this argument; Dr. Wallis made no stay at all to return this answer and second refutation.

The Conjunction of the two Seas, the Ocean and the Mediterranean, by a Channel, cut out through Languedoc in France, by the Authority of Louis XIV, and the Contrivance and Management of Monsieur RIQUET. *N°* 56, *p.* 1123.

It has been always agreed upon, that the conjunction or communication of the seas has been thought possible; and that many ways, even within the bounds of France, without any dependence on or participation with the neighbouring countries. The thing has also been often proposed under the reigns of Henry IV, and Louis XIII. But whether it be that it has ever been judged too difficult or too expensive, or that those great princes were engaged in more urgent affairs, or that their ministers would not apply themselves to it in good earnest; so it is, that it has been hitherto rejected.

Monsieur Riquet, who long since had considered the project, having opened it to the Minister Colbert, was happy enough to make him relish it, by showing him the advantages which then might accrue to the kingdom, by facilitating commerce, and by declaring to him the difficulties to be grappled with in this work, with the means of surmounting the same. This great Minister soon acquainted his Majesty therewith, who gladly hearkened to him, put the thing in deliberation, and having resolved on it, gave a general commission to M. Riquet, esteemed the most capable person for executing such a design.

This difficulty being overcome, and M. Riquet having satisfied the objections made by several sorts of people against this enterprise, it was necessary, before the actual undertaking of the work, to resolve upon the choice that was to be made of the two ways by which the channel was to be carried from the river Garonne to the Aude. Some persons interested would have it carried all along to the plains, reaching from Cærcassone to Tholouse. Others would have it run into the river Fresqueil, from thence into the plain of Revel, and thence into the river Agout, and so into the Garonne. M. Riquet followed neither of those two ways, but formed thence a third, which was to make use of what was best in these two ways, by taking from the former what it had of good convenient country from the point of separation to Tholouse; and from the latter, what advantage the bed of Fresqueil would afford him; yet so that, whereas some pretended to make this channel of communication pass into the very channels of the little rivers Lers, Fresqueil, Aude; Monsieur Riquet makes his channel pass, as much as he can, in the adjacent plains, leaving the ancient channels of those rivers to serve for receiving and carrying away the great and frequent inundations of waters, and thereby securing the new channel from the danger of being inconvenienced by them.

The highest place of the whole channel was well enough known, which was to be the point, called the point of division or separation. Nature herself sufficiently pointed at it by the spring of Grave, which partly runs towards the Ocean, and partly towards the Mediterranean: and it is near this place where the great basin is made to receive the waters that are to run both sides of the channel. But this water being not near sufficient, it was necessary to find out others, that might be high enough to fall into the said bason, and copious enough to keep it always full, and to furnish for the sluices of those two parts of the great channel.

It appeared also that this plenty of waters could only be had from the Black Mountain, distant enough from it; but it did not appear how this could be effected. In short, it is this wherein the greatest difficulty of the whole work consisted. For we must know, that there are many springs issuing from the higher part of that mountain, and that among the rest there are five great ones, which make as many small rivers, that are never dried up, but run along the hillocks of this mountain, and fall together into the river Fresqueil, but so far above the said point of separation, that that remoteness would render them absolutely useless, it not being possible to make them remount. Wherefore to make them serve for this purpose, it was necessary to interrupt their natural course from north to south, and to give them a contrary one, from east to west, by digging a conduit for them across the mountain, through places which are all steep rocks and horrid precipices. There was then a necessity to make those five rivers run, the first into the second, and the second into the third, fourth and fifth, and to re-unite them all in the deriving channel, which at last carries them to the point of separation for the end above-mentioned.

Some Considerations relating to Dr. Wittie's *Defence of Scarborough Spa; concerning a Salt Spring in Somersetshire; and a Medical Spring in Dorsetshire. By Dr.* Highmore, *in a Letter to Dr. J.* Beale, *at Yeovil in Somersetshire. N° 56, p. 1128.*

Dr. Highmore doubts of the existence of alum in the Scarborough spa. He remarks, that from a wine quart of the salt spring at East Chenock, in Somersetshire, (above 20 miles from the sea) he obtained by evaporation near 80 grains of salt; though this experiment was made at a time when the spring was less salt than usual, by reason of the late rains. He supposes that the Farrington waters in Dorsetshire, which are called chalybeate, are impregnated principally with vitriol or salt of iron; for he found that by dissolving two grains of the salt of iron in four ounces of common water, and adding a given proportion of gall [gall-nut], it appeared of the same colour, yielded the same precipitate, and had the same taste as the Farrington waters when so treated.

The Causes of Mineral Springs further inquired into: and the strange and secret Changes of Liquors examined. By Dr. J. BEALE. N° 56, p. 1131.

There is nothing in this paper that is worthy of being extracted.

Instances, Hints, and Applications, relating to a main Point, concerning the Use that may be made of Vaults, deep Wells, and cold Conservatories to find out the Cause, or to promote the Generation of Salt, Minerals, Metals, Crystal, Gems, Stones of divers Kinds; and helps to conserve long; or to hasten Putrefaction, Fertility of any Land, &c. By Dr. BEALE. N° 56, p. 1135.

What has been said of the preceding, is equally applicable to the present paper.

An Account of a Book, entitled, De Respirationis usu Primario Diatriba, Auth. MALACHIA THRUSTON, M. D. Cui accedunt Animadversiones à Cl. Viro in eandem conscripta, una cum Responsionibus Authoris. Londini, 1670. *N° 56, p.* 1142.

In this treatise the learned author maintains, first, That there is and must be motion in the blood. 2dly, He declares what kind of motion it is, and how various, showing also that all those motions are to be ascribed to the vital blood, and to be preserved therein. 3dly, He proves (which is his main design) that those motions are both continually produced, and maintained by the means of respiration, premising something about the nature and properties of the air, and the fabric and motion of the respiratory organs.

Then he shows the probability of his hypothesis, as being intelligible, and able to solve innumerable questions, and among them such as have been esteemed almost insoluble. And first, he teaches how respiration maintains that progressive motion, which he also calls the motion of rivers; and then, how it preserves the motions of fluidity and warmth, by the air's subduing, comminuting, and dilating the blood. Where he digresses to give an answer to those that will not allow the air to have any ingress into the blood; as also to explain the cause of sanguification, ascribing it neither to the heart nor the liver, but principally to the lungs in those that are born; but in fœtuses to the maternal blood and the umbilical vessels.

Next he proceeds to explain the many problems of respiration by the delivered hypothesis: and chiefly why respiration is so absolutely necessary to life, viz. because life principally consists in the motion of the blood, which soon ceases when respiration is stopped. Upon which question, thus resolved, depends also the solution of divers others to be found in this book.

END OF VOLUME FOURTH OF THE ORIGINAL.

Extract of a Letter from John Winthrop, *Esq. Governor of Connecticut in New England, to the Editor, concerning some Natural Curiosities of those Parts, especially a strange and curious Fish, sent for the Repository of the R. S. N° 57, p. 1151. Vol. V.*

I know not whether I may recommend some of the productions of this wilderness as rarities or novelties, but they are such as the place affords. There are, among the rest, two or three small kinds of oaks, which are so slender and low, bearing acorns, that the hogs can reach them on the branches. Of this sort of dwarf-oak there are whole forests in the inland country, which by the spreading of the roots makes the land very difficult to break up at first with the plough.

There are also sent you some pieces of the bark of a tree, which grows in Nova Scotia, and in the more easterly parts of New England. On this bark there are little knobs, within which is a liquid matter, like turpentine, of a very healing nature.

In the same box are pods of a vegetable, called silk grass, which are full of a kind of very fine down-like cotton-wool, many such flocks in one and the same pod, ending in a flat seed. It is used to stuff up pillows and cushions: being tried to spin, it proves not strong enough.

You will find also a branch of the tree called the cotton-tree, bearing a kind of down, which is not fit to spin. The trees grow high and thick. At the bottom of some of the leaves, next to the stalk of them, is a knob, which is hollow, and a certain fly, somewhat like a pismire fly, is bred therein.

There are also some of the matrices, in which those shells are bred, of which the Indians make the white wampanpeage, one sort of their money; they grow on the bottom of sea-bays, and the shells are like periwinkles, but larger.

Whilst they are very small, and first growing, many of them are within one of the concave receptacles of these matrices.

There is besides a strange kind of fish, which was taken by a fisherman, when he was fishing for cods in the sea which is without Massachusets Bay in New England. It was taken alive by a hook. The name of it I know not;* nor can I write more particularly of it, because I could not yet speak with the fisherman who brought it from sea. I have not seen the like. The mouth is in the middle; and they say, that all the arms you see round about, were in motion when it was first taken.

We omit the other particulars here, that we may reflect a little on this elaborate piece of nature. The fish, as it is yet nameless, we may call Piscis Echinostellaris Visciformis; its body resembling an echinus or egg-fish, the main branches, a star, and the dividing of the branches, the plant missletoe. See fig. 1, pl. xi. This fish spreads itself from a pentagonal root, which encompasses the mouth, being in the middle at a, into five main limbs or branches, each of which, just at the issuing out from the body, subdivides itself into two (as at 1) and each of those 10 branches do again (at 2) divide into two parts, making 20 lesser branches: each of which again (at 3) divide into 2 smaller branches, making in all 40. These again (at 4) into 80; and those (at 5) into 160; and these (at 6) into 320; these (at 7) into 640; at 8, into 1280; at 9, into 2560; at 10, into 5120; at 11, into 10240; at 12, into 20480; at 13, into 40960; at 14, into 81920; beyond which, the farther expanding of the fish could not be certainly traced, though possibly each of those 81920 small sprouts or threads, in which the branches of this fish seemed to terminate, might, if it could have been examined when living, have been found to subdivide yet farther. The branches between the joints were not equally of a length, though for the most part pretty near; but those branches, which were on that side of the joint on which the preceding joint was placed, were always about a fourth or fifth part longer than those on the other side. Every one of these branchings seemed to have, from the very mouth to the smallest twigs or threads in which it ended, a double chain or rank of pores, as appears by the figure. The body of the fish was on the other side; and seemed to have been protuberant, much like an echinus (egg-fish or button-fish) and, like that, divided into five ribs or ridges, and each of these seemed to be kept out by two small bony ribs.

In the figure is represented fully, and at length, only one of the main branches, whence it is easy to imagine the rest, cut off at the fourth subdividing branch,

* The animal here described is improperly termed a fish; it belongs to the tribe of *vermes* in modern natural history, and is the *asterias caput medusæ* of Linnæus.

which was done to avoid confusion, as well as too much labour and expence of time in the engraving.

The Reflections relating to Medical Springs, N° 52, considered. The Indications of some healing Springs remarked; with an Account of some such Springs in England, which confirm the Indications; and of others obiter. By Dr. J. Beale. N° 57, p. 1154.

Of a piece with the other chemical papers of this author, prolix in the extreme, and wholly uninstructive.

Some Observations, Directions, and Inquiries concerning the Motion of Sap in Trees, in pursuance of what was formerly begun therein, about the latter end of 1668, *and the next following Spring. By Dr. Ezerel Tonge, and Francis Willoughby, Esq. N° 57, p.* 1165.

After the inquiries and remarks of Dr. Tonge, on the difference between his experiments and former ones, Mr. Willoughby adds as follows:—

It is no wonder that Dr. Tonge's experiments, concerning the bleeding of the sycamore, do not agree with ours, they being made in a different season; his in February, and ours towards the end of March, viz. the cold which caused the increase of the bleeding in the sycamore and walnut, happened upon the 23d, 24th, 25th, 26th of March; and one sycamore, which ceased to bleed from the 11th of the same month, bled afresh copiously from wounds that had been made so long before: the buds before the cold were just ready to open into leaves, and the sap had begun to coagulate above a fortnight before. This year, making incisions in the sycamore and common maple, in January, immediately on the relenting of the first frost, we found that they both bled, and faster as the weather grew hotter, nor did the succeeding cold promote, but rather hinder their bleeding. So that the learned doctor most ingeniously conjectures, that the ascent of sap in trees depends on a certain degree of heat, sufficient to raise, but not to coagulate their respective juices. In those months wherein the heat ordinarily falls short of that degree, an accidental heat or warmth of weather promotes the bleeding; but in those months wherein the ordinary temper of the air exceeds that degree, an extraordinary fit of colder weather makes them bleed again. The experiments concerning the northern and southern sides of sycamores were made at the same time; and are well solved by the same hypothesis.

In walnut trees, we never yet found that heat promoted their bleeding, but always cold. From a wound made in a walnut-tree in January, and the beginning of this present March, in mild weather, nothing issued; but the weather changing and growing colder, it bled plentifully: which seems not well to consist with that hypothesis, and must be better examined.

Last year, the 6th and 7th of March, we made the experiments of the bleeding of poles, held perpendicular at both ends, in willow, birch and sycamore: which may so easily and certainly be found to succeed, that there needs no such exact niceness in the observation of those particulars. The 11th of March, roots of birch, great and small, bled both ways; and about the same time sycamore roots also. Of all which I doubt not but Dr. Tonge is by this time fully satisfied by his own experiments. The same birch which first began to bleed the 3d of March this year, bled three weeks sooner last year.

Some Considerations on M. Cassini's Method of finding the Apogees, Excentricities, and Anomalies of the Planets; by M. Nic. Mercator. Translated from the Latin. N° 57, p. 1168.

1. M. Cassini supposes two lines drawn from both foci to the planet, revolving in an ellipsis, one of which is the line of the mean motion, and the other that of the true.

Construction. (*Fig.* 2, *Pl. xi.*)

L is the centre of the concentric ABCDE.
BLD its diameter.
BA, BC, BP apparent distances.
DE, DF, DQ distances of mean motion.
BE, BF, BQ, DA, DC, DP right lines.
BE cuts DA in H; BF cuts DC in G; BQ cuts DP in R.
RHG is a right line.
BI is perpendicular to RHG.
I the centre of the ellipse.
LI is the excentricity.
IO = LI.
O the focus, or centre of mean motion.
L the other focus, or centre of the true motion.
IM = IN = LB.
M the apogeum; N the perigeum; BLM the true anomaly.

Demonstration.—Bishop Ward, in his Examination of the Philolaic Astronomy, published at Oxford 1653, in chap. 6, gives a method of finding the true anomaly of the planets from their mean anomaly; which is this:

In fig. 3, C is the centre of the ellipsis AEP; F the focus and centre of the mean motion; S the focus or centre of the true motion; A the apogeum; P the perigeum; E the planet; AFE the mean anomaly; ASE the true anomaly; FET a right line; ET = ES; ST a right line.

In the triangle SFT, are given, SF the distance of the foci; FT = FE + ES = AP; and AFT, the external angle or mean anomaly, equal to the sum of the angles FST and T: Therefore the angle FSE, or the true anomaly, = the difference of the angles FST and T, may be found thus: viz. As half the sum of the sides FT and FS is to half their difference, so the tangent of half the sum of the angles FST and T, to the tangent of half their difference. But half the sum of the sides FT and FS is found, by substituting for FT its equal AP, whose half is AC, which added to CS, the half of FS, makes the half sum AS, the greatest distance of the planet. If then we subtract from AS the lesser side FS, there remains the half difference of the sides FA = PS, the least distance of the plant. Then the rule for finding the true anomaly from the mean will be; As AS the greatest distance of the planet, is to PS its least distance; so is the tangent of half the mean anomaly, to the tangent of half the true anomaly.

Corol. 1. If SE be produced to V, so as that EV be = EF, and the whole SV = AP the axis; then the angle V of the triangle FSV will be half of the prosthaphæresis FES, consequently = to the half difference of the angles of the true and mean anomaly, that is, of the angles AFE and ASE; and the external angle AFV is = their half sum, after subtracting the half difference VFE from the greater AFE; and hence arise these two analogies:

1. As the sine of half the sum of the mean and true anomalies, AFV, is to the sine of half their difference V; so is SV = AP the transverse axis to SF, the distance of the foci.

2. As the sine of half the sum of the mean and true anomalies AFV, is to the sine of the true anomaly, FSV; so is SV, or the axis AP, to FV, the subtense of the true anomaly; and so likewise is the semiaxis AC, to the semi-subtense VX or FX.

Corol 2. If, in the same triangle FSV, be erected on half the subtense FV, the perpendicular XE; it will cut SV into two parts, of which the one VE is = FE the line of the mean motion, and the other SE is the line of the true motion.

II. In fig. 4, let a be the centre of the concentric chfi.
cad the diam. and line of the apses.
ch the arc of true anomaly, to which answers
di the arc of mean anomaly; therefore
cdh is the angle of half the true anomaly; and
dci the angle of half the mean anomaly.
ci and dh are right lines, which intersect in g.
gb is perp. to cd.—Then,

As db : bg :: radius : tang. bdg or cdh;
and cb : bg :: radius : tang. bcg or aci;
therefore db × tang. cdh = bg × rad. = cb × tang. dci,
and db : cb :: tang. dci : tang. cdh.

That is, db will be to cb, as the tangent of half the mean anomaly, to the tang. of half the true anomaly, consequently, by the preceding rule, as the greatest distance of the planet is to its least distance. Therefore *db* will be equal to the greatest distance, *cb* the least, and *ab* the eccentricity.

And since the same demonstration holds of all the other points of intersection, viz. that the perpendiculars from them to the line *cd* fall upon the point *b*; the line joining these intersections must coincide with the perpendicular *bgf*.

III. Having drawn the diameter *hak*, take the arch *kl* equal to the arch *id*, and draw *kc* and *hl* intersecting each other in *p*; let fall the perpendicular *hr* from *h* upon *bgf*, and parallel to the line of the apses *cd*; then the angle *rhs* will be equal to the half difference of *ch* the true anomaly and *di* the mean. Then draw the right line *h*β from the same point *h*, making with *kh* an angle equal to *rhs*, and meeting the line of the apses in β, so shall the angle β*ah* be the measure of the arch *ch*, or of the true anomaly, and β*ha* the half difference of the true and mean anomalies, by the construction; also the external angle *c*β*h*, which is equal to the two internal and opposite angles β*ah* and β*ha*, and consequently compounded of the true anomaly, and half its difference from the mean, will be half the sum of the true and mean anomalies. Consequently, by the former analogy of the first corollary, as the sine of *c*β*h*, is to the sine of β*ha*, so is the radius *ah*, to the eccentrity *a*β. But it was demonstrated above, that *ab* is also equal to the eccentricity. Therefore the point β coincides with the point *b*.

Then erecting *bt* perpendicular to *hb*, this, if produced, will fall on the point of intersection *p*. For the triangles *rhs* and *bht* are similar by the construction; as also the triangle *hpk* is similar to *hgi*, for the angles *pkh* and *gih* are equal, insisting upon the same arch, *ch*, as also the angles *phk* and *ghi* insisting on the equal arcs *kl* and *id*, consequently the two other angles *hpk* and *hgi* are equal; and taking away the equal angles *bht* and *rhs* from the equals *phk* and *ghi*, there will remain *phb* equal to *ghr*. Whence I argue thus, *srh* = *tbh*, and *rhs* = *bht*, therefore *hsr* = *htb*, consequently their complements to a semicircle are equal, viz. *rsi* = *btk*, and *sig* = *tkp*, therefore also *igs* = *kpt*, which subtracted from the equal angles *igh* and *kph*, there remains *hgs* = *hpt*, and *ghr* = *phb*; therefore also *hrg* = *hbp*; but *hrg* is a right angle, consequently *hbp* is also a right angle; and since *hbt* is a right angle by construction, *tb* will make the same right line with *bp*. And since the same demonstration holds of any other intersection of lines drawn from *h* and *k*, to the corresponding points of the true and mean anomalies; it is plain, that not only the right line that joins these intersections, will pass through the point *b*, but that *hb* will be a perpendicular to it, Q. E. D.

Corol. If from any point of the true anomaly, as *h*, to a corresponding point *i* of the mean anomaly, we draw the right line *h i*; then *b f* raised perpendicular to *c b d* will cut *h i*, in *s*, in the ratio which the line of the mean motion has to that of the true. For, by the latter analogy of the first corollary, *h b* is the half subtense: consequently, by cor. 2, the perpendicular erected from *b*, viz. *b t*, cuts the diameter *h k*, in *t*, in the same ratio that the line of the mean motion has to that of the true. Therefore *r s*, or *bf*, cuts *h i* in the same ratio in *s*, because of the similar figures *t b h k p h b* and *s r h i g h r*.

Another method of finding the apogee and eccentricities from that of Dr. Ward, for finding the first inequality, is thus: Let *l* and *d* be the two foci of the ellipsis, (fig. 5) *t* and *u* two points of the planets true motion; *tu* an arch of the ellipsis, seen from *l* under the angle *tlu*, and from *d* under the angle *tdu*; also *ld*, the distance of the foci, seen from *t*, under the angle *dtl*, and from *u*, under the angle *dul*. I say, the difference of the angles *tlu* and *tdu*, is equal to the difference of the angles *dtl* and *dul*.

For since the sum of the three angles of the triangle *lux* is equal to that of the triangle *dtx*; and if from both sums, the equal angles *lxu* and *dxt* be deducted, the remainder will be $ulx + lux = tdx + dtx$; and if from these two sums be taken the unequal angles *ulx* and *tdx*, the difference of the remaining angles *lux* and *dtx*, is equal to the difference of the subtracted angles *u l x* and *tdx*.

With the centre *l*, and distance *mn* of the transverse axis, describe the circle *abc*, whose arch *ab* is seen from *l* under the angle *alb*, and from *d* under the angle *adb*. Also the distance of the foci *ld* is seen from *a* under the angle *lad*, and from *b* under the angle *lbd*; therefore again the difference of the angles *alb* and *adh*, is equal the difference of the angles *lad* and *lbd*. But, by cor. 1, the angle *lad* is half the angle *lud*, and the angle *lb d*, half the angle, *ltd*; therefore the difference of the angles *lad* and *l bd*, is equal to half the difference of the angles *lud* and *lt d*; and consequently, the difference of the angles *alb* and *adb*, is equal to half the difference of the angles *ult* and *udt*, the former of which is the apparent interval of the two observations, and the latter, the interval of the mean motion. The difference of these intervals being given, we have the half of this difference, viz. the difference of the angles *alb* and *adb*.

But *alb* is the same with *ult*, which is given; we have therefore the angle *abd*, under which the arch *ab* is seen from *d*.

After the same manner it will be demonstrated, that the difference of the angles *tly* and *tdy*, is equal to the sum of the angles *ltd* and *lyd*; as also that the difference of the angles *blc* and *bdc* is equal to the sum of the angles *lbd* and *lcd*. And since *lbd* is the half of *ltd*, and *lcd* half of *lyd*; the sum of *lbd* and *lcd* will be equal to half the sum of the angles *ltd* and *lyd*, that is, the difference of the angles *blc* and *bdc* will be equal to half the difference of the angles *tly* and *tdy*, the former of which is the apparent interval of the two observations, and the latter, that of the mean motion. Therefore their difference being given, we have the difference of the angles *blc* and *bdc*. But *blc* is the same with the given angle *tly*; therefore the angle *bdc* is also given, under which the arch *bc* is seen from *d*.

Whence it appears, that from the given mean and apparent intervals of two observations, the angles are given under which any arches of the circle *abc* are seen from *d*, intercepted between the lines of the true motion. Therefore, by Herigon's Theory of the Planets, l. 1, c. 3, prop. 12, schol. 1, so many segments of a circle may be described, which may contain the angles under which these arches are seen from *d*, all which segments will intersect each other mutually in *d*; so that, after this manner, the apogees and eccentricities of the planets may be found by a geometrical delineation, by any number of observations: and circles are as easily drawn as right lines.

But to grant what is true, viz. that the geometrical delineation of M. Cassini is somewhat more expeditious; yet, should we aim at that accuracy which astronomers desire, it might be feared it would require very large diagrams, and become more operose than the calculus itself. But if we make use of this, we shall find both methods to be equivalent.

It now remains that we examine the hypothesis.

The invention of elliptical orbits is undoubtedly owing to Kepler; but the determining the degrees of the acceleration and retardation with which the planets move, is no less necessary for completing the hypothesis, than the defining the orbit itself. Though nothing to this purpose is to be observed in Cassini or his interpreter; and from the construction of the problem and its solution it is manifest, that he supposes a planet seems to move from the superior focus with an equable motion: and Kepler himself was of this opinion, as appears from his writings. But, when he found that this did not agree with his observations, he changed his mind, and maintained that the line of the true motion of a planet described equal elliptical areas in equal times; and that there is no point from which a planet is seen to move with an exactly equable motion, unless we suppose it a librating or moving point: but that the equable motion of a planet cannot be more properly assigned to any point than the superior focus. No person has yet denied that Kepler's areas do satisfy the appearances; but since neither he himself, nor any after him, could determine them by a direct calculus, some have blamed Kepler as giving way too much to physical causes, and swerving from geometry: as if physical causes were inconsistent with it; or, as if the problem were two ungeometrical, which, without mentioning physical causes, is thus proposed: *The area of the trilinear figure intercepted between the line of the apses and that of the true motion, and the elliptical periphery, being given; to find the angle at the sun.*

M. Bulliald attempted to investigate by geometrical reasonings, both the orbit and the degrees of remission and intention in the motion of the planets; that we might be led from that equable motion, assumed by astronomers before Kepler's time, to that inequality observed in the heavens. And bishop Ward, adopting this hypothesis, first shows how to do the like with the line of the equable motion revolved round the other focus of the ellipsis, and then gives that method of direct calculation, which we have repeated above. And two years after, the illustrious Count Pagan undertook to maintain the truth of this hypothesis, so far, that he ascribed the difference observed about the octants, to the unskilfulness of astronomers. But M. Bulliald, upon second thoughts, and considering that astronomical observations were the best guides, upon applying some limitation to his former inventions, made that difference vanish. Whence then it appears, that that hypothesis, on which M Cassini builds the investigation of the apogees and eccentricities, deviates so far from the truth, as is stated by that limitation of Bullialdus.*

An Account of three Books. N° 57, p. 1175.

I. Esperienze intorno alla Generatione Degl' Insetti, fatte da Francisco Redi,† Academico della Crusca. In Firenze, 1668, 4to.

* In this state of uncertainty remained the difference between the old hypothesis and the phænomena or observations, till Newton demonstrated in his Principia, that the planets must move in elliptical orbits, and that they must describe elliptical areas always proportional to the times.

† The notes upon Redi's experiments on vipers, at p. 58 of this vol. having, for the better elucidation of that subject, been extended to some length, it was deemed proper to postpone a biographical account of this author until another opportunity of introducing it should present itself. This opportunity now occurs.

Francis Redi, not more distinguished by his scientific labours than by his poetical talents and classical acquirements, was descended from an ancient and honourable family, and was born at Arezzo, in 1626. He studied the profession of physic at Pisa, and soon brought himself into notice by his writings, the fame of which procured him the appointment of physician to Ferdinand II. and afterwards

The learned and ingenious author of this book, with much industry undertakes to evince, that there is no such thing as equivocal generation, but that every animal is generated by the seed of another, (its parent,) or at least from some living and uncorrupted plant, as out of oak-apples, and several protuberances and excrescences of vegetables.

First, then, in the assertion of the universal and true generation of insects by a peculiar and paternal seed, the author positively affirms, that he could never find, by all the experiments and observations he ever made (of which he relates a great number made by himself upon all sorts of animals) that any insects were ever bred from flesh, or fish, or putrefied plants, or any other bodies, but such as flies had access unto, and scattered their seed upon; he having taken extraordinary care and pains to observe, that always on the flesh before it did verminate, there sat flies of the self-same kind with those that were afterwards produced thence; and again, that no worms would ever come from any flesh in vessels well covered, and defended from the access of flies; so that to him there is no generation of insects from any dead animals, but such as have been fly-blown.

And, lest it should be objected that the reason why, in vessels exactly closed, no insect breeds, is the want of air necessary to all generation, he has carefully covered several vessels with very fine Naples veil, for the air to enter though flies could not; but that no worms at all were bred there, notwithstanding that many flies swarmed about them, invited by the smell of the flesh inclosed therein.

2dly. To make out the other part of his position, viz. that those animals that are not bred by the seed of other animals, are produced from some live plant, or its excrescence; this author esteems it not absurd to affirm, that that

to Cosmo III. for with the family of the Medici literary and scientific merit led to preferment, and was sure of receiving its due tribute of respect and reward. How highly the last-mentioned prince thought of Redi's labours, he further evinced by this circumstance, that after his death, which happened at Pisa in 1698, he caused a medal to be struck to perpetuate his name. His experiments on vipers have been already noticed at p. 58, above referred to. By these on insects he completely refuted the false doctrine handed down from the ancients, and blindly taught unto his days, respecting the generation of this class of animals by putrefaction; whereas he proves that they are produced from eggs deposited by the females in or upon the putrefying substances where they are found. In his treatise on animals that live within other living animals, there are, besides his account of intestinal worms, many original observations concerning serpents, snails, tortoises, &c. His letters (which form two 8vo. vols.) contain a variety of medical cases and remarks, with observations relative to anatomy, natural history, and experimental philosophy. Redi's style is regarded by his countrymen as highly classical, and he is often quoted as a standard authority in the dictionary of the academy Della Crusca. His collected works (comprising, besides the treatises already mentioned, his poems and writings on literary and miscellaneous subjects) amount to 7 vols. in 4to.

anima, or power, which is able to produce flowers and fruits in living plants, may be alike capable to breed worms in them; since that soul is so powerful, as to cause plants to feed, to grow, and to produce seed, as it doth in animals. For confirmation of which he observes, that both the generation of worms in vegetables is always and constantly the same (not at all casual,) and that all galls grow constantly in one determinate part of the branches, and always in the new branches; as also that those little galls, which grow on the leaves of the oak holm, &c. do all grow constantly on the fibres or strings of those leaves, not one of them being seen to grow on the smooth part betwixt two strings: Farther, that there are found many leaves of other trees, on which grow vesicles or small bags, or some wrinkled or swelled places full of worms, springing forth with those leaves. Besides, that there is not one gall, but it has its proper worm, and that each sort of galls has its peculiar and determinate race of worms and flies, which never vary: where he takes notice of the singular art of nature both in forming the egg, which is found in the centre of galls, and in preparing its place therein, furnishing it with many filaments, that pass from the gall apple to the egg, as so many veins and arteries, serving for the formation of the egg and worm, and for the nourishment requisite thereto. To which he adds this observation, that there being certain sorts of galls, which breed more worms at a time than one, nature has carefully provided and distinguished places for them, as she doth in those animals that are multiparous.

Having established this ground against equivocal generation, he proceeds to particulars, and refutes the opinion of those, that will have bees to be bred of the putrefied flesh of bullocks; wasps of asses' or mules' flesh; drones of horses; scorpions of buried crawfishes, or the herb basilica, or dead scorpions; toads of ducks buried in dung; mites of cheese, affirming that none of these insects have any such origin mentioned, but that all those substances have been first blown upon by some fly or other.

He intersperses through the whole book many curious and considerable observations relating to this subject: as, that some flies are viviparous, others oviparous only; not denying, however, that the same flies may sometimes breed live worms, sometimes lay eggs, according to the more or less heat of the season. 2. That human bodies breed worms, but not immediately, but by insects blowing on them. 3. That fruit and herbs, crude or boiled, kept closed up, breed no vermin, but in an open place, do breed some. 4. That there are no animals partly animated, partly yet earthy, nor animals half animated and half wood. 5. That worms breed in the livers of sheep, and the heads of stags, he having seen divers of them in both these kinds of animals, and esteeming that the soul of the superior animal was able to breed those inferior animals.

6. That lice are bred of eggs or nits, laid by their female parent; he having discerned by a microscope some nits yet pregnant with young ones, others emptied of them. 7. That all living creatures are subject to lice or some such kind of vermin; the ass not being exempted from them, as Aristotle, and upon his authority Pliny, Mouffet, &c. would have it: and that all sorts of fowl (except ostriches, in which he never could meet with any vermin in any season,) and fish have each sort their peculiar lice: of which he has represented divers of several sorts.

[While Redi proved so satisfactorily the generation of certain insects in putrefying substances from eggs, it is surprising he was not aware that gall insects and the worms in the liver of sheep must have a similar origin.]

II. Pharmacopœia Regia, sive Dispensatorium Novum locupletatum et absolutum, cum annexa Mantissa Spagyrica, et gemino Discursu Apologetico contra Ott. Tachenium, et Franc. Vernis. Auth. Johan. Zwelfer, M. D. 1668, fol.

To notice the contents of this book would not now be desirable, the pharmaceutical art having undergone a thorough change since the time when this dispensatory was written.

III. Affectionum quæ dicuntur Hystericæ et Hypochondriacæ Pathologia Spasmodica Vindicata, contra Responsion. Epistol. Nathanaelis Highmori, M. D. cui accessere Exercitat. Medico-physicæ duæ, 1. De Sanguinis Accensione. 2. De Motu Musculari, Auth. Tho. Willis, M. D. Nat. Phil. Prof. Oxon. nec non Med. Coll. Londini et Soc. Regiæ Sodalis. Lond. in 4to.

The learned author of this book makes it his chief business, to vindicate his doctrine, that the two affections, expressed in the title, belong to the brain and the nerves, from the arguments alleged by the other learned physician, Dr. Highmore, who makes the one to be a distemper of the blood and lungs, and the other of the stomach. In doing which, our author first undertakes to show, by several arguments, that the affections, called hysterical, cannot proceed from the lungs stuffed up to a great degree of stiffness with flatulent blood; seconding them by some histories and observations, which seem very pertinent: which done, he proceeds to remove the difficulties and objections, alleged by Dr. Highmore in his epistle, formerly printed and taken notice of in Number 54 of these Transactions.

2dly, He endeavours to evince that the hypochondriac passion is wrongfully ascribed to the stomach. Where he takes occasion, both to deny to the spleen the office of warming the stomach, and to assert the fermentative function of the same: teaching withal, that the acidity is not produced in the stomach alone, nor thence only communicated to the blood and other humours; and that it proceeds from the fluor of the salt, and that that salt is one of the elements of natural things; which latter he attempts to make out by experiments against the objections of his adversary.

To this vindication are annexed two exercitations, whereof the first is employed in proving the accension of the blood: the author undertakes to show that the blood being animate, that animation or life depends on its being kindled, since the proper affections of fire and flame belong to the life of the blood; which is here deduced at large; although it be withal acknowledged that this vital flame does not, as the common flame, appear to view, in regard that its form is subordinate to another superior form, viz. the soul of the animal.

The second discourse treats of muscular motion, where having declared, that Dr. Steno has been the first that has delivered rightly the structure of the muscles, and that the figures described by him are visible in them; and also made out the motions of their fibres by divers anatomical experiments; besides many other considerable particulars: he asserts that the motion of muscles depends upon a constant influx both of the blood and the animal spirits; and that the latter alone, without being associated with the former, cannot perform that moving function; maintaining, that as the spirits (or springy particles) in the contraction of a muscle rush out of the tendons into the fleshy parts of it, and in the relaxation, skip back from these into those, so those spirits lying quiet within the tendons, do swell the fleshy fibres by conflicting and struggling with the particles of the blood. To which he adds the manner how the instinct of performing or stopping the muscular motion is imprinted by the nerves in the muscles: also divers important particulars concerning two sorts of spasms or convulsions proceeding from the muscles, with an illustration of the same by a very remarkable case. He concludes with solving the objections to which his doctrine of the muscles may be liable, and annexing some figures, representing some of the muscles, together with an explanation of the same.

Extract of a Letter from Mons. de MARTEL, *of Montauban, concerning a Way for the Prolongation of Human Life, together with some Observations made in the Southern Parts of France. Translated by Mr.* OLDENBURG. *N° 58, p.* 1179.

There is nothing in this paper on the subject of longevity which is worthy of being noticed. As to the observations made in the south of France, one of them relates to the manner of making muscadine wine from grapes suffered to remain upon the vines after they are ripe, until they are half dried; the second relates to the discovery of oyster-shells and other marine productions in some mountains between Beziers and Narbonne, two leagues from the sea, and about 15 or 16 fathoms above the level thereof. In the mountains near Nice cockle-shells are found imbedded in the same manner. In the last observation there is an account of the liquor of the pericardium being found congealed into a consistence fit to be cut with a knife, and two square fingers thick about the heart.

Magnetical Variations at Rome. By M. Auzout. Nº 58, *p.* 1184.

The declination of the loadstone has for many years been observed not to continue always the same in the same places; and the variation to be such, that it can be no longer imputed to any defect in the observations, as it was believed at first, when it was not very great: it has been noted some years since, that the magnetic needle, which almost every where had declined eastward to 8, 10, and 12 degrees, after its diminishing little by little as far as to the meridian, began to decline westward.

M. Adrian Auzout has made here in Rome the following observation about the declination of the loadstone, on many meridian lines drawn as exactly as possibly he could with a needle, three quarters of a palm long;* and on all the lines it was seen to decline somewhat more than two degrees westward, and on some near two degrees and a half.

But by the observations here made formerly, it appears that the needle has declined eastward to eight degrees, and has afterwards been diminishing, till it is come to the other side, where we find it at present.

It seems not that this difference of ten degrees and more can be attributed to the change of the pole of the earth, as some esteemed, perhaps before they knew it was so great; nor, as others would have it, to the magnet, or to the iron, that are found in certain places, because there is but little loadstone; and Mr. Auzout affirms, that the mines, which he has seen, make no impression at all on the needle. So that it is difficult to hit the true cause of such a variation; yet, however, if the direction of the magnet, and of the needle touched by it, depends on the flux of a certain matter, passing through the whole earth, or the exterior parts of it, straight along the axis; it may be said that it proceeds from changes made in the said flux, which, supposing the inequalities of the earth, and the alterations continually made therein, as well artificial, by excavations and such-like other works, as natural, by corrosions caused by fire and water, or by the generation of metals and stones, cannot but in progress of

* This is about six inches.

time change its situation; as rivers cannot remain long without winding and changing their course, if it happen that the ground over which they run be unequal, or of a different nature.

If this should be the case, there would be no hopes of finding a regular hypothesis for that change, forasmuch as it would depend on causes that have no regularity at all in them, as most of the mutations of nature are.

From this observation mathematicians are invited from time to time to make the like in their countries, to see whether in this change there be any regularity. If it had been observed every year, we should already know the progress thereof, and see whether there were an uniformity, and in what time the needle did exactly respect the pole. Wherefore it were very desirable that for the future they would use greater care and diligence in making most exact meridians, as well for their own observations, as for the conveniency of those who in their travels shall have the curiosity of observing with the needle itself, as M. Auzout designed to do in the cities where he passed, if he had found meridians there, or such as had been unsuspected of the proximity of iron.

It were well to observe, whether the declination which almost through all Europe has been eastward, be now every where westward; as also, whether in America, where the declination was almost every where westward, it be increased or no proportionably; and so of other parts of the world.

So far this relation, in pursuance of which, order has been given by the Royal Society, that precise meridians be made in several places of England, for observing the present declination of the needle from them here in London, and other cities of this kingdom; and that even those meridians that were made very exactly many years ago be examined by a careful describing of new ones, to see whether they still hold true, in regard of the suspected alterations in nature.

If any shall inquire after the manner of finding the variation of the needle, he will find several ways of doing it accurately, both by sea and land, in N° 24 of these Tracts; where also the great usefulness of making exact observations of this kind is taken notice of, both for the discovery of the true cause of the magnet's verticity, and for the finding the longitude at sea.

Extract of a Letter, dated Venice, Jan. 25, 1670, from Sig. Jacomo Grandi, concerning some Anatomical Observations, and two odd Births. Translated by Mr. Oldenburg. N° 58, p. 1188.

In my anatomical dissections at Venice, the first year, I met with nothing curious, but the virsungian channel manifestly inserting itself in the spleen, and admitting a silver stiletto, which I had never observed in any corpse. And then, a liver divided into five lobes, together with a spleen of the figure of a saw, of extraordinary bigness. Last year, one drowned of about 35 years of age, had the lacteal vessels so apparent and large, that having shown how they lay in the body, I exhibited them even the day after in the mesentery, taken out and displayed upon a table.

I also lighted upon two odd births, one was of twin females, very handsome, but so fastened together by the breast that only one body was discernible; their chins united together, they seemed to kiss one another. I could not dissect them as I wished, because they were delivered to me to embalm, and the indigent father of them, who looked for gain, would not let me have them but for a great sum of money. Wherefore not to spoil them for the purpose designed, having only opened them upwards from the navel, which was common to them both, I took out the intestines, the stomach, the heart, the lungs. There was but one heart, though greater and rounder than ordinary, so that nature seemed to have united the matter of two into one. They had two lungs, and one stomach, the pylorus of which did strangely branch itself into two ranks in the bowels. There was but one liver, but large; two spleens, four kidneys, two wombs, full of a white matter, like a concreted semen: two vulvas, with their distinct hymens. In short, they were so well made in all the other members, that the painter who was employed to draw them affirmed, that if they were done in ivory he would have paid any money for them.

The other monster was a boy terrible to behold, born with his breast open, the bowels out of the belly, the legs distorted, the bladder in the place of the fundament; in the genitals, besides that the testiculi were close to the kidneys, there was nothing but a membranous expansion, wherein the spermatic vessels were lost. Signor Steno, who honoured me with his visit, saw the administration of it, which I had before made in the presence of many noblemen and physicians at my house.

Concerning the Mines, Minerals, Baths, &c. of Hungary, Transylvania, Austria, and other neighbouring Countries. By Dr. EDWARD BROWN.* *N° 58, p.* 1189.

I have not been unmindful of the inquiries you were pleased to honour me

* Dr. Brown was born about the year 1642, and was educated first at Cambridge and then at Oxford, where he took his doctor's degree. In 1668 he visited several parts of Europe, and at his return published his travels. He was made physician to Charles the Second, and to St. Bartholomew's Hospital. He became president of the College of Physicians; and died in 1708. Charles the Second used to affirm that he was one of the best bred men in England.

with on the account of the Royal Society; and in answer to them I shall first acquaint you with what I found and learned of the salt-mines: concerning which I now present you with those two kinds of Transylvanian stone salts which you mentioned; and also with salts out of the mine at Eperies in Upper Hungary; together with some account of that mine. Of the sal gemmæ I have sent you four pieces, and a stone of salt, as it was taken out of the mine, which, if you please, for your further satisfaction, to break with a hammer, you will find to split in your hand into tables or parallelopipeds. These are accompanied by a specimen of that mineral salt which is commonly used at table. This is found in most of the salt mines.

Half an hour's journey from the city Eperies, there is a salt-mine of great note. From the first place of descent, to the bottom, it is about 180 fathoms deep. Into this the miners descend first by ropes, and at last by ladders to the lower parts. The mine is for the most part in an earthy and not a rocky ground. The veins of salt are large, and there are pieces to be found of ten thousand pound weight. They commonly hew out the salt into long square pieces of two feet in length, and one in thickness; and for use it is broken and ground between two grind-stones. The mine is cold and damp; but the salt being a stone-salt, is not easily dissolved, or at least in any great quantity, by dampness or moisture: Yet the water of the mine is impregnated with salt in such sort, that being drawn out in large buckets, and afterwards boiled up, it affords a blackish salt, which they give to their cattle in the country.

The colour of the ordinary stone-salt of this mine is not very white, but somewhat gray; yet being broken and ground to powder, it becomes as white as if it were refined: And this salt consists of pointed parts or facets. There is also another sort of salt which consists of squares and tables; and a third, of striæ or long shoots. Nor is all the salt of this mine of one colour, but of several; that which is found grossly mixed with the earth receives some colour from it; and even that which is most pure and resembles crystal, often receives tints of several colours.

But to proceed to the gold and silver mines: As to the former, that among the seven mine towns in Hungary, viz. Chremnitz, Schemnitz, Newsol, Koningsberg, Bochantz, Libeten and Tiln, Chremnitz is the richest in gold. They have also at present gold mines at Bochantz and Koningsberg; and they report in that country, that there has been formerly a rich gold mine at Glashitten, but lost, since that Bethlem Gabor overran those parts, when the undertakers stopped up the mine and fled. They have worked in the gold mine at Chremnitz nine hundred years. This mine is several English miles in length, and about one hundred and sixty fathoms deep. Many veins of the ore run to the north and to the east. They work also towards one, two and three o'clock, as they speak: for the miners direct themselves under ground by a compass, not of 32 points, such as is used at sea, but by one of 24, which they divide, as we do the hours of the day, into twice 12. Of the gold ore some is white, some black, red, or yellow: that with black spots in white is esteemed the best, as also the ore which lies next to the black veins. Pieces of pure gold have been found in this mine; some of which I have seen in the Emperor's treasury, and in the Elector of Saxony's repository; one piece as broad as the palm of my hand, and others less, and upon a white stone many pieces of pure gold; but these are very rare. The common yellow earth of the country near Chremnitz, although it be not esteemed ore, affords some gold.

Some passages in this mine cut through the rock, and long disused, have grown up again; and I observed the sides of some, which had been formerly wide enough to carry their ore through, to approach each other, so as we passed with difficulty. This happens in moist places. The passages unite not from the top to the bottom, but from one side to another.

There is vitriol in this mine, white, red, blue and green; and also vitriolate waters. There is a substance found, which sticks to the gold ore, of small pointed parts like needles, called by them antimony of gold. There are crystals found here, and some tinctured yellow.

The miners will not allow any quicksilver or brimstone to have been found here, yet in the lately mentioned antimony of gold there is evidently sulphur, as you will perceive by burning it. The quicksilver mine mentioned in the answer to Kircher's Inquiries in his Mund. subterraneus, is an Hungarian mile, or seven English miles, distant from Cremnitz, and is not wrought in at present.

There is a vitriol mine in these hills near the gold mine; the earth or ore of it is reddish, and sometimes greenish. This earth is infused in water, and after three days the water is poured off and boiled seven days in a leaden vessel, till it becomes a thick granulated whitish substance, which is afterwards reduced to a calx in an oven, and serves in the making of aquafortis, or the separating water used at Schemnitz.

They have divers ways of taking the gold out of its ore; by burning the ore, by melting, by adding silver ore and other minerals, sand and lead; as they find the ore in a fluid or solid state. But to avoid prolixity, I will set down that way only, which they proceed in without lead. They break and pound the ore in water, very fine; wash it often, and lay it in powder upon cloths, and by the gentle oblique descending of the water over it, and their continual stirring it, the earthy, clayish, and lighter parts are washed away, while the heavier and metalline remain in the cloth. These cloths are afterwards washed clean in several tubs, and the water, after some settling, poured off from its sediment; which sediment is again washed and stirred up in several vessels and troughs, till at length they sprinkle quicksilver upon it, and knead it well together for an hour, and then washing it again in a wooden vessel, after the separating of much of it which the quicksilver touches not; by striking this vessel against their leg, they bring the gold and quicksilver together, in an amalgama, to one corner of it. From this amalgama they strain as much of the quicksilver as they can through coarse cloths first, and then through fine: they put the mass remaining upon a perforated plate, which they set over a deep pan placed in the earth; in the bottom of which pan they also put quicksilver: This pan they cover and lute the cover well, and then make a charcoal fire upon it; they drive down the quicksilver yet remaining in the gold to the rest in the bottom of the pan; then taking out the gold, they cast it into the fire, that it may still become purer.*

Concerning the silver mines, there are divers of them at Schemnitz in Hungary, as the Windschacht, the Trinity, of St. Benedict, of St. John, of the three Kings, and several others of lesser note. The chiefest and most wrought are those of Windschacht and Trinity. They have no river here, but much water in the mines, which is a double inconvenience to them, viz. to want water above, and to be glutted with it under ground, so that they are constrained to send much of their ore to Hodrytz and other places, where are small rivers, by which their bellows and hammers may be moved, their ore pounded, washed, and other works requisite performed. Nor do they want engines to pump the water out of the mines, moved by wheels drawn about continually by horses, 12 horses at a time to each wheel. In Windschacht mine, deep in the earth, is a large wheel of 12 yards diameter, turned about by the fall of subterraneous waters. This wheel moves engines, which pump out the water from the bottom of the mine up to the cavity, wherein this wheel is placed. The water, which moves this

* Within these few years great improvements have been made by Baron Born and other metallurgists, in the amalgamation of gold and silver ores. The savings which result from these improvements are immense, not only in respect to the recovery of the quicksilver employed in the amalgamation, but also in regard to fuel and labour. In Baron Born's process, the combination of the quicksilver with the precious metals was effected in copper vessels, aided by a moderate heat; but Mr. Gellert found that this combination might be accomplished without the aid of fuel, by triturating the prepared ores with quicksilver, in wooden cylindrical churns provided with perforated cast-iron pistons, moved quickly up and down by a crank motion. From the amalgama obtained in either of these ways, the superfluous quicksilver is separated by pressing it with the hands through a strong linen bag; the other portion which remains in the amalgama balls is expelled from them by distillation per descensum in cast-iron pots, the receiving pot being constantly cooled by cold water, for the condensation of the quicksilver vapour.

wheel, falls no lower into the mine, but passes away through a cuniculus made on purpose, through which both this and the other water, pumped from the deepest parts of the mine, run out together at the foot of a hill.

Trinity mine is 70 fathoms deep; built and kept open with underwork at a great expense. Much of this mine being in an earthy soil, the ore of it is much esteemed. Divers veins lie north; and other rich veins run to the north-east. When two veins cross one another, they esteem it fortunate. So that all veins of ore keep not the same point even in the same mine; which would be an help to discover them; but they have no certain way to know either which way they run, or where they are, till by the industrious persevering in the labour of the mines they are at last found out. They use not the virgula divina, but dig always as the adventurers desire. They showed me one place, which they had digged straight on six years, when the ore was but two fathoms distant from the place where they first began: And in another place they digged 12 years outright, and at last found a vein, which in a short time paid their charges.

There is often found a red substance, which grows to the ore, called cinnaber, cinnaber of silver, cinnabaris nativa, minium nativum, or berg-cinober; of which I have sent you some by itself, and some also sticking to the ore. This substance ground down with oil makes a vermilion, equal to, if not surpassing, the cinnaber made by sublimation. I discovered a sulphur in it, by casting it upon a hot iron-plate, on which it burned blue. Whether it also contains quicksilver, I have not tried, because I would not diminish that small quantity here sent. The miners say they meet not with any.

There are also found in these mines, crystals, amethysts or amethistine mixtures in the clefts of the rocks, and sometimes nigh or joined to the ore; as also vitriol, naturally crystallized in the earth in divers of these mines, and particularly in a mine in Paradise-hill near Schemnitz.

As there is great variety in the silver ore, as to its mixtures with earth, stones, marcasite, cinnaber, vitriol, &c. so also in its richness, some holding a great proportion of silver in respect of other. A hundred pound weight of ore sometimes yields but half an ounce or an ounce of silver; sometimes 2 ounces, 3, 4, 5, and unto to 20 ounces. What is richer is very rare; yet some has been found to hold half silver, and I have seen of it so rich as to be cut with a knife.

As Chremnitz gold ore has silver in it, so most of the Schemnitz silver ore holds some gold; which they separate by melting the silver, then granulating it, and afterwards by dissolving it in aquafortis, whereby the gold is left at the bottom, and is afterwards melted; the aquafortis is distilled from the silver, and serves again for use.

The silver then separated from all its former associates, is sent to Chremnitz, where they coin it into pieces of a mixed metal (which is the common money of the country,) after this manner: They melt it with about the same quantity of copper, and run it into bars, which they beat out: then softening them in the fire, draw them out to an exact thinness between two steel wheels; then they cut them out into round pieces with an instrument like a shoemaker's punch, and then boil them with tartar and salt, shake them in a sack with small-coal and water, dry them in a kettle perforated, and afterwards they are drawn between two wheels, in which they receive their stamp.

Some Inquiries relating particularly to the Bleeding of Walnuts; suggested by Dr. EZEREL TONGE, *in a Letter to the Editor, March* 22, 1670. *N°* 58, *p.* 1198.

In these inquiries there is nothing sufficiently interesting for an extract.

Extract of a Letter from FRANCIS WILLOUGHBY, *Esquire, to the Editor, containing some Observations of his made on some Sycamore Trees, the Black Poplar, and the Walnut: as also his Thoughts about the Dwarf Oaks, and the Stellar Fish, described in N°* 57. *N°* 58, *p.* 1199.

I am sorry I cannot return you a better answer to yours of March 19; the experiments which our leisure has since permitted us to make, being not sufficient to found a new hypothesis on, to confirm Dr. Tonge's. Since the leaves have been unfolded, we have observed the sycamore after several frosty nights to bleed afresh in the morning, soon after sun-rising, when it had ceased several days before: though this must not be understood of all sycamores, but of some only that are more sensible and observant of the weather.

April 3 and 4, all the sycamores quite ceased.

The 5th, being after a white frost, they began to bleed about eight o'clock, and ceased towards noon.

The 9th, 12th, 13th, 15th they bled again.

The 10th, 11th, 14th were not observed.

This 16th they bled not, it being rainy and the sun not shining.

From the observations we have hitherto made, we think it may be certainly inferred, that a morning-sun after a frost will make all the bleeding tribe bleed afresh, though they had before ceased; and that this new bleeding towards the latter end of the season commonly ceases before morning. Possibly some may bleed after a frost, yet further in the summer.

I observed last year in August a copious and spontaneous exudation, very like bleeding, of a viscous yellow juice out of the buds of a black poplar. Our walnut-trees bleed here in January.

The star-fish, in the last Transactions, is the stella arborescens Rondeletti first described by him, and since by other naturalists.

There is no such dwarf oak in Old England, growing wild, as was sent you out of New England, nor in any other country where we have been, unless it be the ilex coccifera, which is a low shrub, bearing large acorns, and has a prickly leaf like holly. If it prove that, it will be a luciferous discovery.

An Account of some Books. *N°* 58, *p.* 1200.

I. Joh. Sig. Elsholtii, Elector. Brandenburg. Medici, Clysmatica Nova, sive Ratio in venam sectam Medicamenta immittendi. Coloniæ Brandeburgicæ. 1667, 8vo.

The title-page of this book, but very lately arrived in England, shows this to be the second edition; and the author in the conclusion of it tells the reader, that the first edition thereof was published in 1665, at which time he affirms there had been nothing printed (for aught he knew) either by English, French, or Italians, of this argument. To which we shall answer by referring the candid reader to what has been said already on infusing medical liquors, and transfusing blood, in several of these Tracts, as Number 7, p. 45, Number 20, p. 128, Number 22, p. 143, Number 35, p. 246.

II. Nicolai Hobokeni* Anatomia Secundinæ Humanæ, Ultrajecti. 1669, 8vo.

This author intending to inquire more narrowly into all the particulars concerning human generation, premises these observations, touching the human secundine, lately made by himself, and accompanied with 15 accurate schemes, drawn by his own hand, and representing, first, in a female fœtus, the placenta uteri, together with the membranes and string on both sides, where the same respects the uterus as well as the fœtus, and more particularly the membrane amnios, severed from the placenta, and the vessels running through the other

* This Dutch anatomist and physician was born at Utrecht in 1632. He held a medical professorship at Harderwick. He was author of the following treatises: Ductus Salivalis Blasianus in lucem protractus, 1662; Anatomia Secundinæ Humanæ, 1669, (and again with considerable emendations 1675); Anatomia Secundinæ Vitulinæ, 1670; Medicina Physiologica, 1685. Hoboken's Anatomical Descriptions of the Secundines are deemed excellent works; the figures are neat and accurate, and were drawn by himself. Prefixed to his treatise on Physiology is an Oratio de Nobilitate Medicorum.

membrane, chorion; then, that portion of the string, wherein the laxness, inequalities, and contorsions of the membrane, together with all the contained vessels in their proper situation, are observed; next, the macula, appearing somewhat prominent in one of the arteries, by which may easily be found the passage of the blood to the placenta, and its difficult regress towards the navel of the fœtus; moreover, the interior substance of the placenta, and all the ramifications of the veins and arteries.

Secondly, in a male fœtus, a review of all the former particulars, together with several differences from that of the female.

To these observations is subjoined a collection of letters, written by this author to divers learned men, touching generation, with their answers.

III. Joh. Ludov. Gansii, M.D. Coralliorum Historia. Francofurti, 1669. 12mo.

In this history the author affirms to have digested both what has been delivered of corals, and what himself has observed and tried of that curious product of nature. His opinion is, that coral is formed out of a glutinous juice, which being turned into stone by a salt, abounding in it, rises up in the form of a shrub; the salt being the cause that makes plants spread into branches.* The places of the production of coral he names to be the Red Sea, the Persian Gulf, the Sicilian and Neapolitan shore. Some of them are red, others white, others blackish. To this account he annexes various preparations made of them: as also divers solutions, of which he delivers one as an excellent dissolvent both of corals and pearls, made of very well rectified spirit of salt, digested and freed of its corrosiveness, and then mixed with good spirit of wine distilled and brought over the alembic.

The Answer of Mons. Mariotte to Mons. Pecquet, concerning the Opinion, that the Choroides is the Principal Organ of Sight. N° 59, p. 1023.

Sir,—I have in your answer † seen the reasons which hinder you from believing that the choroides is the principal organ of sight; but though they were very acute, and carried a great deal of probability with them, yet I did not find them strong enough to oblige me to grant back again this pre-eminence to the retina.

You say in your first objection, that if the sclerotis and the choroides be taken away from an eye that is very fresh, and the retina be left distended on

* This explanation of the formation of corals is very unsatisfactory. It is now known that coral-branches are the habitations of a peculiar tribe of polypous animals or zoophytes, and that the growth of these branches keeps pace with the growth and multiplication of the inhabiting animals.

† See No. 35, p. 243, &c. of this volume.

the vitreous humour, one shall not be able to see well through this membrane: whence you conclude, that it is not transparent enough, to let so much light as is sufficient for vision pass upon the choroides.

In my opinion there is good reason to doubt of this consequence, as there may be a great deal of difference between the retina of a dead animal, after it has been exposed to the air, and that of a living animal, while it is exactly shut up between the vitreous humour and the choroides. Different dispositions ordinarily change the qualities of things; fat, which is transparent when melted, grows opaque when cold; and the tunica cornea of an eye being held some hours in one's hand, in a hot air, grows thick, and a little after entirely opaque. But that you may be persuaded that the choroides is sufficiently enlightened in a living animal, you must take the eye of an ox, newly killed, while hot, and cut it in two in such manner that a good part of the vitreous humour may remain extended on the retina; then you shall see distinctly the colours of the choroides, the basis of the optic nerve, the trunk of the little vessels which proceed from thence, and their dispersion through the thickness of the retina, with so much perspicuity, that you cannot even discern whether there be a retina beyond the vitreous humour or not. Hence you may judge, that the light which the objects send to the choroides is more than sufficient for vision, seeing that being much weakened by the reflection, and by a second passage through the retina and the vitreous humour, it is yet strong enough to form a clear and distinct vision of the choroides in our eyes.

Your second experiment to prove the opacity of the retina, which is to put it into water, is also extremely deceitful.—For you make no doubt but the hyaloides, which envelopes the vitreous humour, is perfectly transparent; yet, if you lay in a dish, half full of water, part of the vitreous humour, the parts of the hyaloides which stick to it will appear whitish and thick, like a spider's web, although the vitreous humour do still retain its transparency. It is not then a sufficient proof to know whether the retina be opaque in a living animal, by putting it into the water: and by what trial soever you make of it, after it has been exposed to the air, you can draw no consequence to prove that it is opaque in its natural state; for the crystalline itself becomes a little thickish in water, and if it be left there some time, or exposed to the frost, it becomes white and opaque like snow.

It is therefore necessary for resolving our difference, and for knowing with certainty whether the light of objects pass almost all entire to the choroides, or whether it is almost all intercepted by the retina, to bring observations made on the retina and the choroides, while they are in their natural state, as I shall do in the following experiment.

Place by night a lighted candle very near your eyes; and cause a dog 8 or 10 paces distant from the candle to look upon you, then you will see in his eyes a light sufficiently bright, which I hold to proceed from the reflection of the light of the candle, the image of which is painted on the choroides of the dog, which having much whiteness and lustre, causes this very strong reflection; for if it proceeded from the crystalline or retina, the same appearance would be seen in the eyes of men, birds, and other animals, who have the choroides black, which is not found so by us. It is therefore manifest by this experiment, that the luminous rays pass with considerable force as far as the choroides, and that the retina receives very little impression. Now this appearance is made thus: the little picture of the candle which is upon the choroides of the dog, (where is the focus of the crystalline and other humours together) sending rays back through these humours, makes its reciprocal focus towards the candle, and by consequence the eyes which are near to the point, where these rays reunite, ought to see the crystalline of the dog very much illuminated.

I believe that darkness is not absolutely necessary for vision, but only for a strong vision: nor that the picture of objects ought to be expressed on the organ of vision; for, it is sufficient that the rays of each point of the objects be reunited in a distinct and separated point, according as they answer one another; and you will easily agree, that as a convex glass makes the image of the sun appear on white paper with a great deal of brightness and light, and on black paper very obscurely; although black paper, which soon takes fire, receives a great deal more impression than white; so the rays of illuminated objects do reunite on a whitish choroides, and express there a visible picture, and on a dark choroides a very obscure one, and which cannot be seen; but then the impression is also much stronger in the black than in the white; and this is the cause why men and birds see better and more distinctly than the greatest part of other animals; for their choroides being black, and by consequence very sensible of light, they contract much their pupil or sight-hole of the eye, which makes the rays that pass there, from each point of the object, to be all very near the axis of the crystalline, and to reunite more exactly in a point than in the eyes of most other animals, which have their choroides white towards the axis of sight, and by consequence less sensible of light, who in recompense can very much dilate the pupil of their eyes, when they stand in need of a great light; but also their sight is not so distinct, because the rays, which fall on the extremity of the crystalline, intersect the axis too near in their refraction.

It is true, that to supply in some sort this defect, they have a little crystalline in the middle of the great one, and this little crystalline being of a more spiss consistence than the great one, its refraction is also more strong, and makes the rays which come from one point in the axis, and pass near the centre of the crystalline, to refract more than if there had been but one crystalline. And by these means the greatest part of the rays, which fall upon the extremity of the great crystalline, intersect the axis; which causes their sight to be less confused, although it be never so distinct as that of men and birds, which have but one crystalline. Fishes have also a double crystalline, for otherwise their sight would be more confused than that of other animals who live in the air, for their crystalline being spherical, the rays cut the axis more unequally than if it were lenticular, and its convexities were of a greater sphere; and it ought necessarily to be spherical, because the refraction of the rays, which pass from the water into the crystalline, is very small, and would make its focus too far distant if the crystalline were lenticular.

The difficulty of your second objection proceeds also from an ambiguity of words, and consists in knowing what we are to call that which has a great continuity and communication with the brain. My hypothesis is, that the nerves are all coated with the pia mater, (which envelopes all the spinal marrow) and have with it the same continuity of fibres; so that if these nerves be never so little moved, the impression is carried to the brain by the continuity of these fibres; and, whether it be that the texture is different in nerves of different senses; or that they contain some spirituous liquors which determine their sensations, by some differences they have among themselves; it is certain, that the nerves of sight, in what manner soever they are moved, represent colours and light, those of hearing sounds, and those of the touch, pains, &c.

Now the choroides is an expansion and dilatation of the pia mater, which envelopes internally the optic nerve, and which comes from the tuberosity of the spinal marrow by a continuity of fibres; whence it follows, that how little soever the choroides be touched, the impression may be easily communicated to the brain; and that the same thing may be said of the retina, there must be a little channel in the optic nerve, through which the retina in its proper substance extends itself to this tuberosity by a continuity of fibres, which is not seen; and you are constrained to say, that there are little filaments of nerves which come from the interior of the optic, and expand themselves through the retina, which have this continuity; but if there were these filaments, they should spread themselves through the retina, as from a centre to a circumference, and should lie closer together near the optic nerve than a good way farther in the retina, which nevertheless is not observed to be the case.

Besides, if you thrust a pin through the thickness of the retina, you will often meet with filaments; but if you look on them through a convex glass, you will discover that they end in little vessels of veins and arteries, which are in the retina; and infallibly, if there were any nerves, you might find them in the same manner, and they would stop the pin, since they are as hard and firm as the little arteries; and when you say, that one may distinguish these filaments in the water, because the rest of the retina disappears, that is repugnant to experience and to what you have said before, viz. that the retina may be seen all white in the water, and without transparency; and you ought to show these filaments, or else we shall take them for a thing invented at pleasure.

You bring after that two experiments, the first of which is, that if an aperture be made on the upper part of the eye, we may discover the picture of the objects on the anterior surface of the retina; but if this aperture be made in the white of the eye, the aqueous humour will run to the tunica cornea, and make wrinkles, which will hinder the picture from being distinct; besides, he that looks in at this aperture, will hinder the rays of the object from passing into the eye, and he shall see nothing there but his own image. But if you mean, that the tunica cornea should be wholly taken off, there will not then be distance enough between the crystalline and the retina to make the picture distinct. In conclusion, I do not believe this experiment can be made, much less that it is to be discerned, whether this picture be formed on the anterior or posterior surface of the retina, since the thickness of it is less than half a line, or the 24th part of an inch; and there is reason to believe, that you have trusted to the report of some other concerning this experiment; or that you have believed the images which appear in the eyes to be painted on the retina, whereas they proceed from the reflection made on the exterior of the cornea.

Your second experiment is true and easy to be made, but according to you it were impossible; for, since you hold that it is on the anterior part of the retina that the picture is seen, and that you have elsewhere said, that one cannot well see through this membrane, it follows that you cannot see this picture through the thickness of the retina; but because I believe there remains transparency enough in that part of the retina which is not exposed to the air, I doubt not but the picture may be seen on the posterior part of it, after it is become sufficiently opaque, though in a living animal this picture passes as far as the choroides, as it has been already proved; and if the retina itself were taken away, and there remained only the vitreous humour, you might notwithstanding see the inverted picture of the windows toward the bottom of the eye, if you held it at the farther side of the room, in the same manner as the picture is to be seen in the focus of the spherical glass-bottle filled with water, though it seem to be on the exterior surface of the glass, and by consequence this experiment proves not the opacity of the retina.

In your third objection you quote my observation erroneously, for I wrote that the retina was about half a line in thickness, and not half a line precisely; which showed I had not measured it exactly; but if it were but a quarter of a line or less, it suffices that it had enough for the effect I did attribute to it.

You see then, Sir, that hitherto your objections have been able but very lightly to shake my opinion, and that the transparency of the retina is well enough established. Let us come now to the proof I make of the want of vision on the basis of the optic nerve. It must first of all be agreed, that in this experiment almost all men lose sight of an entire circle of white paper, whose diameter is about the 9th or 10th part of its distance from the eye. Now the visual triangle, whose basis is the diameter of the circle, and whose top is the centre of vision, is proportioned to the triangle, whose basis is the diameter of the picture of the circle on the bottom of the eye, and the top, the same centre of sight, which centre being distant six or seven lines from the basis of the optic nerve, whose breadth is about $\frac{3}{4}$ of a line, this basis also will be about the 9th or 10th part of its distance from the centre of sight, and by the principles of optics, the image of the circle of the white paper, falling on the basis of the optic nerve, will cover it precisely; and because the paper then wholly disappears, it follows, that all the basis of the optic nerve is insensible of light; whence I conclude, that the choroides is the principal organ of sight; and that the retina is not, seeing it is placed in that part, and is there apparently disposed in like manner as to the rest of the bottom of the eye.

To elude the force of this argument, you allege other causes of this fault of sight; the first two are almost like one another. But it seems to me that you suppose them without any ground: for, as I said before, there are no such filaments of nerves to be seen coming out of the basis of the optic nerve. The other cause which you bring, is the trunk of the vessels which proceed from the basis of the nerve. Yet you cannot deny but they are very small, and that it is very hard to discern the little holes through which they pass, when the nerve is cut off above its insertion into the eye; and because they often come out of the basis by two several little holes, the diameter of each of which does not take up the space of above the eighth part of the diameter of the basis; it follows, that if the rest of the nerve were sensible of light, we should not lose sight of a paper of two inches diameter at most, at ten feet distance: and sometimes in fixing one eye on a little piece of paper, two other very little ones, separated one from the other, would disappear; which is contrary to experience: for the default of vision is continued.

Other observations I add, as follow:

The first, which is very common, is, that the pupil dilates itself in the shade, and contracts itself in a great light, and it is very hard to find the cause of this involuntary motion, but by the hypothesis, that the choroides is sensible of light; for then it is easy to conceive, that being hurt by too strong a vision, it may dilate or contract its fibres, which have one continuity with those of the forepart of the uvea, so that it can contract its aperture, and when not hurt, relax it again: whereas if the retina be supposed to be the organ of sight, it will be very difficult to explain how this contraction is made.

The second is that of the glass bottle full of water, when a lighted candle is placed near its focus; for it is easy to prove that if one hold his hand between the candle and the bottle, he will feel more heat than if he held it in the reciprocal focus, that is to say, the place where the rays, which have passed through the bottle, make a great image of the flame of the candle appear inverted upon a white surface opposed to it; for I draw this consequence, that the image of a candle which is painted on the choroides of a dog, as I have proved to you, makes a much greater impression on the retina of the dog than on that of him that looks on it, and sees it very bright; whence I conclude, that if the retina were the organ of sight, this dog would not see the objects indifferently enlightened which are about the candle, although at three or four feet distance, because they would receive a great deal more impression from this reflection, and that a greater sensation obliterates a less; which is contrary to experience, and it is not at all likely there should be such a defect in the sight of animals.

The third is, that the eyes of birds are so framed, that the optic nerve, after its insertion into the eye, is inflected, and extends itself on the concavity of the sclerotis, about the breadth of two or three lines, more or less, according to the bigness of the eyes; and the length of this reflection is covered by the choroides, leaving but one little white streak in the middle, from whence the retina takes its original, which extends itself on the choroides through all the bottom of the eye; but it is covered on the side of this white streak with a little black membrane, as long as the inflexion of the nerve, and almost as broad, which proceeds from the pia mater, and is as it were an appendix of the choroides; and if you consider the situation of this membrane, you will find it is near the axis of sight, and that the rays of the objects which the birds look on with both eyes fall precisely upon it after their refraction. Since then, in the place where vision ought to be strongest, the retina is covered, and that no man doubts but birds are more clear-sighted than other animals, you ought to acknowledge, that the retina is not the principal organ of vision, but that that pre-eminence belongs to the choroides.*

* The arguments here employed are ingenious but by no means convincing. Some of the anatomical assertions are erroneous.

Concerning the Copper Mine at Herrngroundt in Hungary. By Dr. EDWARD BROWN. *N° 59, p.* 1042.

Herrngroundt is a little town, situated very high between two hills, upon a part of land of the same name, an Hungarian mile distant from Newsol. In this town is the entrance into a large copper-mine, very much digged.

I went in through a cuniculus, called Tach-stoln, and continued divers hours in the mine, and visited many of the most remarkable places in it. The steep descents in this mine are made by ladders or trees set upright, with deep notches or stairs cut in them, to stay the foot on. They are not troubled with water, the mine lying high in the hill, so that the water can drain away; but they are molested with dust and damps. The veins of this mine are large; many of them cumulate; and the ore very rich; in 100 pounds of ore they usually find 20 pounds of copper, sometimes 30, 40, 50, and even to 60 in the hundred. Much of the ore is joined so fast to the rock, that it is separated with much difficulty. There are divers sorts of ore, but the chief difference is between the yellow and the black: the yellow is pure copper ore: the black contains also a proportion of silver.

They find no quicksilver here: the mother of the ore is yellow; and the copper ore, heated and cast into water, makes it become like that of some sulphureous baths. They separate the metal from the ore with great difficulty. The ore commonly passes 14 times through the furnaces: sometimes it is burned, and other times melted; sometimes by itself, and sometimes mixed with other minerals and its own dross.

There are divers sorts of vitriol found in this mine, green, blue, reddish and white. There is also a green earth or sediment of a green water, called Berg-grun. There are likewise stones found of a beautiful green and blue colour, and one sort on which turcoises have been found; therefore called the mother of the turcoise.

There are also two springs of a vitriolate water, which are affirmed to turn iron into copper.* They are called the old and the new Ziment. These springs lie deep in the mine. The iron is usually left in the water 14 days.

An Account concerning the Baths of Austria and Hungary; also some Stone Quarries, Talcum Rocks, &c. By the same. N° 59, p. 1044.

Baden is a little city in Austria, four German miles southward from Vienna, seated on a plain, but near a ridge of hills, which are the excursions of mount Cetius. It is much resorted to for the natural baths of the place, where the

* *i. e.* The vitriolic acid having a greater affinity to the iron than it has to the copper, this last is precipitated in a reguline state upon pieces of iron that are thrown into the abovementioned vitriolate water.

springs are so numerous, as to afford convenient baths; two within the town, five without the wall, and two beyond a rivulet called Swechet.

The Duke's bath is the largest, about 20 feet square, in the middle of a house of the same figure built over it. The vapour passes out through a tunnel of wood at the top: and the water is conveyed into the bottom of the bath, at one corner, through wooden pipes and trees, under the town-wall from the spring head, which rises at a little distance westward. The springs of the rest of the baths rise under them, and are let in through holes of the flooring, for all the baths are wainscoted, the seats, sides, and bottoms being made of fir. The water for the most part is clear and transparent, yet somewhat bluish, and makes the skin appear pale in it, as the smoke of brimstone does. It discolours all metals (except gold, whose colour it also heightens) turning them black in a few minutes. The coin of this country, mixed of copper and silver, (having $\frac{7}{15}$ of silver, and $\frac{8}{15}$ of copper) is in a minute changed from white to a dark yellow, and soon after becomes black. To the moss and plants which it washes it gives a fine green colour, and leaves often a scum upon them of a purple mixed with white. As it runs from the spring head, it somewhat resembles the sulphur river in the way from Tivoli to Rome, but is not so strong or stinking, nor does it incrustate its banks.

The spring head rises under a rocky hill, at some distance from the entrance into it: for I passed to it, about the length of 40 yards, through an arched passage cut in the rock, which is also a natural stove, (as that of Tritola and Bajæ) made by the hot bath water running under it. Most part of this cave is incrustated with a white substance, by them called saltpetre. At the mouth of the cave it becomes harder and stony. I caused some of the pipes, through which the bath water runs, to be opened, and from the upper part of the pipe I took some quantity of fine sulphur in powder, somewhat like flower of brimstone; this being, as it were, sublimed from the water, and not deposited, being found in the upper part of the pipe. Oleum Sulph. per campanam dropped into this water, is received into it quietly. Oleum tart. per deliquium causes an ebullition, as in the making of tartarum vitriolatum.

The second bath within the wall is that of our Lady, about 12 feet broad, and 24 long. One end of it is under a church of the same name. This is fuller of sulphur than the rest, and more blue, and leaves a yellow flower on the boards, as the others do a white one. The third is the new bath, out of the town, near the gate; which when I saw was full of people singing. The fourth, the Jews' bath, which has a partition in the middle, to separate the men from the women. The fifth, St. John's bath, of a triangular form. The sixth, the beggars' bath, always shallow, so that they lie down in it. The seventh, the bath of the Holy-cross, about two fathoms square, chiefly for the clergy. The eighth, St. Peter's bath, greener than the rest. The ninth, the Sour bath, set about with stone balistres, and covered with a handsome cupola and lanthorn. The water is very clear. In the steam of this bath I have often coloured money black without touching the water; and staying only in the room where the bath is, the buttons of my clothes and what else of silver the vapour could come at, were coloured yellow or gilded; and yet the water itself, once cold, changes not the colour of metals, though boiled in it. The hottest of these baths have not the heat of the Queen's bath at Bath in England. They use no guides, as with us, but direct themselves with a short turned staff.

Manners-dorff, seated under a hill on the east side of the river Leyta, has only one bath. It rises under a church, built over the spring head. The water of it is lukewarm, and therefore they boil it in great coppers, when they desire it hotter, and bathe in tubs filled with this boiled water. From the substance which sticks to the coppers in boiling, it appears that it is impregnated with sulphur, saltpetre and chalk.* This water colours the stones in it of a fair green, like a turcoise; and the steam of it, which sticks to the moss under the church, turns into drops of gold or amber colour.

Dotis, two Hungarian miles from Comora in Hungary, famous for being watered with great numbers of springs, has also sulphureous baths, said to be warm in winter. I was there in March and October, and both times found their warmth scarcely perceivable. In colour they are bluish and to taste acid. The Queen's bath and the great bath rise in a marsh, northward of the castle. There is another bath in the governor's garden, within the town. They are used as those of Manners-dorff, by being boiled, and poured into bathing tubs.

At Banka, two Hungarian miles from Freistat, in a meadow, I took notice of 15 baths: and there have been more, but the river Waag wears away the banks and swallows up the baths, and it has also broke into three of these 15. The water of these is like to that of Baden in Austria: it leaves a white sediment on the moss and places it washes, and tinctures the metals black; which I experimented by putting money into it; and sticking some into the ground over which the water passes, that part which was in the ground retained its own colour, and the other part in the bath-water acquired a coal black. These baths are open and very hot.

The baths of Boinitz, nigh the river Nitra, in Hungary, are of a moderate gentle heat, delightful to bathe in, much beautified by Count Palfi Palatine of Hungary: and all of them covered under one large roof. The first is the no-

* This account of the ingredients of this mineral water is not to be relied on.

bleman's bath, built of stone, descended into on all sides by stone stairs. Four more are of wood, but very handsomely and well built.

At Stuben, three Hungarian miles from Newsol, and two from Chremnitz, near a rivulet, are some baths of great esteem, and much frequented. The water is clear, and smells of sulphur, the sediment green. It colours the wood over it green and black, but does not change the colour of metals so soon as most others. I left money in it a whole night, which was yet but faintly coloured. The springs rise underneath, and pass through the holes in the flooring of the baths. The heat is the same as the King's bath in England. These baths are seven. The first is the nobleman's bath. The second the gentleman's. The third the countryman's. The fourth the countrywoman's. The fifth the beggars' bath. The sixth for such as are infected with the lues venerea. The seventh, the bath of the gypsies, of whom there are many in those parts. These baths are in a plain, encompassed on all sides with hills.

Glas-Hitten, an Hungarian mile, or about seven English miles from Schemnitz, has five baths; two of which are large. It deposes a red sediment, and incrustates the wood and seats of the bath under water with a stony substance; and it gilds silver. But the most remarkable of these baths is that which is called the sweating bath, whose hot springs drain through a hill, and fall into a bath built to receive them; at one end of which, by ascending I went into a cave, which is made a noble stove by the heat of these thermæ, and so ordered with seats that every one who sits in it, either by chusing a higher or lower seat, may regulate his sweating, or enjoy what degree of heat he pleases. This cave and the sides of the bath also, are covered by the continual dropping of these hot springs with a red, white and green substance: the red and green make the best show, but the white is used against the stone, and cures ulcers and sore backs of horses.

Eisenbach, about four English miles from Glas-Hitten, and five or six from Schemnitz, has also hot baths. I have seen great trees placed at the top or superficies of the water in the bath, which have suffered petrifaction. Here are two convenient baths much frequented; and a third, which is made by the water let out of the former, called the snakes' bath, from the number of snakes coming into and delighting in it, when it is filled with these warm waters.

The natural baths of Buda are esteemed the noblest of Europe, not only in respect of the large and hot springs, but of the magnificence of their buildings also. For the Turks bathe very much, and though little curious in most of their private houses, yet are they very sumptuous in their public buildings, as their chans or caravansaras, mosques, bridges and baths declare. Here are eight baths. The first is a large open bath, at the foot of a high rocky hill, formerly called Purgatorium, of which the people have some odd and scrupulous apprehensions. The second is covered with a cupola, and stands near the same hill, but more into the town, and near a place where they use tanning. The third is called the bath of the green pillars, though at present they are of a red colour. The water is hot, but tolerable without addition of cold water. It is impregnated with a petrifying juice, which discovers itself on the sides of the bath, on the spouts and other places, and makes a gray stone: and the exhalation from the bath, reverberated by the cupola, by the irons extended from one column to another, and by the capitals of the pillars, forms long stones like icicles, which hang to all the said places; such as may be observed in many subterraneous grottos, and particularly in England in Okey Hole in Somersetshire, and Pool's Hole in Derbyshire. The water is let out at night, when the women have done bathing, who often stay late. The bath is set round about with large pillars supporting a cupola, which has openings to let out the steam, and yet the whole room continues to be a hot stove.

The baths of the west end of the town are, 1. Tactalli or the bath of the table; a small bath covered; the water white, and of a sulphureous smell. They drink of this as well as bathe in it. What they drink, they receive from a spout, bringing the water into this place. I delivered a five sols piece to a Turk bathing in it, to gild for me, which he did in about a minute, by rubbing it between his fingers while the hot water fell from the spout upon it. 2. Barut Degrimene, or the bath of the powder-mill. It rises in an open pond near the highway, and mixes with the fresh springs, so that the pond is of a whitish colour in one part, and clear in the other, as also cold and hot in several parts. 3. Cuzzoculige, the little bath, or the bath of the Saint; for which name the Turks give a superstitious reason. It is kept by Turkish Monks. The bath where the springs rise, is so hot as scarcely to be endured; but being let out into another bathing place at some distance, it becomes tolerable, and fit for use. This water has neither colour, smell, nor taste different from common water, and deposes no sediment; only the sides of the bath are green, and have a fungous substance all over. 4. Kaplih, a very noble bath, but part of the building was consumed this year (1669) by a great fire which happened in Buda; but is by this time repaired by the Turks. The water is very hot, not without a petrifying juice in it. The building about is octangular, with a noble bath in the middle; with a circle of a trench of water about it for the better ornament. On every side it has a niche wherein is a fountain. In the middle of the antichamber, where they leave their clothes, there is also a fair stone basin and a fountain. 5. The bath of Velibey; which has a strong sulphureous smell, and a petrifying quality; and is so hot, that to make it tolerable, it requires the addition of cold water. This is the noblest bath of any, the antichamber very large, the bath room capacious, and high arched, and adorned with five cupolas: one a very fair one over the great round bath in the middle; and one lesser over each of the four corners, where are either baths or bath stoves for more private use: in these the Turks take off the hair of their bodies by a psilothrum mixed with soap; it being not their custom to have any hair, except on their beards, and a lock on the crown of their heads. Twelve pillars support the great cupola, between eight whereof are fountains of the hot water, and between the other are places to sit down, where the barbers and bath men attend. And each of these places has two cisterns of free-stone, into which are let hot bath water, and also cold water, to be mixed and tempered as every one likes.

Men bathe in the morning, and women in the afternoon. When any man intends to bathe, having entered the first rooms, he finds there divers servants attending, and furnishing him with a cloth and apron. Then he puts off his apparel, and having put on the apron, he enters the second room, where the great bath is, and sits on the side of the bath, or between the pillars nigh a fountain; where the barber strongly rubs him with his hand opened, stretching out his arms, and lifting them up, after which the party bathes. Then, if he be a subject of the Grand Signor, or it be the custom of his country, he has his head shaved, and if a young man his beard, except the upper lip. Next, the barber rubs his breast, back, arms, and legs, with a hair-cloth, while he either sits or lies with his face downward; then washes his head with soap, and after throws cold water upon him all over his body; and so the party walks about in the steam of the bath for a time.

These baths are made use of two ways, either by entering into the water, or sitting about the bath in the steam. For the vapour of the bath makes the whole room a stove; and most sweat as long as they stay in it, and some enter not the water at all, but have it poured on them; or else only continue in the steam of the bath, which sufficiently produces sweat.

So much for the baths. Upon the side of mount Calenberg, towards the north, are stones marked with trees and leaves. In the hermitage of the Camaldulensis, seated on a peak of this hill, I saw fine specimens, with which they paved the walk in their gardens. This place is two German miles from Vienna.

Not far from Manners-dorff is the Emperor's quarry of stone, out of which are made the best buildings in Vienna: In which, wheresoever there is a cleft or separation of one stone from another, the water falling between them leaves a petrification, as it were healing the wound, by making a stony callus.

An English mile from Freistat in Hungary northward, is a quarry of stone, out of which many large stones are dug, transparent, and resembling sugar-candy.

At Banka, two Hungarian miles from Freistat northward, is a quarry of white stone, next the hot baths of that place; over which is a layer of chalk, of about a yard thick, very beautiful to the eye, as being of all colours, except green; so finely mixed, streaked, and shaded, that it surpasses marble-paper; and the water dropping upon it varnishes it over.

At Schemnitz in Hungary, famous for silver mines, is a high perpendicular rock, part of which from the top to the bottom, is naturally tinctured with a shining fair blue and green: And I have heard from a Spaniard, who lived long in the West Indies, that there is also a rock like this, near the silver mines in Peru.

The mountain of Clissura, being a part of mount Hæmus, as also mount Pyrlipe, shine like silver, and day and night, either by the light of the sun or moon, afford a pleasant glittering show, caused by the great quantity of Muscovy-glass, wherewith these hills abound. There are also talc rocks over Spital in Upper Carinthia, as I have been informed by M. Donellan, who resides there. I am unwilling to omit a hill nigh, Sarvizza, two days journey on this side Larissa, which consists of an earth of a fine red colour, out of which the red earthen vessels of that country are made; as also the great number of acidulæ nigh Transchin in Hungary, there being 32 plentiful springs of them; likewise a hot bath nigh Bellacherqua in Bulgaria, it being situated far from any habitation, yet well built by the Turks, and very refreshing to travellers. It has a red sediment, and makes a gray stone.

An Account of some Books. N° 59, p. 1051.

I. Cosmopœia Divina, seu Fabrica Mundi explicata, per Ludov. de Beaufort, Parisinum Med. Doct. Lugd. Batav. 1656, in 12mo.

This author professes his aversion from devising any other system of the world, than that which is described by Moses; and employs his labours and endeavours to adjust and accommodate all the phænomena of nature to the mind of that divine writer.

II. Cartesius Mosaizans, Auth. Joh. Amerpoel. Leowardiæ, 1669, in 12mo.

The design of this author is to prove, that the philosophy delivered by the famous Descartes well agrees, at least not disagrees, with the History of the Creation, recorded by Moses. A design which that eminent philosopher entertained himself, and would have set upon, if death had not prevented him, as appears in his letters to Mersennus, viz. the 24th and 53d of the 2d vol. where he affirms, that comparing his principles with the Mosaic History, he had found to his wonder, that the latter could be much better explained according to the former, than by any other of all those ways wherein interpreters have hitherto explained it. To evince this, our author has taken the pains to make a parallel between the first chapter of Genesis, and the principles of Descartes.

A Letter of John Evelyn, Esq. to the Right Hon. Lord Viscount Brouncker, Chancellor to her Majesty and President of the Royal Society, &c. concerning the Spanish Sembrador, or New Engine for Ploughing, and equal Sowing all Sorts of Grain, and Harrowing, at once; by which a great Quantity of Seed-corn is saved, and a rich Increase yearly gained; together with a Description of the Contrivance and Uses of this Engine. Englished out of Spanish, and lately presented to the Royal Society. N° 60, *p.* 1056.

This paper contains the description and use of a drill plough, for sowing corn at equal depth and distance, &c. It was invented by an Austrian, but carried into Spain, and there made use of. The long account of it is here omitted, as useless at this time; being far inferior to the drill ploughs now employed in England.

An Account of a Halo seen at Paris: also, on the Cause of these Meteors, and of Parhelias or Mock Suns. By M. Huygens. N° 60, *p.* 1065.

This halo, or circle about the sun, was observed at Paris, March 12, 1667, about nine o'clock in the morning.

The diameter of this circle was 44 degrees, and the breadth of its limb about half a degree. The upper and lower part were of a vivid red and yellow, with a little purple colour, but especially the upper; the red was within the circle. The other parts appeared but whitish and of little clearness. The space within the halo was a little darker than that about it, especially towards the parts that were coloured. Besides, there was seen the portion of another great circle, which touched the halo above, and whose extremities were bent downward, as is represented in fig. 1, pl. 12. This portion of a circle had also its colours like those of the halo, but fainter. The height of the sun, at the beginning of the observation, was about 46 degrees. There were in the air little clouds, which somewhat tarnished the blue colour of the sky, and lessened the brightness of the sun, which seemed as in an eclipse. The weather was cold, considering the season of the year, and it was affirmed for certain, that it had frozen the night before. This halo appeared in the same beauty and splendor of colours unchanged, from nine in the morning till about half an hour past ten; after which time it became fainter and fainter, till two o'clock in the afternoon, when it ended, after it had resumed a little more force some time before it disappeared.

The observation of this phenomenon engaged M. Huygens to propose to the company, assembled at his Majesty's library, what he had meditated some years ago, concerning the cause, not only of these halos, but also of the parhelia, which have been hitherto considered by many as prodigies, and as prognostics of some singular event.

As for the halos, he said, that they were formed by small round grains, made up of two parts, one transparent, the other opaque, the latter being inclosed in the former, as a cherry-stone is in a cherry; as may be seen in the fig. 2, where A A represents one of these grains, and B the kernel or opaque part. He related the observations of those who have seen hail formed after this manner, and among others, that of M. Descartes, in his Treatise of Meteors; and explained how some of these little grains, which swim up and down in the air between us and the sun, being less distant from the axis, which extends itself from the sun to our eye, than of a certain angle, do necessarily hinder the rays, which fall on them, from coming to our eyes, since the opaque kernel is the cause that there is behind every such grain a space of a conical figure, as MNO in fig. 2, in which the eye of the spectator being situated, cannot see the sun through that grain, though it may see it when posited elsewhere, as somewhere in P.

And to make the company the more distinctly to understand the effect which these grains suspended in the air must produce, he drew the 3d figure; in which B is the place of the eye; BA, the axis which passes from the eye to the sun; C, M, F, some of the icy grains with their kernel, making them semi-opaque: Among which, the grain C being in the axis BA, and the lines CK, LH, representing the rays of the sun nearest to the axis, the passage of which is not hindered by the opacity of the kernel, it is certain, not only that the grain C will not be able to transmit any ray of the sun towards B, but also that conceiving the superficies of a cone, whose top is in the eye, and its sides BD, BE, parallel to the rays CK, LH; all the grains MM, which this superficies may comprise, will likewise not suffer any ray to pass to the eye, because it must needs be in their cone of obscurity; but those that are without this superficies, as the grains FF, will let them pass, because the eye is without their cone of obscurity. Whence it follows, that the angle of this cone BDE is that which determines the diameter of the halo, which depends on the proportion the opaque grain has to the transparent, in which it is inclosed. For if this diameter is of 44 degrees, as is observed in most halos, the size of the opaque grain will be to the transparent, as 40 to 19. But he remarked that this proportion was not always the same, and that the diversity of it was the cause that sometimes there were seen many halos, one about the other, all having the sun for their centre.

He added, that it was easy to know why these halos were always of a round figure, whether the sun be little or much raised above the horizon; as also to give the reason for their colours, which is the same with that in the triangular glass-prisms; as is evident by the tangents AC, drawn to the grain A, at the points where the ray DA enters or comes out. He also noticed, that it was manifest why the red colour is in the interior circumference of the halo. And why the space which it takes in, and chiefly near the most lively coloured parts, appears obscurer than the air about, viz. because it is there where most grains are, which transmit no rays of the sun to our eyes, and so only darken the air, as the drops of water when it rains. He farther noted, that M. Descartes, endeavouring to explain the cause of these halos, had committed a mistake, for want of observations, truly relating to this last circumstance: Because he maintains that the space comprised within the halo is clearer than the air without; and to render a reason of it, he supposes certain grains, altogether transparent, having the form of a lens, which supposition cannot therefore be true, because what he deduces from it, is contrary to what is observed: Besides that the roundness of the halo, in all the elevations of the sun, agrees not with it, as is easily shown.

As to the arch of the circle, which above touched the last halo seen May 12, as also that the colours were more vivid in this place, and in that below, than in the rest of the circle; he said, that these effects did not proceed from the grains he had been speaking of, but from another cause, which also serves for the production of the parhelia, and the circles which almost always accompany them. Touching which circles and parhelias, he remarked, that besides the round and semi-opaque grains, there were also formed in the air certain little cylinders of the like nature, and of which M. Descartes himself declared, in his Treatise of Meteors, he had observed some, not indeed with opaque kernels within, but that the same cause which produces them in the round grains, could also produce them in cylinders: Which being supposed to be such as fig. 4 represents them, viz. oblong icy grains, and roundish at both ends, having the inner kernel of the same shape, it was found, that from their different dispositions all the appearances of the parhelia and their circles did necessarily follow.

And first, that some of these cylinders being erect, in the situation which probably they ought to have in being formed, there must appear in the heavens a large white circle, parallel to the horizon, passing through the sun, and of near the same breadth with him; as has been observed in the phenomenon of Rome, An. 1629, of which Gassendus and Descartes have written, and which is here exhibited in fig. 5. That this circle L K N M is caused by the reflection of the rays of the sun on the surface of these cylinders; it being easy to demonstrate, that there are none but those which are raised at the same angle above the horizon with that of the height of the sun, that can reflect his rays to our eye. Whence it manifestly follows, that it must appear white, and throughout of equal altitude with the sun himself, and consequently parallel to the horizon. That considering afterwards the transparency of these perpendicular cylinders, and their opaque kernels, it is easily seen that those of the white circle, which are distant from the sun at a certain angle, begin to give passage to his rays to strike our eyes, in the same manner as has been said of the round half-dark grains. That these cylinders are those which on each side of the sun make us see a parhelion in the large white circle, as has been noted in the observation of Rome (where they are marked with K and N) and in many others. That these parhelia have commonly luminous tails; because the cylinders which follow those first ones that form the parhelia, and which are yet further distant from the sun, let also pass his rays to our eye, so that these tails may be 20 degrees and more in length. That the same parhelia are always coloured, because they are made by refraction, as the halo.

That besides, there are two other images of the sun, generated by these perpendicular cylinders, and so disposed in the large white circle, that the spectator turning his face towards the true sun, has them behind him; as in the Roman observation are the parhelia L and M. That these are produced by two refractions and one reflection in these cylinders, in the same manner as the ordinary rainbow in the drops of water, according as M. Descartes has declared: so that the opaque kernels do nothing to the production of these two suns, but that they may be sometimes so large as to make them not appear. That according to the altitude of the sun, more or less, these two parhelia are more or less near to one another. That they should appear coloured, as the rainbow, and that sometimes they have been seen such; but that when they are faint they may also seem white, even as the halos when they are not very bright. That these same perpendicular cylinders can also produce a halo about the sun, by reason of the rounding of their two ends; so that, being distant from the sun at a certain angle, on what side soever it be, they begin from thence to give passage to the rays, transmitting them to the eyes of the spectator. And that these halos are probably those we see almost always pass through the two parhelia, that are on the side of the true sun, as the halo GKNI in the phenomenon of Rome.

That there is yet another situation of these cylinders very considerable, which is so as their axes are parallel to the horizon, but turned divers ways, like needles confusedly thrown on the ground: which horizontal disposition is very natural to those cylindric bodies, supported by the vapours which rise from the earth, as may be made out experimentally in bodies, thus figured, being let fall in the air.

That it is in these cylinders that the arches, which touch the halos above or below, are formed; such as there were in the phenomenon observed at Rome, An. 1630, which is described by P. Schenir in a letter to M. Gassendus, as also in all those which M. Hevelius has related at the end of his Mercurius in Sole. And that the arch which appeared on this last halo at Paris, was of the same kind. That the figure of these arches is different according to the different altitudes of the sun, and the several magnitudes of the diameters of the halos. That when the sun is very near the horizon, such an arch appearing on an ordinary halo of 44 degrees, must show like two horns, as in fig. 6, AB, AC: but that the sun rising higher, those horns become lower in proportion, and make such arches as are represented in the same figure, where each height of the sun is marked near the arch which it is to make. That the place of the arches where they touch the halos, being more strongly enlightened and coloured than the rest, makes us judge that there are parhelia in those places.

That the reason why these arches do usually touch a parhelion, was that the same horizontal cylinders, which produce the arch, produce also that parhelion by means of their two round and transparent ends; in the same manner as has been said of the perpendicular cylinders. And that the parhelion last seen at Paris had been formed in these couchant cylinders. That that was also confirmed by its being brighter in the superior and inferior part, than any where else; which necessarily comes to pass in a parhelion caused by cylinders thus disposed; whereas when produced by the round grains, it must appear every where equally strong.

That in these same cylinders parallel to the horizon, there is also found the cause of the white cross, observed with the paraselenes or mock moons, by M. Hevelius, and exhibited at the end of his Mercurius in Sole: the perpendicular fillet of that cross, coming from the reflection of the rays of the moon on the surface of these cylinders; as the other fillet, parallel to the horizon, is produced by the reflection of the perpendicular cylinders which make the great white circle, of which this fillet is a part. That yet the moon must not be very high above the horizon, so that the couching cylinders may produce this effect.

That besides the perpendicular cylinders, and those that are parallel to the horizon, there are often a great many which move to and fro in the air in all sorts of positions; and that those, for the same reason as the round grains, must produce a halo about the sun, and even a more vivid one than that which is caused by the grains, forasmuch as each cylinder sends many more rays to the eye, than each of these little spheres. That the little halo DEF, in the Roman phenomenon fig. 5, may very well have been caused by such cylinders.

As to those mock suns, which sometimes show themselves directly opposite to the true sun, that he could find nothing, neither in the round grains nor in the cylinders, which should make these suns necessarily to meet in the great white circle, parallel to the horizon; and that if that should be always verified by future observations, the cause of it must be looked for elsewhere.

That for the generation of these suns, he supposed a number of small cylinders with opaque kernels, like the foregoing; which were carried in the air, neither perpendicularly, nor couching, but inclined to the plane of the horizon at a certain angle, being near half a right one; to which were particularly appropriated those cylinders which M. Descartes saw fall from the heavens, having stars at both ends; as may be seen experimentally by forming cylinders of that fashion which is represented in fig. 7, and letting them descend in the air or in water. That in these cylinders was found not only the cause of the anthelia made by the intersection of two arches, as in fig. 8, but also that of some other extraordinary arches and rods, that are sometimes observed near the sun, of which notwithstanding there could nothing be as yet affirmed with certainty, for want of exact and faithful observations.

To make all these different effects of the cylinders manifest to the eye, M. Huygens produced one of glass, a foot long, of the shape of that in fig. 4; and for the opaque kernel in the middle, a cylinder of wood, and the ambient space filled with water, instead of transparent ice: which cylinder being exposed to the sun, and the eye put in proper places, there were successively seen all those reflections and refractions above-mentioned. Whence it might be concluded, that a great number of the like cylinders, although very small in comparison to that, being found in the air, and having the several postures that have been supposed, all the appearances of the parhelia and their circles must exactly follow.

A Discourse of Dr. R. Wittie, relating to the Notes of Dr. Foot in N° 52, and to those of Dr. Highmore in N° 56 of these Tracts; concerning Mineral Waters, and Extracts made out of them. Communicated to the Editor. N° 60, *p.* 1074.

Useless, and for the most part erroneous conjectures and reasonings, concerning the chemical composition of the Scarborough mineral waters.

An Account of some Books. N° 60, *p.* 1083.

I. The Divine History of the Genesis of the World, explicated and illustrated. London, 1670, in 4to.

This author, not thinking fit to give us his name, takes no small pains to explain in this his book the Genesis of the World, as it is delivered by Moses, esteemed by him the only true philosopher.

II. Franc. Travagini, Super Observationibus a se factis tempore ultimorum Terræ-Motuum, ac potissimum Ragusiani, Physica Disquisitio, seu Gyri Terræ Diurni Indicium. Lugduni Batavorum, 1669, in 4to.

This Venetian philosopher acquaints the curious, in this book, with some observations made by himself in two late earthquakes, and by others also in the last about Ragusa, whence he thinks an argument may be drawn to confirm, among other proofs, the diurnal motion of the earth.

His observations are, that in those earthquakes he found, besides a subsulting perpendicular motion, found by others, a concomitant lateral one, from west to east; which latter he conceives was not caused by the former, but only discovered by it; just as the progressive motion of a boat, carried with a still stream, is not produced, but only made sensible, from an accidental check, to a person that shall have been put in it asleep, when it was at rest, but awakens, after it was made to swim down a still river; who will think himself unmoved, till the boat meet with some stop, whereby for the time its course will either be hindered or disturbed, and he made sensible of his being in motion.

He alleges divers other observations, made of rivers, suspended bells, and church lamps, which were all observed by himself and many others, to have likewise the said lateral and vibrating motion. Whereupon he admonishes his readers that they would, on the like occasions, take very particular notice of all the several motions in earthquakes; and then consider with themselves, whether from such observations universally made, importing that the earth in earthquakes is vibrated towards the east, and that that vibration cannot proceed from its succussation, (which is only able to cause a perpendicular motion in the trembling earth,) it may be validly inferred, that the earth has a diurnal progressive motion from west to east.

III. Quæstio Triplex de Anno Mense et Die Christi Nati, Baptistati et Mortui. Auth. R. P. Michaele Seneschallo è S. J. Leodii, 1670, in 4to.

This author undertakes to prove, in this chronological treatise, not only the year and month, but also the day and hour of the nativity of Christ, and of his baptism and death.

IV. Hermanni Grube, M. D. Commentarius de Modo Simplicium Medicamentorum Facultates cognoscendi. Hafniæ et Francofurti, 1669, 8vo.

In this treatise there is nothing interesting to medical men of the present times.

V. De Lacte Lunæ Dissertatio Medica, Johannis Danielis Majoris, Ph. et M. D. Kiloni, 1667, 4to.

By the word lac lunæ nothing else is meant here than flores argenti, or a fine white porous and friable earth, insipid and without scent, dissoluble in water, and tinging it with a milky colour, and sometimes raising a kind of ebullition in it, found commonly in silver mines, and in them sublimed and sticking to the roofs of the rocky hills; having a drying and abstersive quality, and therefore good against the afflux of sharp humours in ulcerated parts, serving also for an excellent cosmetic. All which particulars are at large deduced and discoursed upon by the author; who observes, that Gesner in his book of Fossils takes good notice of this mineral earth, and affirms it to be found in the mountains of Helvetia, especially that which is called mount Pilate. From whom he thinks that others, as Boethius a Boot, Olaus Wormius, Aldrovandus, Calceolarius, and others, have taken the hint.

He makes the matter of this earth to be the metallic vapours of silver ore, by some fermentation raised and sublimed, and then condensed.*

Extract of a Letter of Dr. John Wallis *to* Robert Boyle, *Esq. concerning the Doctor's Essay of Teaching a Person Dumb and Deaf to speak and to understand a Language, with the Success thereof. N° 61, p.* 1087.

The task consists of two very different parts; each of which renders the other more difficult. For, besides that which appears on the first view, to teach a person who cannot hear to pronounce the sound of words; there is that other,

* What is here termed lac lunæ appears to have been a calcareous earth. It was not soluble in water, but merely diffusible through it. Its supposed origin from "the metallic vapours of silver fermented, sublimed and condensed," is an egregious error.

of teaching him to understand a language, and to know the signification of those words, whether spoken or written, whereby he may both express his own sense, and understand the thoughts of others, without which the former were useless.

But to this disadvantage of teaching a first language, that of deafness must needs augment the difficulty: since it is manifestly evident from experience, that the most advantageous way of teaching a child his first language, is that of perpetual discourse, not only what is particularly addressed to himself, as well in pleasing diversions or delightful sports, and therefore insinuates itself without any irksome or tedious labour, as what is directly intended for his more serious information; but that also which passes between others, where, without pains or study, he takes notice of what actions in the speaker accompany such words, and what effects they produce in those to whom they are directed, which by degrees insinuates the understanding of those words.

And, as that deafness makes it the more difficult to teach him a language; so on the other hand, that want of language makes it more hard to teach him how to speak or pronounce the sounds. For there being no other way to direct his speech, than by teaching him how the tongue, the lips, the palate, and other organs of speech, are to be applied and moved in the forming of such sounds as are required, to the end that he may by art pronounce those sounds, which others do by custom, it may be thought hard enough to express in writing, even to one who understands it very well, those nice curiosities and delicacies of motion, which must be observed, though we need it not, by him, who without help of his ear to guide his tongue, shall form that variety of sounds we use in speaking; many of which curiosities are so nice and delicate, and the difference in forming those sounds so very subtle, that most of ourselves, who pronounce them every day, are not able without a very serious consideration to give an account by what art or motion ourselves form them, much less to teach another how it is to be done. And if, by writing to one who understands a language, it be thus difficult to give instruction, how, without the help of hearing, he may utter those sounds, it must needs increase the difficulty, when there is no other language to express it in, but that of dumb signs.

As to the first part, though I did not doubt but that the ear as much guides the tongue in speaking, as the eye does the hand in writing, or playing on the lute; and therefore those who by accident wholly lose their hearing, lose also their speech, and consequently become dumb as well as deaf; yet since we see that it is possible for a lady to attain so great a dexterity as in the dark to play on a lute, though to that variety of nimble motions, the eye's direction, as well as the judgment of the ear, might seem necessary to guide the hand, I did not think it impossible but that the organs of speech might be taught to observe their due postures, though neither the eye behold their motion, nor the ear discern the sound they make.

And as to the other; that of language might seem yet more possible. For as in children, every day, the knowledge of words, with their various constructions and significations, is by degrees attained by the ear, so that, in a few years, they arrive to a competent ability of expressing themselves in their first language, at least as to the more usual parts and notions of it; why should it be thought impossible, that the eye (though with some disadvantage) might as well apply such complication of letters or other characters, to represent the various conceptions of the mind; as the ear, a like complication of sounds? For though, as things are, it be very true that letters are, with us, the immediate characters of sounds, as those sounds are of conceptions: yet is there no reason, in the nature of the thing itself, why letters and characters might not as properly be applied to represent immediately, as by the intervention of sounds, what our conceptions are.

Which is so great a truth, that it is practised every day, not only by the Chinese, whose whole language is said to be made up of such characters as to represent things and notions, independent of the sound of words; and is therefore differently spoken, by those who differ not in the writing of it: like as what, in figures, we write, 1, 2, 3, for one, two, three; a Frenchman, for example, reads *un, deux, trois*: But, in part, also amongst ourselves; as in the numeral figures now mentioned, and many other characters of weights and metals, used indifferently by divers nations to signify the same conceptions, though expressed by a different sound of words: And more frequently, in the practice of specious arithmetic, and operations of algebra, expressed in such symbols, as so little need the intervention of words to make known their meaning, that, when different persons come to express, in words, the sense of those characters, they will as little agree on the same words, though all express the same sense, as two translators of one and the same book into another language.

And though I will not dispute the practical possibility of introducing an universal character, in which all nations, though of different speech, shall express their common conceptions; yet, that some two or three (or more) persons may, by consent, agree upon such characters, whereby to express each to other their sense in writing, without attending the sound of words; is so far from an impossibility, that it must needs be allowed to be very feasible, if not easy. And if it may be done by new-invented characters, why not as well by those already in use? Which though to those who know their common use, may signify sounds; yet to those that know it not, or do not attend it, may be as immedi ately applied to signify things or notions, as if they signified nothing else: And consequently, so long as it is purely arbitrary, by what character to express such a thing or notion, we may as well make use of that character or collection of letters, to express the thing to the eyes of him that is deaf; by which others express the sound or name of it to those that hear. So that, indeed, that shall be to him a real character, which expresses to another a vocal sound; but signifies to both the same conception: Which is, to understand the language.

I shall add this also, that the person to be taught could once speak, though so long ago, that I think he scarcely remembers it. But having, by accident, when about five years of age, lost his hearing, he consequently lost his speech also; not all at once, but by degrees, in about half a year's time: by which I was very certain that his want of speech was but a consequence of his want of hearing, and did not proceed originally from an indisposition in the organs of speech to form those sounds. And though the neglect of it in his younger years, when the organs of speech, being yet tender, were more pliable, might now render them less capable of that accurateness which those of children attain unto: yet, if he can attain to speak but so well as a foreigner, at his years, may learn to speak English; what shall be farther wanting to that accurateness which a native from his childhood attains unto, may, to an indifferent estimate, be very well dispensed with.

But as to the other branch of our design, concerning the understanding of a language: I see no reason at all to doubt, but that he may attain this, as perfectly as those that hear; and that, allowing the like time and exercise, as to other men is requisite to attain the perfection of a language, and the elegance of it, he may understand as well, and write as good language as other men; and no whit inferior to what he might attain to, if he had his hearing as others have. And what I speak of him in particular, I mean as well of any other ingenious person in his condition; who, I believe, might be taught to use their book and pen as well as others, if a right course were taken to that purpose.

As to that of speech; I must first, by the most significant signs I can, make him to understand in what posture and motion I would have him apply his tongue, lips, and other organs of speech, to the forming of such a sound as I direct. Which if I hit right, I confirm him in it: If he miss, I signify to him, in what he differed from my direction; and, to what circumstances he must attend to mend it. By which means, with some trials, and a little patience, he learns first one, then another sound; and, by frequent repetitions, is confirmed in it; or, if he chance to forget, recovers it again.

And for this work, I was so far prepared beforehand, that I had heretofore, upon another occasion, (in my treatise De Loquela, prefixed to my grammar for the English tongue,) considered very exactly the accurate formation of all sounds in speaking, without which, it were in vain to set upon this task. For if we do not know, or not consider, how we apply our own organs in forming those sounds we speak, it is not likely that we shall this way teach another.

It remains now, for the perfecting the account which at present you desire of me, only to tell you what progress we have already made. He has been already with me somewhat more than two months; in which time, though I cannot be thought to have finished such a work, yet the success is not so little, as to discourage the undertaking: but as much as I could hope for in so short a time, and more than I expected. So that I may say, the greatest difficulty of both parts being almost over, what remains is little more than the work of time and exercise. There is hardly any word, which with deliberation he cannot speak; but, to do it accurately, and with expedition, we must allow him the practice of some considerable time.

And, as to the language; though it were very indifferent to him who knew none, which to begin with, yet, since it is out of question, that English to him, is likely to be the most useful and necessary; it was not advisable to begin with any other. For though he can pronounce the Latin with much more ease, as being less perplexed with a multitude of concurring consonants; yet this is a consideration of much less importance than the other. To this therefore having applied himself, he has already learnt a great many words, and, I may say, a considerable part of the English, as to words of most frequent use: But the whole language being so copious, though otherwise easy, will require a longer time to perfect what he has begun.*

* The person to whom the foregoing letter refers, is Mr. Daniel Whaley (son of Mr. Whaley, late of Northampton, and mayor of that town.) He was on the 21st of May 1662, present at a meeting of the R. S. and did in their presence, to their great satisfaction, pronounce distinctly enough such words as by the company were proposed to him; and though not altogether with the usual tone or accent, yet so as easily to be understood. About the same time also (his Majesty having heard of it, and being willing to see him) he did the like several times at Whitehall, in the presence of his Majesty, his highness Prince Rupert, and divers others of the nobility, though he had then employed but a small time in acquiring this ability. In the space of one year, which was the whole time of his stay with Dr. Wallis, he had read over a great part of the English Bible, and had attained so much skill as to express himself intelligibly in ordinary affairs; to understand letters written to him, and to write answers to them, though not elegantly, yet so as to be understood: and in the presence of many foreigners (who out of curiosity have come to see him,) has often not only read English and Latin to them, but pronounced the most difficult words of their languages, even Polish itself, which they could propose to him.

Nor is this the only person on whom the doctor has shown the effect of his skill, but he has since done the like for another, a young gentleman of a very good family, and a fair estate, who from his birth wanted his hearing. [Note inserted from the original.]

A Relation concerning the Sal-Gem Mines in Poland. N° 61, *p.* 1099.

The mines of Sal-Gem in Poland are a mile distant from Cracovia, near the small town of Wilizka, which, the church excepted, is altogether one hollow under ground. There are four descents or holes, each four or five feet long, and as broad, lined downwards through with timber. Above is a great wheel, with a strong rope, of the thickness of a lusty arm, drawn about by a horse, as in a horse-mill.

They first descend perpendicularly by a rope to the depth of 100 fathoms. They afterwards come to certain ladders, by which they go down 100 fathoms deeper, where there are double passages and holes, one above another, in abundance; for the mine-men dig on still, and cut out every where and on all sides as long as the salt vein lasts and salt is found; but the vein being lost, and no more salt appearing in one place, they search for other salt veins, whence come so many holes and passages out of one into another. The great holes, to secure both the town above and the work below from falling in, are very carefully filled out and supported by strong and well-compacted timber, of which there is enough in those works to build a large town with.

Out of these mines they dig and cut three sorts of salt; one is common, coarse, and black; the second somewhat finer and whiter; the third very white and clear, like crystal. The coarse and black salt is cut out in large pieces, roundish, and three Polonian ells long, and one ell thick, which costs fifty to seventy Polonian florins. The great pieces lie at Cracow about the streets, before the doors of the citizens; as also in the country, in the small towns and villages, and before the forts and houses of the nobility; where the cattle passing to and fro, lick of those salt stones; which afterwards by mills and other engines are ground and beaten small for use.

The colour of these salt stones is darkish grey, with some mixture of yellow. The instruments with which they are dug and cut out, have almost all German names with Polonian terminations; for when this salt work was first found, which is now above four hundred years ago, the mine-men that first began to work in it were Germans; whence the Poles have retained those names of the tools, but given them Polish terminations.

These salt works belong to the king of Poland, who appoints and maintains the officers of them; and it is one of his best royal revenues, amounting to considerable sums of money. There is no less than a thousand men that are constantly employed in these mines, and there was then a provision of salt valued at two millions.

There are in these works three horses, that stay always below, having their stable and other necessaries there; they carry the salt from the places where it is cut and dug out, to the rope, whence it is by the wheel drawn up, by a horse above ground, going round about. The horses, after they have been a while under ground, grow blind from the sharpness of the salt, and all the three, which then laboured there, were quite blind; and one of them that had been longest in those mines had the hoofs of his feet grown as long again as they are usually, so that each hoof was near a span long.

This salt work has also beneath it certain salt springs, whence the salt water is by channels conveyed to several places, where it is boiled to salt.

There is another mineral salt work in Poland, viz. at Bochna, but not so well ordered as the former. Besides there are divers other places in Poland, and in Russia also, which yield salt; as at Holitz, Colomeja, Solum, Pintz, Oswentz, &c. In the Podolian desert, near the river Boristhenes, is a salt lake, whose water is by the heat of the sun dried up, and turned to salt, so that the people there ride into it with horses and waggon s, like unto ice, and cut it into pieces and carry it away.

The Way of making Vinegar in France. Communicated by an ingenious Physician of that Nation. N° 61, p. 2002.

They take two large casks, within each of which they put at the bottom a trevet, which must be one foot high, and as large as the size of the cask permits. On this trevet they put vine twigs, whereon they lay a substance called rape, with which they fill both vessels within half a foot of the top. This rape is nothing but the wood or stalks of the clusters of grapes, dried and freed from the grapes. The trevet and the vine branches are put at the bottom of the casks, only to keep the rape from settling at the bottom. It is this rape which alone heats and sours the wine. The two vessels being almost quite filled with the rape, one of them is filled up with wine, and the other only half full for the time: and every day they draw by a cock half the wine that is in the full vessel, therewith quite to fill up the other that is but half full, observing interchangeable turns of filling and unfilling the vessels. Ordinarily, at the end of two or three days, the half-filled vessel begins to heat, and this heat augments for several days successively, continuing to do so till the vinegar is perfectly made; and the workmen know that the vinegar is made by the ceasing of the heat. In summer it is a work of fifteen days: in winter it proceeds more slowly, and that according to the degree of cold weather.

When the weather is hottest the wine must be drawn twice a day, to put it out of one vessel into the other. It is only the half-filled cask that heats, and as soon as they have done filling up, its heat is choaked and stopped for the time, and the other cask, which is unfilled, begins to heat. The full vessel is quite open at the top, but a wooden cover is put on the vessel that is but half full. The best wine makes the best vinegar; but yet they make good vinegar of wine that is turned.

The wine in changing leaves a certain grease, which sticks partly to the sides of the cask, (and that they take care to do clean away) partly to the rape: so that if they cleanse not the rape from it almost every year once, the wine turns into a whitish liquor, which is neither wine nor vinegar. At the time when they pour the wine out of one vessel into the other, a scum arises on the top of the vessel, which must be carefully taken away. In the casks, which have never served for this purpose before, the vinegar is made more slowly than in such as have been used.

As soon as the rape is separated from its grapes, which is done immediately after vintage, it is carefully put up in barrels, lest it take air, and heat itself, and be spoiled. Rape will serve a year, more or less, provided care be taken to clear away every morning with a piece of linen the grease that is on the sides of the vessel, and with a little broom, that which swims on the top of the liquor. The rape may be freed from its grease with water, rubbing it between the hands.

An Account of some Books. N° 61, p. 2005.

I. Mechanica, sive de Motu Tractatus Geometricus; Pars Secunda; in qua, De Centro Gravitatis ejusque Calculo: Auth. Johanne Wallis, SS. Th. D. &c. Londini, 1670, in 4to.

In this second part the author demonstrates the nature and place of the centre of gravity. He shows also, from general principles, how by calculation to determine, as well the magnitude, as the centre of gravity, in innumerable sorts of lines, surfaces, and solids, all right-lined figures whatsoever; in all solids bounded by plains; in cones also and cylinders: And in curve-lined figures innumerable; not only (with Archimedes) in the parabolar figure, but likewise in all paraboloids whatever; with their ungular solids insisting on them; and their conoids or other solids made by the conversion of those plains about any axe in the same plain assigned; and the centre of gravity of all these solids. And the like also in other figures reciprocal to these paraboloids, infinitely continued between such curves and their asymptotes: Showing which of those figures, infinitely long, are of finite magnitude, and what that is; which, of infinite: and, which of them have, which have not centres of gravity; and, in those which have, how to assign them: And the like of the ungulas appertaining to them, and the solids made by their conversion about an axe: In many of which, the magnitude of the ungula or solid is shown to be but finite, where the magnitude of the respective plain (on which they stand, or by whose conversion they be made) is infinite: and, contrarywise, in others, the magnitude of the ungula or solid to be infinite, where that of the plain is but finite.

He gives also general methods, from the centre of gravity of a plain, or of lines, to find the magnitude of the ungulas and solids made by rotation about assigned axes. He shows particularly, that the scalene cylindric surface, is to the erect, as the perimeter of an ellipse to that of a circle. He shows the centre of gravity of all arches of circles, with their superficial ungulas, and the surfaces made by the conversion of such arches about assigned axes. And the like of the sectors, segments, and other portions of circles, which are applicable also to those of ellipses; with the ungulas and solids made by such conversion; and their centres of gravity. He does the like in the cycloid, showing the length of the curve, and of the portions thereof, with their centres of gravity, and their several superficial ungulas, and surfaces, made by conversion and the centres of gravity of all these, &c.

He does the like in the figure of right sines, in the figure of versed sines, and of arches; assigning the magnitude of those figures, and of their segments and portions; with their ungulas, and solids by conversion; and the centres of gravity of all these. Whence (amongst many other things) are deduced the sums of the right sines, versed sines and arches, appertaining to any assigned portion of a circle; and the sums of their squares, cubes, or other powers.

He does the like in spiral figures, as well that of Archimedes, as an infinite number of other spirals; showing the magnitude of the several parts or sectors; with their centres of gravity; and the respective paraboloids. He prosecutes the same in part, but more briefly, in the cissoid and conchoid, and the figure of tangents; as to the magnitude of those figures, and the parts thereof; their ungulas, solids, and centres of gravity.

He shows also the quadrature of the hyperbola, and parts thereof; their ungulas, solids, and centres of gravity: As also an hyperbolical solid, made by the conversion of a straight line about an axis not in the same plain; showing the magnitude of that solid, and of its parts, and their centres of gravity; and the several sections of that solid made by plains in any assigned position; being parabolas, hyperbolas, ellipses, circles, parallelograms, and triangles; according to the different positions of the cutting plains, &c.

II. Exercitationes Mechanicæ, Alexandri Marchetti. Pisi, 1669, in 4to.

This author declares, that though many eminent men have already treated of the subject of his book, as Aristotle, Archimedes, Lucas Valerius, Guldinus, Galileus and others; yet he thinks he has handled it more fully, more distinctly, and easily.

III. The Natural History of Nitre, or, a Philosophical Discourse of the Nature, Generation, Place, and Artificial Extraction of Nitre, with its Virtues and Uses, by William Clarke. London, 1670, in 8vo.

There is nothing in this treatise worthy the attention of modern chemists.

New Pneumatical Experiments about Respiration, by the Honourable ROBERT BOYLE. *N° 62, p.* 2011.

The First Title.

Observations on the lasting of Ducks included in the Exhausted Receiver.

Nature having, as zoologists teach us, furnished ducks and other water-fowl with a peculiar structure of some vessels about the heart, to enable them, when they have occasion to dive, to forbear for a while respiring under water without prejudice; I thought it worth the trial, whether such birds would much better than other animals endure the absence of the air in our exhausted receiver. The accounts of which trials were registered as follows.

Exp. I.—We put a full grown duck into a receiver, of which she filled a third part or somewhat more, but was not able to stand in an easy posture in it; then pumping out the air, though she seemed at first to continue well somewhat longer than a hen in her condition would have done; yet within the space of one minute she appeared much discomposed, and between that and the second minute, her struggling and convulsive motions increased so much, that her head also hanging carelessly down, she seemed to be just at the point of death; from which we presently rescued her by letting in the air upon her: So that this duck being reduced in our receiver to a gasping condition within less than two minutes, it did not appear that she was able to hold out considerably longer than a hen, or other bird not aquatic, might have done: and to manifest, that it was not closeness and narrowness of the vessel, in reference to so bulky an animal, that produced in the subject of our trial the great and sudden change above recited, we soon after included the same bird in the same receiver, and having by a special way cemented it on very close, we suffered her to stay thus shut up with the air for five times as long as formerly, without perceiving her to be discomposed; and she would probably have continued longer in the same condition, if we had tried it.

Exp. II.—Having procured a duckling, that was yet callow, we conveyed her into the same receiver wherein the former had been included, and observed, that, though for a while she appeared not much disquieted, whilst the air was pumping out of the glass, yet before the first minute was quite ended, she gave manifest tokens of being much disordered; and the operation being continued a while longer, she grew so much worse, that several convulsive motions which she fell into before a second minute was expired, obliged us to let in the air upon her, whereby she quickly recovered.

N. B. I determine not whether it be proper in this place to add, that when the receiver was pretty well exhausted, the included bird appeared to the spectators manifestly larger than before air was withdrawn, especially about the crop, though that was very turgid before. And to manifest, that in this duck, as in the former, the convulsions that used to be immediately followed by death, proceeded from the withdrawing of the ambient air, and not from the clogging of it; we kept the same duckling in the same receiver very close to keep out all external air, and to keep in the excrementitious steams of her body for above six minutes, without perceiving her to grow sick upon her imprisonment; which yet lasted above thrice the time that sufficed to reduce her in the absence of the air to a gasping condition.

N. B. It not being intended, that ducks and other water-fowl should, any more than other birds, live in an exceedingly rarefied air, but only be able to continue upon occasion a pretty while under water, it may suffice that the contrivance of those parts, which relate to respiration, be so far fitted for the purpose, as we shall see it when we come to the tenth title.

The Second Title.

Of the Phænomena afforded by Vipers included in an Exhausted Receiver.

Considering that vipers are animals endowed with lungs (though of a different structure from those of men, dogs, cats, birds, &c.) and that their blood is actually cold; I thought it might, upon both those accounts, be very well worth trying what effect the withdrawing and absence of the air would have upon animals so constituted. I therefore made divers trials, some of which did not displease me; but I know not by what misfortune the memorials of them were lost, except two or three, which I shall here subjoin though imperfect.

Exp. I.—Jan. 2, 1662-3. We included a viper in a small receiver, and as we drew out the air, she began to swell, and afforded us these phænomena. 1. It was a good while after we had left pumping, ere the viper began to swell so much as to be forced to gape, which afterwards she did. 2. That she continued, by our estimate, above $2\frac{1}{2}$ hours in the exhausted receiver without giving clear proof of her being killed. 3. That after she was once so swelled, as to be compelled to open her jaws, she appeared slender and lank again, and yet very soon after appeared swelled again, and had her jaws disjoined as before.

Exp. II.—We took a viper, and including her in the largest sort of small receivers, we emptied the glass very carefully, and the viper moved up and down within, as if to seek for air, and after a while foamed a little at the mouth, and left some of the foam sticking to the inside of the glass: her body swelled not considerably, and her neck less, till a considerable time after we had left pumping; but afterwards the body and neck grew prodigiously tumid, and a blister appeared upon the back. An hour and a half after the exhaustion of the receiver, the distended viper gave manifest signs of life; but we observed none afterwards. The tumor reached to the neck, but did not seem much to swell the under-chop. Both the neck and a great part of the throat being held betwixt the eye and the candle, were transparent enough where the scales did not darken them. The jaws remained mightily opened and somewhat distorted; the epiglottis with the rimula laryngis (which remained gaping) was protruded almost to the further end of the nether-chop. As it were from beneath this epiglottis came the black tongue, and reached beyond it, but seemed by its posture not to have any life, and the mouth also was grown blackish within: but the air being readmited after 23 hours in all, the viper's mouth was presently closed, though soon after it was opened again, and continued long so; and scorching or pinching the tail made a motion in the whole body, that argued some life.

Exp. III.—April 25. To these experiments upon vipers, I shall add one made upon an ordinary harmless snake. We included such an animal, together with a gage, in a pretty portable receiver, which being exhausted and well secured against the ingress of the air, was laid aside in a quiet place, where it continued from 10 or 11 o'clock in the forenoon, till about nine the next morning; and then my occasions calling me abroad, I looked upon the snake, which though he seemed to be dead, and gave no signs of life upon the shaking of the receiver, yet upon holding the glass a convenient distance from a moderate fire, he did in a short time manifest himself to be alive by several tokens, and even by putting forth his forked tongue. In that condition I left him, and by reason of several avocations, came not to look upon him again till early in the afternoon of the next day, at which time he was grown past recovery, and his jaws, which were formerly shut, gaped exceedingly wide, as if they had been stretched open by some external violence.

The Third Title.

Of the Phænomena afforded by Frogs in an Exhausted Receiver. Sept. 9, 1662.

The same considerations that induced me to make trials upon vipers, invited me also to make several upon frogs; the success of some of which, the following notes will declare.

Exp. I.—We took a large lusty frog, and having included her in a small receiver, we drew out the air, and left her not very much swelled, and able to move her throat from time to time, though not so fast as when she freely breathed before the exsuction of the air. She continued alive about two hours, that we took notice of, sometimes removing from the one side of the receiver to the other; but she swelled more than before, and did not appear by any motion of her throat or thorax to exercise respiration, but her head was not very much swelled, nor her mouth forced open. After she had remained there somewhat above three hours, perceiving no sign of life in her, we let in the air upon her, at which the tumid body shrunk very much, but seemed not to have any other change wrought in it; and though we took her out of the receiver, yet in the free air itself, she continued to appearance dead. Nevertheless to see the utmost of the experiment, having caused her to be laid upon the grass in a garden all night, the next morning we found her perfectly alive again.

Exp. II.—June 29, 1660. About 11 of the clock in the forenoon, we put a frog into a small receiver, containing about 15¼ ounces, troy weight of water, out of which we had tolerably well drawn the air, (so that when we turned the cock under water, it sucked in about 13¼ ounces of water,) the frog continued in it (the receiver all the while under water) lively enough till about five of the clock in the afternoon, when it expired. The frog at first seemed to be not much altered by the exsuction of the air, but continued breathing both with her throat and lungs.

Exp. III.—Sept. 6, 1662. We included in a pretty large receiver a couple of frogs newly taken, the one not above an inch long, and proportionally slender, the other very large. Whilst the air was drawing out, the lesser frog skipped up and down very lively, and clambered up several times to the sides of the receiver, insomuch that he sometimes rested himself against the side of the glass. When his body seemed to be perpendicular to the horizon, if not in a reclining posture, he continued to skip up and down a while after the exsuction of the air, but within a quarter of an hour we perceived him to lie dead with his belly upwards. The other frog, that was very large and strong, though he began to swell much upon the withdrawing of the air, and seemed to be distressed by his frequently leaping up after the air was drawn out, which he did not before, yet held out half an hour, at which time it was remarkable that the receiver, though it had held out against the pressure of the outward air, during that space of time, notwithstanding that a piece of it had been cracked out, and was mended with a cloth dipt in cement, yet at the end of the half hour, the weight of the outward air suddenly beat it in, and thereby brought the imprisoned frog a reprieve, which hindered us from bringing the experiment to an issue.

Exp. IV.—Sept. 11. We took a small frog, and having conveyed her into a very small portable receiver, began to pump out the air. At first she was lively enough, but when the air was considerably withdrawn, she appeared to be very much disquieted, leaping sometimes after an odd manner, as it were to get out of the uneasy prison, but yet not so, but that after the operation was ended, and the receiver taken off, the frog was perfectly alive, and continued to appear so near an hour, though the abdomen was very much, and the throat somewhat extended; this latter part having also left that wonted panting motion, that is supposed to argue and accompany the respiration of frogs. At the end of about 3¾ hours, after the removal of the receiver from the pump, the air was let in; whereupon the abdomen, which by that time was strangely swelled, did not only subside, but seemed to have a great cavity in it, as the throat also proportionably had; which cavities continued, the frog being gone past all recovery.

Exp. V.—April 14. A large frog was conveyed into a plated receiver, and the air being withdrawn, her body by degrees was distended; as appeared, when by a casual springing of a leak, the air got in again, and made her look much more lank and hollow than ever. The receiver with the gage were kept under water near seven hours, because I was obliged to stay long abroad; at the end of which coming home I found the receiver staunch, but the frog dead and exceedingly swelled: upon the letting in of the air, she became more hollow and lank than ever.

N. B. I have purposely, both under this title and some others, subjoined some trials, whose events are not altogether such as others, recited under the same head, would invite one to expect; but I purposely do it, not only to be true to the impartiality I proposed to myself in writing these narratives, but to awaken the curious to consider and observe what variety of phænomena, in such trials, may be attributed to the season of the year wherein they are made; and to the strength, bulk, age, peculiar constitutions, &c. that relate to the respective animal on which the experiments are made; besides, what things may on other accounts be fit to be also considered.

The Fourth Title.

Of the Phænomena afforded by a newly kittened Kitling in the Exhausted Receiver.

Being desirous to try whether animals, that had lately been accustomed to live either without any, or without a full respiration, would not be more difficultly or slowly killed by the want of the air, than others, which had been longer used to a free respiration; we took a kitling that had been kittened the day before, and put it into a very small receiver (which we guessed to hold about a pint or less) that it might be the sooner exhausted. As soon as the pump began to play, I took notice of the time, and found by a watch, that within one minute or a little more after the air first began to be withdrawn, the little animal, who in the mean time had gasped for life, and had some violent convulsions, lay as if dead, with his head downwards, and his tongue out; but upon letting in of the air, he immediately showed signs of life, and being taken out of the receiver quickly recovered: And to allow him the benefit of his good fortune, we sent for a kitling of the same age and litter, which being put into the same receiver, quickly began, like the other, to have convulsions, after which he lay as dead; but observing very narrowly, I perceived some little motions, which made me conclude him alive, in which I was not mistaken. For though we continued pumping, and could not perceive that the engine leaked more than in the former experiments; the kitling began to stir again, and after a while had stronger and more general convulsions than before; till at the end of full six minutes after the exsuction of the air was begun, the animal seeming quite dead, the outward air was readmitted into the receiver, which not reviving him as it had done the other, he was taken out of the vessel, and lay with his mouth open, and his tongue lolling out without any sensible breathing and pulsation; till having ordered him to be pinched, the pain or some internal motion, produced by the external violence, made him immediately give manifest signs of life, though there was yet no sensible motion of the heart or the lungs; but afterwards gaping and fetching his breath in an odd manner, and with much straining, as I have seen some fœtuses do, when cut out of the womb, he little by little, within about a quarter of an hour, recovered: wherefore thinking it severe to make him undergo the same measure again, we sent for another, kittened at the same time, and inclosing that also in the receiver, observed that divers violent convulsions, as it were gasping for breath, into which he began to fall at the second or third suck, ended in a seeming death, within about a minute and a half. But being made more diffident by the late experiments, I caused the pump to be plied, the rather, because I had a mind to observe whether, when the air was from time to time drawn away, there would not, upon the opening of the stop-cock to let it out, appear some sudden swelling of the body of the animal, by the spring and expansion of some air (or aërial matter) included in the thorax, or the abdomen. Such an inflation, though not great, we thought we observed; but till further trial I dare not acquiesce in it. A while after, notwithstanding our continuing to pump, the kitling gave manifest signs of life, which was not till it had endured divers convulsions, as great as those of the first fit, if not greater. When seven minutes from the beginning of the exhaustion were completed, we let in the air; upon which the little creature, that seemed stark dead before, made us suspect that he might recover; but though we took him out of the receiver, and put aquavitæ into his mouth, yet he irrecoverably died in our hands.

These trials may deserve to be still further prosecuted by others, to be made not only with such kittens, but with other very young animals of different kinds; for, by what has been related, it appears that those animals continued three times longer in the exhausted receiver, than other animals of that size would probably have done.

The Fifth Title.

Some Trials about the Air usually harboured and concealed in the Pores of Water, &c.

It might assist us in making the more rational conjectures about the phænomena of divers of our experiments, if we knew what quantity of aërial substance is usually found in the liquors we employ about them, especially in that most common of them, water. And therefore, though it be very difficult, (if at all possible) to determine the proportion of the air that lurks in water with any kind of certainty, many circumstances making it subject to vary very much, yet to make the best estimate I easily could, where none at all that I know of has been hitherto made by any man, I considered that it might afford us some light, if we discovered at least what proportion as to bulk, the air latent in a quantity of water would have to the liquor it came from, when the aërial particles should be gathered together into one place. For, though about this union, and the spring that may be consequent to it, some doubts may be suggested, which I have not now time to discuss; yet I supposed that at least some discoveries would by this way be made, though not of the true proportion between the air and the water, yet about two or three particulars, in due time to be taken notice of.

To find instruments, which would any way accommodate our purpose, proved a very difficult work; so that among other things that we were fain to do, this was one, that to evince how little the air latent in water did appear to lessen the bulk of that water, if it were suffered to fly away in an open tube; we suffered it to escape in an exhausted receiver without any artifice to catch it; by which trial the water did not sensibly lose any part of its bulk. Wherefore we endeavoured to make this loss visible by some other trials, of which I can find but a few hasty memorials among my loose entries.

A chemical pipe, sealed at one end, about 36 inches in length, was filled with water, and inverted into a glass vessel, not two inches in diameter, and but $\frac{1}{4}$ of an inch or little more in depth. These glasses being conveyed into a fit receiver, and the air being leisurely pumped out, and somewhat slowly re-admitted, the numerous bubbles, that had ascended during the operation, constituted at the top an aërial aggregate, mounting to $\frac{8}{10}$ wanting about 100th part of an inch.

Presently after, the tube (hereafter to be described) was filled again with the same water, and inverted, and the water being drawn down to the surface of the vesseled water, and the air let in again, the water was impelled up to the very top within a tenth and half a tenth of an inch.

The tube for measuring the air latent in water was $43\frac{1}{2}$ inches above the surface of the stagnant water: the air collected out of the bubbles at the top of the water, was the first time $\frac{3}{4}$ of an inch, and somewhat better; the second time we estimated it but $\frac{1}{3}$ and $\frac{1}{10}$. The first time the water in the pipe was made to subside full as low as the surface of the restagnant water: the second time the lowest that we made it subside, seemed to be four or five inches above the surface of the water in the open vessel.

Matter of fact thus recited would afford several difficulties worthy to be considered, which I have not leisure to discuss; especially the odd thing that happens to the aërial particles of water: For though, whilst they lay concealed in the water, they took up so little room in it, that it was insensible, and when they were permitted to escape out of the tube, the water was not manifestly diminished by their recess; yet when they were associated at the top of the tube, their aggregate did sometimes maintain a place, that was considerable enough in reference to the capacity of the whole tube; though I must here notice, that this aggregate did at the top of the tube possess more room than its bulk absolutely required, because it was somewhat defended from the pressure of the atmosphere by the weight of the subjacent cylinder of water, which might be about three or four feet long.

Query. Whether any considerable proportion of bubbles will be afforded by the same liquor, if it be suffered to continue in the glass for some competent time, after it has been once or oftener freed from bubbles already?

Query. How far it may be worthy our consideration, whether in common water there may not be concealed air enough to be of use to such cold animals as fishes; and whether it may be separable from the water that strains through their gills?

But though I was at first content to make use of this way of estimating the air concealed in water, yet when I came where I could be a little better accommodated with glasses, I bethought myself of a small instrument, that would much better disclose the quantity of the aërial particles I designed to discover. The structure and use of this glass may be easily enough understood by the recital of the first experiment that was made with it, as follows:

We provided a clear round glass, with a pipe or stem of about nine inches in length, the globular part of the glass being on the outside about $3\frac{1}{2}$ inches in diameter; the pipe of this glass was, within an inch of the top, melted at the flame of a lamp, and drawn out for two or three inches as slender as a crow's quill, that the decrement of the water upon the recess of the air harboured in its pores, might, if any should happen, be the more easily observed and estimated. Above this slender part of the pipe, the glass, as was before intimated, was of the same size (or near it) with the rest of the pipe, that the aërial bubbles, ascending through the slender part, might there find room to break, and so prevent the overflowing or loss of any part of the water.

This vessel being, not without difficulty and some industry, filled, till the liquor reached to the top of the slender part, where not being uniformly enough drawn out, it was somewhat broader than elsewhere, we conveyed the glass, together with a pedestal for it to rest upon, into a tall receiver, and pumping out the air, numerous bubbles disclosed themselves, ascending nimbly to the upper part of the glass, where they made a kind of froth or foam; but by reason of the above-mentioned figuration of the vessel, they broke at the top of the slender part, and so never overflowed.

This done, the pump was suffered to rest a while, to give the aërial particles lodged in the water time to separate themselves and emerge, which when they had done a pretty while, the pump was plied again, for fear some air should have stolen into so large a receiver. These vicissitudes of pumping and resting lasted for a considerable time, till at length the bubbles began to be very rare, and we weary of waiting any longer; soon after which the external air was let into the receiver, and it appeared somewhat strange to the spectators, that notwithstanding so great a multitude of bubbles as had escaped out of the water, I could not, by attentively comparing the place where the surface of the water rested at first with that where it now stood, discern the difference to amount to above a hair's breadth, if so much, and the chief operator in the experiment professed that, for his part, he could not perceive any difference at all.

Thus far for the narrative of the trial made by water; but that was not the only liquor into whose aërial particles I designed by our little instrument to inquire; and therefore filling a glass of the same shape, and much of the same size, with claret, and placing it upon a convenient pedestal in a tall receiver, we caused some of the air to be pumped out, whereupon in a short time there emerged through the slender pipe such a multitude of bubbles, darted, as it were, upwards, as much pleased and surprised the beholders; but it forced us to go warily to work for fear the glass should break, or the wine overflow. Wherefore we seasonably left off pumping, before the receiver was any thing near exhausted, and suffered the bubbles to get away as they could, till the present danger was overpassed, and then from time to time we pumped a little more air out of the receiver, till we were weary, the withdrawing of a moderate quantity of air at a time sufficing, even at the latter end, to make the bubbles not only copiously but very swiftly ascend, for above a quarter of an hour together.

The little instrument, made use of about these trials, being designed for the examination, among other things, of the quantity of bubbles lurking in several liquors, is to be applied to spirit of wine and chemical oils, which are more subtle liquors than wine itself. And some circumstances of our trials made us think that it might be worth examining what kind of substance may be obtained by this way of handling aërial and spirituous corpuscules. But of the other uses of our instrument elsewhere.

The Sixth Title.

Of some Phænomena, afforded by Shell Fishes in an Exhausted Receiver.

Exp. I.—An oyster being put into a very small receiver, and kept in long enough to have successively killed three or four birds or beasts, &c. was not thereby killed, nor, for aught we could perceive, considerably disturbed, only at each suck we perceived that the air contained between the two shells broke out at their commissure, as we concluded from the foam which at those times came forth all round that commissure. About twenty-four hours after, coming to see in what condition this oyster was, I found that both this and another that had been put at the same time into the receiver were alive, but how long afterwards they continued so, I did not observe.

Exp. II.—That same day we put a pretty large crawfish into a pretty large receiver, and found, that though it had been injured by a fall before it was brought hither, yet it seemed not to be much incommoded by being included, till the air was in great measure pumped out, and then its former motion presently ceased, and it lay as dead, till, upon the letting in a little air into the receiver, it began forthwith to move afresh. And upon withdrawing the air again, it presently as before became motionless. Having repeated this trial two or three times, we took him out of the receiver, where he appeared not to have suffered any harm.

Exp. III.—But I thought it not unlikely, that there may be some such inequality in the strength or vivacity of animals, as to such kind of experiments as ours, that it might be worth while in several cases to repeat our trials. And on this occasion I shall here add, that having put an oyster into a phial full of water, before we included it in the receiver, that through the liquor the motion of the bubbles, expected from the fish, might be the more pleasantly seen and considered, this oyster proved so strong as to keep itself close shut, and repressed the eruption of the bubbles, which in the other forced open the shells from time to time; and kept in its own air as long as we had occasion to continue the trials.

Exp. IV.—Moreover a crawfish, that was thought more vigorous, being substituted in the place of the former crawfish, though once he seemed to lose his motion together with the air, yet afterwards continued moving in the receiver, in spite of our pumping: whether because there was some unperceived leaking that hindered a sufficient exhaustion of the air, or because this particular animal was more strong or vivid than the other, we could not positively determine.

The Seventh Title.

Of the Phænomena of a Scale Fish in an Exhausted Receiver.

The following experiment is far from being the first that was made on a scale fish in our vacuum; but in regard that the receivers wherein those trials were made, the external air could not be kept out so long and so well as in the vessel I am about to mention, I judged it well worth the pains to observe what would happen to a fish in an exhausted vessel, where it should be kept for some hours together from all supply of fresh air. And therefore I made several trials to that purpose, whereof, that which I think the most considerable, was registered as follows:

We took a receiver shaped almost like a bolt-head, containing by estimation near a pint, and the globulous part of it being almost half full of water, we put into it at the orifice (which was pretty large) a small gudgeon, about three inches long, which, when it was in the water, swam nimbly up and down therein. Then having drawn out the air so well that we guessed by a gauge that about nineteen parts of twenty or more might be exhausted, we took care that the regress of the air should not injure our experiment, about which we observed these particulars.

1st. The neck of the glass being very long, though there appeared great store of bubbles all about the fish, yet the rest of the water, notwithstanding the withdrawing of so much air as has been mentioned, emitted no froth, and but few bubbles.

2dly. The fish both at his mouth and gills for a great while discharged such a quantity of bubbles as appeared strange, and for about half an hour or more; whenever he rested a while, new bubbles would adhere to many parts of his body (as if they were generated there) especially his fins and tail: so that he would appear almost beset with bubbles, and if, being excited to swim, he was made to shake them off, he would quickly, upon a little rest, be beset with new ones as before.

3dly. Almost all the while he would gape and move his gills, as before he was included; though towards the end of the time that I watched, it often happened, that he neither took in, nor emitted any aërial particles that I could perceive.

4thly. After a while he lay almost constantly with his belly upwards, and yet would, in that posture, swim briskly as before.

5thly. Nay after a while he seemed to be more lively than at first putting in; whether by reason, that by discharge of so many bubbles, which by their distension perhaps put him to pain, he found himself relieved, or for some other cause, I examine not.

Having occasion to go abroad, I returned about an hour and a half after he had been sealed up, and found him almost free from bubbles, and with his belly upwards, and seeming somewhat tumid, but yet lively as before. But an hour and a quarter after that, when rising from dinner I went to look upon him again, he seemed to be motionless and somewhat stiff; yet upon shaking the glass, observing some faint signs of life in him, I opened the receiver under water, to try if that liquor and air would recover him; and the external water rushing in till it had filled the vacant part of the ball and the greatest part of the stem, the fish sunk to the bottom of it, with a greater appearance than ever of being alive; in which state, after he had continued some time, I contrived, by the help of the water he swam in, to get him through the pipe into a basin of water, where he gave more manifest signs of life: but yet for some hours lay on one side or other, without being able to swim or lie on his belly, which appeared very much shrunk in, as if something during the time of its being sealed up had been broken in his body, or his belly had been exceedingly distended, beyond restitution to its former tone.

All the while he continued in the basin of water, though he moved his gills as before he had been sealed up; yet I could not perceive that he did, even in his new water, emit as formerly any bubbles, though two or three times I held him by the tail in the air, and put him into the water again; where at length he grew able to lie constantly upon his belly (which yet retained much of its former lankness;) and though it be now above twenty-four hours since he was first included, he continues yet alive.

(Postscript. He lived in the basin eight or ten days longer; though divers gudgeons, since taken, died there in much fewer days.)

The Eighth Title.

Of two Animals with large Wounds in the Abdomen, included in the Pneumatical Receiver.

Exp. I.—Sept. 12. A small bird having the abdomen opened almost from flank to flank, without injuring the guts, was put into a small receiver, and, the pump being set to work, continued for some little time without giving any signs of distress, but at the end of about a minute and a half from the beginning of the exhaustion, she began to have convulsive motions in the wings; and though the convulsions were not universal, or apparently violent, as is usual in other birds from whom the air is withdrawn by the engine, yet at the end of two full minutes, letting in the air, and then taking off the receiver, we found the bird irrecoverable; notwithstanding which we did not find any remarkable alteration in the lungs, and found the heart (or at least the auricles of it) to be yet beating, and so it continued for a while after.

Exp. II.—We took also a pretty large frog; and having, without violating the lungs or the guts, made two such incisions in the abdomen, that the two curled bladders or lobes of the lungs came out almost totally at them, we suspended the frog by the legs in a small receiver, and after we had pumped out a good part of the air, the animal struggled very much, and seemed to be much disordered; and when the receiver was well exhausted, she lay still for a while, as if she had been dead, the abdomen and thigh very much swelled, as if some rarefied air or vapour forcibly distended them. But as, when the frog was put in, one of the lobes was almost full, and the other almost shrunk up, so they continued to appear, after the receiver had been exhausted; but upon letting in the air, not only the body ceased to be tumid, but the plump bladder appeared for a while shrunk up as the other, and the receiver being removed, the frog presently revived, and quickly began to fill the lobe again with air.

The Ninth Title.

Of the Motion of the separated Heart of a Cold Animal in the Exhausted Receiver.

Without discussing the opinions of learned men about the connection and dependency of the motion of the blood and beating of the heart, I thought it might give me a sufficient inducement to make the following experiment, that several sorts of animals would be presently killed in our vacuum by withdrawing the air, and even the insects mentioned in the formerly published digression about respiration, though they also were not totally deprived of life by the absence of the air, yet were of visible motion: Wherefore some good hint or other being to be hoped for from the discovering, whether or no a separated heart, which is but a part of an animal, would continue its motions in our vacuum; we made some trials to that purpose, whose success I find thus set down:

Exp. I.—The heart of an eel being taken out, and laid upon a plate of tin in a small receiver, we perceived it beat there as it had done in the open air; we then exhausted the vessel, and saw, that, though the heart grew very tumid, and here and there sent forth little bubbles, yet it continued to beat as manifestly as before, and seemed to do so more swiftly. The heart of another eel, being likewise taken out, continued to beat in the emptied receiver, as the former had done.

Exp. II.—The heart of another eel, after having been included in a receiver first exhausted, and then accurately secured from leaking, though it appeared very tumid, continued to beat there an hour; after which looking upon it, and finding its motion very languid, and almost ceased, by breathing a little upon that part of the glass where the heart was, it quickly regained motion, which I observed a while; and an hour after, finding it to seem almost quite gone, I was able to renew it by the application of a little more warmth. At the end of the third hour, coming to look at it once more, a bubble, that appeared to be placed between the auricle and the heart, seemed to have now and then a little trembling motion; but I found it so faint, that I could no more by warmth excite it so as plainly to perceive the heart to move: wherefore I suffered the outward air to rush in, but could not discern that thereby the heart regained any sensible motion, though assisted with the warmth of my breath and hands.

The Tenth Title.

A Comparison of the Times wherein Animals may be killed by Drowning, or withdrawing of the Air.

To help myself and others to judge the better of some difficulties concerning respiration, I thought it might be useful, that we compared together the times, wherein animals may be killed by that want of respiration which in those that are drowned is caused by the water that suffocates them, and that other want which proceeds from withdrawing the ambient air. Of the latter of these a sufficient number of instances is to be met with among our other experiments, and therefore I shall now subjoin more trials respecting the former, because this comparison has not, that I know of, been yet thought on by any.

Exp. I.—Sept. 10. A greenfinch, having his legs and wings tied to a weight, was gently let down into a glass body filled with water; the time of its total immersion being marked: at the end of half a minute after that time the strugglings of the bird seeming finished, he was nimbly drawn up again, but found quite dead.

Exp. II.—Whereupon a sparrow, that was very lusty and quarrelsome, was tied to the same weight, and let down after the same manner; but though he seemed to be, under water, more vigorous than the other bird, and continued struggling almost to the very end of half a minute from the time of his being totally immersed (during which stay under water there ascended, from time to time, pretty large bubbles from his mouth,) yet, notwithstanding that as soon as ever the half minute was completed he was drawn up, we found him, to our wonder, irrecoverably gone.

Exp. III.—A small mouse being held under water by the tail, emitted from time to time divers aërial bubbles out of his mouth, and at the last also at one of his eyes; being taken out at the end of half a minute and some few seconds, he yet retained some motions; but they proved convulsive ones, which at last ended in death.

By what is related under the First Title, it does not appear that water-fowl, at least that ducks, could in our receivers endure the want of air much longer than other birds: but now, to show that the contrivance of nature is not insignificant, as to the enabling them to continue much longer under water, without fresh air, than the land birds above-mentioned, it will not be amiss to subjoin the two following experiments.

Exp. IV.—We took the duck mentioned in the First Title, and tied a considerable weight of lead to her body, in such a manner as not to hinder her respiration, and yet to keep her down under water. With the above-mentioned clog, the duck was put into a tub full of clear water, under whose surface she continued about a minute by my watch, quietly enough, but afterwards began to appear for a while disturbed; which fit being over, our not perceiving any motion in her made us, at the end of the second minute, take her out of the water, to see in what condition she was, and finding her in a good one, after we had allowed her some time to recruit herself with fresh air, we let her down again into the tub, which in the mean time had been filled with fresh water, lest the other, which had been troubled with the steams and foulness of the duck's body, might either hasten her death by its being infected with them, or hinder our discerning what should happen, by its being opacated by them.

The bird being thus under water, began after a while, and from time to time continued to emit divers bubbles at her beak. There also came out at her nostrils divers real bubbles from time to time; and when the animal had continued about two minutes or more under water, she began to struggle very much, and to endeavour either to emerge or change her posture; the latter of which she had liberty to do, but not the former. After four minutes, the bubbles came much more sparingly from her: then also she began to gape from time to time, but without emitting bubbles; and so she continued gaping till near the end of the sixth minute, at which time all her motions, some of which were judged convulsive, and others that had been excited by our rousing her with a forceps, appeared to cease, and her head to hang carelessly down, as if she were quite dead. Notwithstanding which we thought fit, for greater security, to continue her under water a full minute longer, and then finding no signs of life, we took her out and being hung by the heels, and gently pressed in convenient places, she was made to void a pretty quantity of water, of which whether any had been received into the lungs themselves, we had not time and opportunity to examine. But all the means, that were used to recover the bird to life, proving ineffectual, we concluded she had been dead a full minute before we removed her out of the water: so that, to sum up the event of our experiment, even this water-bird was not able to live in cold water, without taking in fresh air, above six minutes; which is but $\frac{1}{10}$ of an hour.

Exp. V.—The duckling mentioned in Exp. 2, Title I. having a competent weight tied to her legs, was let down into a tub of water which reached not above an inch or two higher than her beak: during the most part of her continuance, there came out a quantity of bubbles at her nostrils, but there seemed to come out more and greater from a certain place in her head, almost equidistant from her eyes, but somewhat less remote from her neck than they. Whilst she was kept in this condition, she seemed frequently to endeavour to dive lower under water, and after much struggling and frequent gaping, she had divers convulsive motions, and then let her head fall down backward, with her throat upwards. To which motionless posture she was reduced at the end of the third minute, if not a little sooner; but a while after there appeared a manifest but tremulous motion in the two parts of her bill, which continued for some time, but afforded no circumstances whereby we could be sure that they were not convulsive motions; but these also ceasing upon the end of the fourth minute, the bird was taken out and found irrecoverable.

Exp. VI.—A viper that was kept so many hours in an exhausted receiver, till it was concluded to be stark dead, and to have been so for a good while, was nevertheless resolutely hindered by me from being thrown away, till I had tried what could be done by keeping it all night in a glass-body upon a warm digestive furnace. Whereupon this viper was found the next morning not only to be revived, but to be very lively, so as to invite me to make with her, without seeking for another, the following experiment.

We put her into a tall glass-body, fitted with a cork to the orifice of it, and depressed with a weight, so that she could come at no air. In this case we observed her from time to time; and after she had been ducked a while, she lay with very little motion for a considerable space of time. At an hour and a quarter she often put out her black tongue: at near four hours she appeared much alive, and as I remember, about that time also put out her tongue, swimming all this while, as far as we observed, above the bottom of the water. At the end of about seven hours and more, she seemed yet to have some life in her, her posture being manifestly changed in the glass, from what it was a while before; unless that might proceed from some difference made in her body as to gravity and levity. Not long after she appeared quite dead, her head and tail hanging down motionless, and directly towards the bottom of the vessel, whilst the middle of the body floated as much as the above-mentioned cork would permit it.

I must here observe, that though some of the above-mentioned animals seem, by the relations we have given of them, to have been a little sooner destroyed by drowning, than any we have mentioned were by our engine, that is no sure proof that suffocation kills animals faster than the deprivation of air they are exposed to in our engine. For in drowning, that which destroys is applied to its full vigour at the very first, and all at once; whereas, our receivers being made for several purposes, the deprivation of the air, that they make, cannot be made all at once, but the air must be pumped out by degrees; so that till the last the receiver will be but partly emptied. For confirmation of which, I have this to allege, that having in the presence of some virtuosi provided for the purpose a very small receiver, wherein a mouse could live some time, if the air were left in it, we were able to evacuate it at one suck, and thus, to the wonder of the beholders, to kill the animal in less than half a minute.

An Account of three Books. N° 62, p. 2032.

I. De Anglorum Gentis Origine Disceptatio, Auth. Rob. Sheringhamo Cantabrigiensi, Colleg. Gonvilii et Caii Socio. Cantabrigiæ, 1670, in 8vo.

The learned author of this discourse inquires into the origin both of the ancient Britons, and of the Angli or English; having first described the situation, latitude, form, fertility, and temper of the inhabitants of this island. In his inquiry he finds nothing that may be more certain, in so great obscurity, than that the old Britons were descended from the Trojans, by Brutus, the offspring of Æneas; and that the Angli are the race of the Gothic nation, which he makes the offspring not of Japhet, but of Shem: further, that the Getæ or Goths passed through Scythia into Scandia and Sarmatia, and from Scandia into the isles of the Baltic Sea and Germany; but that under that great Captain Filemer they made an excursion again into Asia, and having there ejected out of their seats the Magogæan Scythians and the Cimmerians, settled themselves and their empire in Asia; whence the Saxons, Getes and Angles, the forefathers of the English, were brought back again into Germany, to the ancient seat of the Getes, under the conduct of that famous Woden, the progenitor of the chief kings of Europe, about the year 2910.

II. A Vindication of Hydrologia Chymica, by William Sympson, M. D. London, 1670.

A vindication of a book replete with chemical errors.

III. A Discourse in Vindication of Descartes's System, by M. Des Fourneillis: to which is annexed the System General of the same Cartesian Philosophy, by Francis Bayle, M. D. at Toulouse.

Both these tracts were lately Englished out of French: of the latter of which, whilst it was yet untranslated, some account was given in Number 54; the former shows only, that the system of M. Descartes seems to have been taken out of the first chapter of Genesis; and particularly, that his opinion concerning brutes contains nothing dangerous.

Continuation of Mr. Boyle's Experiment. N° 63, p. 2026.

The Eleventh Title.

Of the Accidents that happened to Animals in Air brought to a considerable degree, but not near the utmost of Rarefaction.

In the generality of our pneumatical experiments upon animals, it suited with our purposes, to rarefy the air as much, and for the most part as fast as we could; but I had other trials in design, wherein an extraordinary degree of rarefaction, but yet not nearly the highest to which the air might be brought by our engine, seemed likeliest to conduce to my inquiries, and particularly seemed hopeful to afford some light in reference to those diseases and distempers that are thought primarily to affect the respiratory organs, or to depend upon something amiss in respiration.

Wherefore having gauges, by the help of which such experiments might be much better performed than else they could, I attempted several of them; some of whose successes I find in the following memorials.

Exp. I.—August 16. A linnet being put into a receiver, capable to hold about 4½ pints of water, the glass was well closed with cement and a cover; but none of the air was drawn out with the engine or otherwise. And though no new air was let in, nor any change made in the imprisoned air; yet the bird continued there three hours without any apparent approach to death: and though it seemed somewhat sick, yet being afterwards taken out, it recovered and lived several hours.

Exp. II.—Aug. 18. From the above mentioned receiver about half the air was drawn out, a linnet being then in the glass, and in that rarefied air, which appeared by a gauge to continue in that state, the bird lived an hour and near a quarter before it seemed in danger of death; after which the air being let in without taking off the receiver, she manifestly recovered, and leaped against the side of the glass; being taken out into the open air she flew out of my hand to a considerable distance.

Exp. III.—Sept. 9. We conveyed into a receiver, capable of holding about 4½ pints of water, a lark, together with the gauge, by the help whereof we pumped out of the receiver ¾ of the air that was in it before; then heedfully observing the bird, we perceived it pant very much, so that a learned physician (from whom I yet dissented,) judged those beatings to be convulsive: having continued thus for a little more than a minute and a half; the bird fell into a true convulsive motion, that cast it upon the back. And although we made great haste to let in the air, yet before the expiration of the second minute, and consequently in less than half a minute from the time immediately preceding the convulsion, the lark was gone past all recovery, though divers means were used to effect it.

Exp. IV.—Sept. 9. Presently after we put into the same receiver a greenfinch, and having withdrawn the air till it appeared by the gauge that there remained but half, we presently began to observe the bird, and took notice that, within a minute after, she appeared to be very sick, and shaking her head, threw against the inside of the glass a certain substance which I took to be vomit, and which afterwards appeared so; upon this evacuation the bird seemed to recover, and continue pretty well, but not without panting, till about the end of the fourth minute, at which growing very sick, she vomited again, shaking her head as at first, but much more unquestionably than before, and soon after eat up again a little of her vomit, at which time, whether that contributed to her recovery or not, she very much recovered. And though she had in all three fits of vomiting, yet for the last seven or eight minutes that we kept her in the receiver, she seemed to be much more lively than was expected, which may in part be attributed to a little air that by an accident got in, though it was immediately pumped out again. At the end of a full quarter of an hour from the first exhaustion of the receiver, the bird appearing not likely to die in a great while, and the engine being needed for other uses, we took out the bird, and thereby put a period to the experiment.

Exp. V.—April 12. I now thought fit to try whether, though a viper would not hold out very many hours in air brought to as high a rarefaction as we could bring it by our engine, yet to that cold and vivacious animal, a very small proportion of air, in comparison of what was necessary to hot animals, would not suffice to keep it alive for a considerable time.

A viper lately taken was included, together with a gauge, in a portable receiver, capable of holding about 3½ pints of water. This vessel being exhausted, and secured against the regress of the air, the imprisoned animal was observed from time to time nimbly to put out and to draw back its tongue about 36 hours after it was first shut up, for which reason we continued the vessel longer in the same shady place, where at the end of 60 hours looking upon her, as I was going to bed, she appeared very dull and faint, and not likely to live much longer; and the next morning being by some occasions carried abroad, and coming to look upon the glass presently after dinner, I found her stark dead, with her mouth opened to a strange wideness; wherefore suffering water to be impelled by the outward air into the cavity of the receiver, to observe how far that vessel was then emptied of air, we found by the water that was driven in, and afterwards poured out again and measured, that four parts of five, or rather five of six of the vesseled air, (if I may so call that which was shut up in the receiver) had been pumped out; so that in an air so rarefied as to expand itself to five or six times its former and usual dimensions, our viper was able to live 60 hours, that we are sure of, and perhaps might a pretty while longer.

A digressive Experiment concerning Respiration upon very high Mountains.

To illustrate what I have taken notice of in the printed experiments about the unfitness for respiration observed by the learned Acosta in the high mountains of Pariacacha, I shall here add what I have had the curiosity to learn from divers travellers, whom I purposely consulted about these matters; whereof you will easily believe that not many of them have had opportunity to give accounts. Meeting with an ecclesiastical person that had visited those high mountains of Armenia, (on one of which, because of their height, the tradition of the natives will needs have the ark to have rested) I asked him whether those mountains are really so high as is given out, and whether at the top of that he visited he found any difficulty of breathing. To the first part of which question he answered, that they were really exceeding high (which he might well judge of, having been upon some of the most famous both in Europe, Asia, and Africa) and that he could not come to the top, because of the unpassable snows; and to the second part he replied, that while he was in the upper part of the mountain, he plainly perceived that he was reduced to fetch his breath much oftener than he was wont, and than he did before he ascended the hill, and after he came down from it. And upon my inquiring, whether or no that difficulty of breathing might not be accidental or peculiar to him, he told me that he himself having expressed some wonder at finding himself so short-winded, the people told him that it was no more than happened to them when they were so high above the plain, it being a common observation among them. And I was the more inclined both to make inquiry about these matters, and to believe what he said, because what he related of their being covered with snow, and of an odd temperature of air, I had learned before from a traveller of another nation, and a stranger to this person.

The same churchman being asked by me, whether he had not in some part of Europe made the like observation (of the difficulty of breathing) told me that he had done it upon the top of a mountain in the country of Souenes, in or near the province of Languedoc; which may serve to confirm what I am about to relate from the mouth of a learned traveller that was upon the top of one of the Pyrenees not very remote from the mountains we speak of.

This gentleman, who was a person curious and intelligent, being brother in law to one of the chief lords of those parts, was by him invited, about the beginning of September, to visit a neighbouring mountain, one of the highest of the Pyrenees, which is commonly called Pic de Midi, upon whose top they stayed many hours. I inquired of him whether they found the air at the top as fit for respiration as common air, which he told me they did not, but were forced to breathe shorter and oftener than usual; and because I suspected that might be caused by their motion, I asked whether they observed it to cease when they came down to the bottom of the hill, which he told me they plainly did, besides that they stayed many hours at the top, too long to continue out of breath.

But that I may not here conceal any thing, that may conduce to the discovery of the truth in the matter under consideration, I shall add, that I sometimes thought it worth further inquiry, whether the sickness, if not also the difficulty of breathing, that some have been obnoxious to in the uppermost parts of Pariacacha, and perhaps some other high mountains, may be imputed not so precisely to the thinness and rarity of the air, in places so remote from the lowermost part of the atmosphere, as to exclude certain steams of a peculiar nature, which in some places the air may be imbued with? In favour of which suspicion I remember, that inquiring once of an intelligent man, who had lived several years in the island of Teneriffe, whether he had been at the top of the Peak of that name, and what he had there taken notice of about the air? he answered me, that he had attempted to go up to the top of the mountain, but that though some of the company were able to do so, he and some others before they had reached near so high, grew so sick upon the operation they felt of the sharp air, and sulphureous exhalations which infected it, that they were forced to stay behind their companions, he having already found this effect of those piercing steams upon his face (which when he made me this relation was of a fair complexion) that the skin began to be of a pale yellow, and even his hair to be discoloured.

The Twelfth Title.

Of the Observations produced in an Animal in Changes as to Rarity and Density made in the self-same Air.

In the experiments hitherto recited, the animals that were recovered from a gasping condition, have been so by letting in fresh air upon them, and not the same that had been withdrawn from them. Wherefore I thought it very requisite to try, whether the same portion of air, without being renewed, would, by being expanded much beyond its usual degree, and reduced to it, serve to bring an animal to death's door, and revive him again; since by the success of such a trial, it would notably appear, that the bare change of the consistence of the air, as to rarity and density, may suffice to produce the above-mentioned effects.

But to devise a way to put this experiment in practice appeared no easy matter; since it required a receiver that should be transparent, and be capable of changing its bulk without suffering any air to get in or out.

To surmount these difficulties, the first thing I thought on was, to take a fine clear bladder of a sheep or hog, made more transparent by being anointed with oil, which was done on the outside, that the smell of it might less offend the animal to be included. Then we clipped off as much of the bladder at the neck, as was judged absolutely necessary to make an orifice capable of letting in a mouse; that sort of animals being, by reason of their smallness, the fittest of those furnished with lungs and hot blood we could procure. And whereas it seemed very difficult, when the neck of the bladder was cut off, to make up so large an orifice without wrinkles, at which the rarefied air may escape; to obviate this inconvenience, we provided a round stick somewhat less than the orifice; that the wood being covered with a close and yielding cement, for pitch or the like common stuff will not always serve the turn, we might be able to tie the bladder fast and close enough upon the thus fitted stopple.

And now to reduce these things to practice, and by their help make our designed experiment, we included a mouse into a receiver made according to this way, leaving in the bladder as much air as we thought might suffice him for as long a time as the experiment was to last. Then putting this limber or extensible receiver, if I may so call it, into an ordinary one of glass, and placing this engine near a window, that we may see through both of them; the air was by degrees pumped out of the external receiver, (as for distinction sake I shall call it,) and thereupon the air included in the bladder did proportionably expand itself and so distend the internal receiver, till being arrived at a degree of rarefaction, which rendered it unfit for the included mouse's respiration, I perceived, though with some difficulty, in this animal the signs of his being in great danger of sudden death. Whereupon the outward air, being hastily let into the external receiver, compressed the swelled bladder to its former dimensions, and thereby the included air to its former density, by which means the fainting mouse was quickly revived. Having given him some convenient time of respite, the experiment was repeated with the like success, and we doubted not but the third trial we made would have ended as the two former did; but that, whilst we were considering of the sickness of the mouse, which, by reason of some opacity that could scarcely be avoided in the wrinkled bladder, was not as to its degree so easily taken notice of, it became irrecoverable by the subsequent condensation of the air.

N. B. The confirmation of this by further experiments will properly fall under another title.

The Thirteenth Title.

Of an unsuccessful Attempt to prevent the Necessity of Respiration by the Production or Growth of Animals in our Vacuum.

Having had frequent occasions to observe how quickly those animals, whose blood is actually warm, did expire in our vacuum; and that even those animals with lungs, whose blood was actually cold, were not able to live any considerable time there; I thought it worth while, though extremely difficult to try, whether there might not be some ways yet unpractised, either to make such animals as nature endows with lungs, live without respiration, or at least to bring such insects, and other animals, as can already live without air, to move also without it in our vacuum.

Therefore considering with myself what happens to infants and other young animals in the womb, and even after they come from thence, if they continue to be wrapt up in the secundines; though as soon as they are brought into the free air they may be presently killed by being kept from breathing: considering also what I elsewhere relate of the slow expiration of a very young kitling in our vacuum; together with the long want of respiration, which custom enables some divers to endure: considering these things, I say, though I know that somewhat may be objected to show that these instances are not altogether full to my purpose; yet they, among other things, invited me to think that the least unlikely projects that occurred to my barren invention, would be these that follow.

First, I thought fit to try, whether the seeds of respiring animals might be either hatched or otherwise brought to produce young ones in our vacuum. For, if that could be compassed, I should obtain my end.

Next, in case of my failing in the former attempt, and that which is to be after a few lines proposed, I thought fit to try, whether at least I could not bring the eggs of insects to hatch or be animated; or aurelias (as they call them) that were already alive, turn according to the course of nature, into winged insects, as flies or butter-fishes; (of which trials and those of the former sort, the account properly belongs to another place, where I relate the success of these and other attempts to produce plants and animals in our vacuum.)

But thirdly, considering that nature has so ordered it, that frogs, though when they are grown large enough to deserve that name, they be amphibious animals endowed with lungs; yet before they attain to it, live wholly in the water like fishes; I thought it the most expeditious and least improbable attempt we could make, to try, whether or no this animal, being as a fish brought to live either in our vacuum, or at least in highly rarefied air, would not continue to do so, after its lungs should be perfectly formed. Wherefore, though I foresaw and foretold the difficulty that would be met with in the prosecution of this experiment, namely, that the aërial bubbles that would be disclosed in such soft bodies upon the withdrawing of the pressure of the ambient, would so violate the slight texture of those tender animals, as to hinder them from living long or moving freely; yet I thought it very fit to attempt the trial.

Exp. I.—We took a number of tadpoles, and put them with a convenient quantity of water into a portable receiver of a round form, and observed, that at the first exsuction of the air they rose to the top of the water, though most of them subsided again, till the next exsuction raised them. The receiver being exhausted, they continued restless, moving all of them in the top of the water, and though some of them seemed to endeavour to go to the bottom, and dived some part of the way, especially with their heads, yet they were immediately buoyed up again. Within an hour or a little more they were all motionless, and lay floating on the water; wherefore I opened the receiver, upon which the air rushed in, and almost all of them presently sunk to the bottom, but none of them recovered.

Exp. II.—We afterwards included a lesser number of tadpoles in a smaller glass, which was also exhausted with the like circumstances with the former. And when I found the other tadpoles dead, I hasted to these, which did not, except perhaps one, give any sign of life; but upon letting in the air, these having not been long kept from it, some few of them recovered, and swam up and down lively enough for some time; though after a while they also died.

Exp. III.—Some years after I repeated the same experiment in a portable receiver of a convenient kind; and though after the exhaustion was perfected, the tadpoles did for a while move briskly enough on the top of the water (none of them appearing able to dive or swim under water) yet coming to look on them at the end of an hour, they seemed to be all of them quite dead, yet continued floating. And though within half an hour after that, I let in the air upon them, yet all the effect of it was, that the most of them immediately sunk to the bottom, as the rest did a very little while after; none of them, that I could observe, recovering any vital motion.

Exp. IV.—We procured with much difficulty some of those odd insects which I elsewhere describe, whereof gnats have by some ingenious men been observed to be generated about the end of August, or beginning of September. These for some weeks live all together in the water (as tadpoles do) swimming up and down therein till they are ripe for a transmigration into flies: which itself is so great a rarity in nature, as makes these little creatures recompense to our curiosity the trouble they often give our faces and hands. Supposing then that, if I could get some of these, and include them, being of those insects they call aquatilia, and so minute as they are, they may live a great while in the receiver without air, and in the mean while attain the period, which, according to nature's course, is wont to turn them into flies, which might come forth winged creatures into a medium not furnished with common air, as others of their kind enjoy; supposing, I say, that these insects would afford me some information about these particulars, having upon much watching met with four or five of them after a shower of rain, that dropped from a house into a vessel laid on purpose for it, we included them with some of their water into a small glass receiver, which being very exactly closed, we kept in a south window, where these little creatures continued to swim up and down for some few days without seeming to be much incommoded by so unusual a habitation; and at the end of that time, and much about the same day, they divested the habit they had whilst they lived as fishes, and appeared with their exuviæ or cast coats under their feet, showing themselves to be perfect gnats, that stood without sinking upon the surface of the water, and discovered themselves to be alive by their motion, when they were excited to it: but I could not perceive them fly in that thin medium; to which inability, whether the viscosity of the water might contribute I know not; though they lived a pretty while, till hunger or cold destroyed them. Something in this experiment may deserve serious reflections; which I cannot spare time to offer at.

A digressive Experiment concerning the Expansion of Blood and other Animal Juices.

For some purposes, relating partly to respiration and partly to other inquiries, I thought fit to endeavour to obtain what information could be procured of the consistence and disposition of blood and other animal liquors to expand; in pursuance of which the ensuing trials, among others, were undertaken.

The warm blood of a lamb or a sheep being taken, as it was hastily brought from the butcher's, where the fibres had been broken to hinder the coagulation, was in a wide mouthed glass put into a receiver, made ready for it, and the pump being early set to work, the air was diligently drawn out; but the operation was not always, especially at first, so manifest as the spirituousness of the liquor made some expect; yet this hindered not but that after a long expectation, the more subtle parts of the blood would begin to force their way through the more clammy ones, and seem to boil in large clusters, some as large as great beans or nutmegs, and sometimes, to the wonder of the by-standing physicians, the blood was so volatile, and the expansion so vehement, that it boiled over the containing glass; of which, when it was put in, it did not by our estimate fill above a quarter. Having also included some milk, warm from the cow, in a cylindrical vessel of about four or five inches high, though the operator was induced to pump a great while before any intumescence appeared in the milk, yet afterwards when the external air was fully withdrawn, the white liquor began to boil in a way that was not so easy to describe as pleasant to behold; and this it did for a considerable time with so much impetuosity, that it threw up several parts of itself out of the wide mouthed glass that contained it (and could have contained as much more) though there were not above two or three ounces of the liquor.

A yet greater disposition to intumescence we thought we observed in the gall, which was but suitable to the viscosity of the texture.

Note, that the two foregoing experiments were made as an inquiry whether, and how far, the destructive operation of our engine upon the included animal, might be imputed to this, that upon the withdrawing of the air, besides the removal of what the air's presence contributes to life, the little bubbles generated upon the absence of the air in the blood, juices, and soft parts of the body, may by their vast number, and their conspiring distension, variously straiten in some places and stretch in others, the vessels, especially the smaller ones, that convey the blood and nourishment, and so by choaking up some passages, and vitiating the figure of others, disturb or hinder the due circulation of the blood? not to mention the pains that such distensions may cause in some nerves, and membranous parts, which by irritating some of them into convulsions, may hasten the death of animals, and destroy them sooner by occasion of that irritation, than they would be destroyed by the bare absence or loss of what the air is necessary to supply them with. And to show how this production of bubbles reaches even to very minute parts of the body, I shall add on this occasion, what I once observed in a viper furiously tortured in our exhausted receiver, namely, that it had manifestly a conspicuous bubble moving to and fro in the waterish humour of one of its eyes.

Another digressive Experiment belonging to the same Title.

To show, that not only the blood and liquors, but also the other soft parts, even in cold animals, have aërial particles latent in them; we took the liver and heart of an eel, as also the head and body of another fish of the same kind, cut asunder crossways somewhat beneath the heart, and putting them into a receiver, upon the withdrawing of the air we perceived that the liver did manifestly swell every way, and that both the upper and lower parts did so likewise, and at the place where the division had been made, there came out in each portion of the fish divers bubbles, several of which seemed to come from the medulla spinalis, or the cavity of the back bone, or the adjoining parts; and the external air being let in, both the portions of the eel presently shrunk, some of the skin seeming to be grown empty or flaccid in each of them.

The Fourteenth Title.

Of the Power of Assuefaction to enable Animals to hold out in Air, by Rarefaction made unfit for Respiration.

The power of assuefaction in other cases, made me think it very well worth trying what it would do in respiration; and the rather, because I presumed it might prove an experiment of good use, if we should discover, that by a gradual accustomance an animal may be brought to live either in a much thinner air, or much longer in the same air, than at first he could. But in regard that to make such a trial perspicuously enough, the opacity of the bladder made use of in the former title, was like to be an impediment, I devised another way to obviate this inconvenience, which may, I hope, be competently understood by the heedful perusal of the following trials.

Exp. I.—We included in a round phial with a wide neck, (the whole glass being capable of containing about eight ounces of water) a young and small mouse, and then tied strongly upon the upper part of the glass's neck a fine thin bladder, out of which the air had been carefully expressed, and then conveyed this fantastical vessel into a middle sized receiver, in which we also placed a mercurial gauge. This done, the air was by degrees pumped out, till it appeared by the gauge that there remained but a fourth part in the external receiver, whereupon the air in the internal receiver expanding itself, appeared to have blown the bladder almost half full, and the mouse seeming very ill at ease by his leaping, and otherwise endeavouring to pass out at the neck of his uneasy prison, we, for fear the over thin air would dispatch him, let the air flow into the external receiver, whereby the bladder being compressed, and the air in the phial reduced to its former density, the little animal quickly recovered.

Exp. II.—A while after, without removing the bladder, the experiment was repeated, and the air by the help of the gauge was reduced to its former degree of rarefaction, and the mouse, after some fruitless endeavours to get out of the glass, was kept in that thin air for full four minutes, at the end of which he appeared so sick, that to prevent his dying immediately, we removed the external, and took out the internal receiver. Whereupon, though he recovered, yet it was not without much difficulty, being unable to stand any longer upon his feet, and for a great while after continued trembling.

Exp. III.—But having suffered him to rest a reasonable space of time, presuming that assuefaction had accustomed him to greater hardships, we conveyed him again into the external receiver, and having brought the air to the former degree of expansion, we were able to keep him there for a full quarter of an hour, though the external receiver did not considerably leak, as appeared both by the mercurial gauge, and by the continuing distension of the bladder. And it is worth noting, that till near the latter end of the quarter of an hour, not only the animal scarcely appeared distressed, remaining still very quiet, but which is more, whereas when he was put in the tremblings formerly mentioned were yet upon him, and continued so for some time; yet afterwards, in spite of the expansion of the air he was then in, they left him early enough. And when the internal receiver was taken out, he did not only recover from his fainting fit sooner than before, but escaped those subsequent tremblings we have mentioned.

Exp. IV.—Encouraged by this success, after we had allowed him some time to recollect his strength, we reconveyed him and the odd vessel wherein he was included into the former receiver, and pumped out the air till the mercury in the gauge was not only drawn down as low as formerly, but nearly half an inch lower, that there the air might be yet further expanded, than hitherto it had been. And though this did at first seem to discompose our little beast, yet after a while he grew very quiet, and continued so for a full quarter of an hour, when being desirous to try what operation a further rarefaction of the air would have upon him, we caused three exsuctions more to be made by the pump, before we discovered him to be in manifest danger, (at which time the bladder appeared much fuller than before) but then we were obliged to let the air into the outward receiver, whereupon the mouse was more speedily revived than one would have suspected.

And these trials of the power of assuefaction seemed the more considerable, because the air in which the mouse had all this while lived, had been clogged and infected with the excrementitious effluviums of his body, for it was the same all along, we having purposely forborn to take off the bladder, whose regular intumescencies and shrinkings sufficiently manifested that the vessel, whereof it was a part, did not leak.

Postscript. Though the success of the recited experiments is very promising, yet a subsequent trial or two, whose particularities are slipt out of my memory, oblige me in point of candour to declare, that, for further satisfaction, the trials of the power of accustomance in reference to air unfit for respiration, ought to be both reiterated, and to be made in different sorts of animals.

The Fifteenth Title.

Some Experiments, showing that Air, become unfit for Respiration, may retain its wonted Pressure.

Exp. I.—We took a mouse of an ordinary size, and having (not without some difficulty) conveyed him into an oval glass, fitted with a somewhat long and considerably broad neck, which we had provided, that it might be wide enough to admit a mouse in spight of his struggling; we conveyed in after him a mercurial gauge, in which we had diligently observed and marked the station of the mercury, and which was so fastened to a wire reaching to the bottom of the oval glass, that the gauge remaining in the neck was not in danger of being broken by the motions of the mouse in the oval part: The upper part of the long neck of the glass was, notwithstanding the wideness of it, hermetically sealed by the help of a lamp and a pair of bellows, that we might be sure that the imprisoned animal should breathe no other air than that which filled the receiver at the time when it was nipped up. This done, the mouse was watched from time to time, and though, by reason of the largeness of the vessel in comparison of so small an animal, he seemed to me rather drooping than very near death at the end of the second hour; yet coming to look upon him about half an hour after, he was judged by the spectators quite dead, notwithstanding our shaking of the vessel to rouse him up. This made me cast my eyes upon the gauge, wherein I could not perceive any sensible change of the mercury's station. But being unwilling to give over the mouse without trying what fresh air would do to recover him, I caused the sealed part of the glass to be broken off, and, notwithstanding that his continuing to appear dead increased the confidence of those that thought him so, I obtained after a while some faint tokens of life; though I am not sure that they would have continued in a vessel where the air was so clogged and infected, if it had not been that fresh air was frequently blown in by a pair of bellows, whose nose was inserted into the neck of the glass. This fresh air seemed evidently, though but slowly, to revive the gasping animal, whom I would not, nor could not conveniently take out of the glass, till he had gained strength enough to make use of his legs; after which, without breaking of the glass, (which I was loath to lose, having then no other of the kind) we took him out, and found him quickly able to go up and down. After which service, and another trial we had made with him, which belongs not to this place, we set him at liberty to shift for himself.

Exp. II.—Such an experiment as the former we made with like success upon a small bird, included with a gauge in a receiver holding about a quart of water. The bird in about half an hour appeared to be sick and drooping, and the faintness and difficulty of breathing increased for about two hours and a half after that, at which time the animal died, the gauge being not sensibly altered, unless perhaps the mercury appeared to be impelled up a little higher than it was when put in; which yet might proceed from some accidental cause.

Exp. III.—To satisfy some curious persons, that it is not want of coldness, but something else in the included air, that makes it destroy the birds that are pent up in it, and by the hot exhalations that steam from their bodies may be supposed to overwarm it, we made the following experiment.

In a glass phial, capacious enough to hold about three quarts of water, we not only included, but for greater accuracy hermetically sealed up, a small bird, and found, that in a few minutes he began to be sick and pant; which symptoms I suffered to continue and increase against the mind of a learned by-stander, (who thought the animal would not hold out so long) till they had lasted just half an hour: at which time having provided a vessel of water with sal ammoniac newly put into it, to refrigerate it, (according to the way I elsewhere published,) and the liquor thus made exceedingly cold, somewhat to the wonder of those that felt it; the phial with the sick bird was immersed in it, and kept there in that condition for six minutes; and yet it did not appear, in the judgment of the by-standers, that the great refrigeration, that must be this way procured to the imprisoned air, did sensibly revive or refresh the drooping animal, who manifestly continued to pant exceedingly as before, and, as some affirmed, more; so that this remedy proving ineffectual, the phial was removed out of the water, and the bird some time after did, as I foretold, make many strains to vomit (though she brought up little) followed by evacuations downward, before she quite expired, which she did within a minute or two of a just hour, after the beginning of her imprisonment.

If I had been able (which I was not) to procure more birds, I would willingly have prosecuted this experiment by several other not unhopeful trials; which for want of subjects I was fain to leave only designed.

The Sixteenth Title.

Of the Use of the Air to elevate the Steams of Bodies.

In the digression about respiration annexed to the 41st of our physico-mechanical experiments formerly published, it is proposed as one of the considerable uses of the air in respiration, that, being drawn into the lungs, it serves to carry off with it, when breathed out again, the recrementitious steams that are separated from the mass of blood in its passage through the lungs: from which fuliginous excrements, if the blood were not continually freed by the help of the air, after nature had been accustomed to that way of discharging them, their stay in the body might have very great and destructive operations on it.

For the illustration of this use of the air, I shall now subjoin the following experiment.

We made, by distillation, a blood-red liquor, which chiefly consisted of such saline and spirituous particles, as may be obtained from the mass of blood in human bodies; this liquor is of such a nature, that if a glass phial about half filled with it, be kept well stopped, the red liquor will rest as quietly as any ordinary one, without sending up any smoke or visible exhalation; but if the phial be unstopped, so that the external air be permitted to come in, and touch the surface of the liquor, within a quarter of a minute or less, there will, upon this contact, be elevated a copious white smoke, which will not only fill the upper part of the glass, but plentifully pass out into the open air, till the phial be again stopped.

My purpose in this tract, to forbear sidings in controversies, keeps me from taking notice of the speculations suggested by some of the phænomena of this liquor; which yet I thought I might lawfully mention, as far as I have done it, because it but adventures upon giving one of the uses rather of the air, than immediately of respiration itself; and is brought but to illustrate what I have not found denied by any, though considered by very few; namely, the office of the air to carry off in expiration the fuliginous steams of the lungs. For in our experiment we manifestly see, that the very contact of the air may give the corpuscles of moist bodies a peculiar volatility, or facility to emerge in the form of steams. I know there are some corrosive spirits, as in nitre and salt, simple, or compounded of them, that, when they are very strong, emit for a while manifest fumes; but the difference of those liquors, and their inferiority to our red spirit, in the capacity of smoking liquors, might easily enough be manifested, if it were judged proper in this place, where it may suffice to take notice of these two things: The one is, that when the phial has lain stopped and quiet a competent time, the upper half of it will appear destitute of fumes, of which the air, it seems, will imbibe and constantly retain but a certain moderate quantity; which may give some light towards the reason, why the same air which will be quite clogged with steams, will not long serve for respiration, which requires frequent supplies of fresh air: The other is, that if the unstopped phial were placed in our vacuum, it would not emit any visible steams at all, nor so much as to appear in the upper part of the glass itself that held the liquor; whereas, when the air was by degrees restored at the stop-cock, without moving the receiver itself, to avoid injuring its closeness, the returning air would presently raise the fumes, first into the vacant part of the phial, whence they would ascend into the capacity of the receiver; and likewise, when the air that was requisite to support them, was pumped out, they also accompanied it, as their unpleasant smell evinced, and the red spirit, though it remained unstopped, emitted no more fumes till the new air was let in.

One may compare with this liquor another smoking one, mentioned in the 29th of the first published pneumatical experiments, where an experiment is related of it, that has something in common with this, and may so far serve to confirm what is now delivered, as this also has some things additional to that: besides that that liquor being made with ingredients corrosive, and of a bad name among chemists themselves, the fumes that proceed from it may fright many from daring to meddle with it: whereas this our red spirit has been found potently medicinal for some distempers of the lungs by a doctor of physic, whom I desired to try it. The other phænomena of this liquor I shall not stay to describe, as not belonging to this place, and the liquor itself with very little variation I have in the history of colours communicated.

The Seventeenth Title.

Of the long Continuance of a Slow-worm and a Leech alive in the Vacuum made by our Engine.

In the often cited digression about respiration, there is mention made of the great vivaciousness of house-snails as they call them, and how little operation the withdrawing of the air had upon them, in comparison of what it is wont to have on other animals. I shall now add, by way of confirmation, that I made trial upon ordinary white snails without shells, whereof two of different sizes (the largest about an inch and a half, and the other about an inch in length) were included in a small portable receiver, which being carefully exhausted, and secured against the return of the air, was attentively considered by me, presently after it was removed from the engine; whereby it was easy to discern, that both the snails thrust out and retracted their horns (as they are commonly called) at pleasure, though their bodies had in the softer places a quantity of newly generated bubbles sticking to them: but though they did not lose their motion near so soon, as other animals were, in our vacuum, wont to do; yet coming to look on them after some hours, they appeared motionless and very tumid, and at the end of 12 hours the inward parts of their bodies seemed to be almost vanished, and they seemed to be but a couple of small full-blown bladders; and on the letting in of the air they immediately so shrunk, as if the bladders having been pricked, the receding air had left behind it nothing but skins; nor did either of the snails afterwards, though kept many hours, give any signs of life.

Upon a supposition that the cold, and clammy constitution of snails might be a main cause of their being able to endure the absence of the air so well, I thought it worth trial, whether efts and leeches might not yet be more able to continue in our vacuum than a snail; and accordingly some experiments were made pursuant to that curiosity; the most fully registered whereof are these that follow.

Exp. I.—We included in a receiver, whose globular part was about the size of a large orange, one of that sort of animals vulgarly called efts; having withdrawn, but not solicitously, the air, and secured the vessel against the unpermitted return of it, we kept him there about 48 hours, during all which time he continued alive, but appeared somewhat swelled in his belly; his under-chop moving the very first night, but not the day and night following. By opening the receiver at length under water, we perceived that about half the air had been drawn out. As soon as the water was impelled into the glass, the animal that was before dull and torpid, seemed by very nimble and extravagant motions to be strangely revived.

Exp. II.—We took a leech of the usual size, or somewhat short of it, and having included it, together with some water, in a portable receiver, guessed capable of holding about 10 or 12 ounces of that liquor; the air was pumped out after the usual manner, and the receiver being removed to a light place, we observed, as we expected, that the leech keeping herself under water, there emerged from divers parts of her body store of bubbles, some of them in a dispersed way, but others in rows or files, if I may so speak, that seemed to come from determinate points. Though this production of bubbles lasted a pretty while, yet the leech did not seem to be very much discomposed by her present condition. This done, we disposed of the receiver, which was well secured from the ingress of the outward air, into a quiet place, where we daily visited it once at least, or oftener, as there was occasion; and found the leech somewhat fastened by her tail to that part of the glass that was under water, and sometimes wandering about that part which was quite above water; and still, when we endeavoured to excite her, she quickly manifested herself to be alive: and indeed (which will be thought strange) appeared so lively after the full expiration of five natural days, that expecting something might have happened to the receiver, and thereupon resolving to try how staunch it had continued, I opened it under water, by which means the outward air impelled in so much of that liquor, that I was satisfied the receiver was immediately before as well exhausted as others are wont to be in our pneumatical experiments.

The Eighteenth Title.

Of what happened to some Creeping Insects in our Vacuum.

Exp. I.—We took five or six caterpillars of the same sort; but I could not tell to what ultimate species the writers about insects referred them. These being put into a separate receiver of a moderate size, had the air drawn away from them, and carefully kept from returning. But notwithstanding this deprivation of air, I found them, about an hour after, moving to and fro in the receiver; and even above two hours after that, I could, by shaking the vessel, excite in them some motions, that I did not suspect to be convulsive. But looking upon them again some time before I was to go to bed, about 10 hours after they were first included, they seemed to be quite dead, and though the air was forthwith restored to them, they continued to appear so, till I went to bed; yet I thought fit to try, whether time might not at length recover them, and leaving them all night in the receiver, I found the next day, that three if not four of them were perfectly alive.

Exp. II.—We took from a hedge a branch, that had a large cobweb of caterpillars in it, and having divided it into two parts, we put them into like receivers, and in one of them shut up the caterpillars together with the air, which from the other was exhausted. The event was, that in that which had the air, the small and scarcely visible insects, after a short time, appeared to move up and down as before, and so continued to do for a day or two; after which, other occasions made the experiment to be neglected; whereas that glass whence the air had been drawn out and continued kept out, showed after a very little while no motion that we could perceive. But to try whether caterpillars may continue so far alive in our vacuum all the winter, as, the next spring or summer, to proceed in the transmigration to a butterfly, is a trial that we have but begun, and therefore must not pretend to say any thing about its event.

The Nineteenth Title.

Of the Phænomena suggested by Winged Insects in our Vacuum.

When our physico-mechanical experiments were dispatched to the press, the inconvenient season of the year, and the difficulty of making the receivers I then employed to keep out the air for any long time, hindered me from then publishing above a trial or two of what would happen to winged insects in our vacuum. But afterwards being provided with more commodious vessels, I thought fit at several times to repair that omission by various attempts, the chief of which are as follow.

Exp. I.—Nov. 12, about eight o'clock at night. There were taken four middle sized flesh flies, which having their heads cut off were inclosed in a portable receiver, furnished with a pretty large pipe and a bubble at the end. As soon as the receiver was exhausted, those flies lost their motion (which was not brisk before;) an hour or two after, I placed them near the fire, which restored not their motion to them; wherefore I let in the air upon them, after which in a very short time they began one after another to move their legs, and one or two of them to walk; and having kept them all night in a warm place, when I sent a person the next morning to try, if they would manifest any motion, he told me, that for a while they did, though when I afterward rose myself, I could not perceive any motion in them.

Exp. II.—Sept. 11. About noon we closed up divers ordinary flies, and a bee or wasp; all which, when the air was fully withdrawn, lay as dead, save that for a very few minutes some of them had convulsive motions in their legs. They continued in this state 48 hours, after which the air was let in upon them, and that not producing any signs of life in them, they were laid in the meridian sun, but not any of them seemed in any degree to recover.

Exp. III.—Dec. 11. We put a great flesh fly into a very small portable receiver, where at first it appeared to be very brisk and lively, but as soon as the air was drawn out, fell on her back, and seemed to have convulsive motions in her feet and proboscis; from whence she presently recovered upon the letting in of the air; which being drawn out again, she lay as dead; but a while after, (within a quarter or half an hour) I perceived, that upon shaking the receiver, she stirred up and down, (but faintly.) This was done pretty late yesternight, since whence I had not occasion to look on the glass, till this night after supper, when I found the fly not recoverable either by warmth, or letting in the air. A while after this note was written, this fly recovered; and being next morning sealed up again in that glass, and kept 48 hours, though over the chimney, she died.

Exp. IV.—We took a large grashopper, whose body, besides the horns and limbs, was about an inch in length, and thick in proportion: this we conveyed into a portable receiver of an oval form, and capable of holding about a pint of water or more, and having afterwards pumped out the air, till by the gauge it appeared to have been pretty well drawn out, we took care no air should re-enter to disturb the experiment. The success whereof was this: First, Though before the exhaustion of the air was begun, the grashopper was stirring, and lively, and continued so for a while after the beginning of the operation; yet when the air began to be considerably rarefied, he appeared to be very ill at ease, and seemed to sweat out of the abdomen many little drops of liquor, which being united trickled down the glass like a little stream, which made at the bottom a small pool of clear liquor, amounting to near a quarter of a spoonful, and by that time the receiver was ready to be taken off, the grashopper was fallen upon his back and lay as dead. Secondly, Though having a little after laid the glass in a south window, on which the sun then shone, I perceived some slow motions in the thorax, as if he strained to fetch breath; yet I was not sure they were not convulsive motions, and whatever they were, they lasted but a while, and then the animal appeared to be quite dead, and to continue so for three hours from the removal of the receiver. Thirdly, That time being expired, the glass was opened and the air let in upon him, notwithstanding which there appeared no sign at all of life; but imagining there might be some time requisite to recover him out of so deep a swoon, I let the glass rest in a convenient posture, that the water that came from him might not endanger him, for a quarter or half an hour, and though I then perceived no signs of life, yet being desirous to pursue the trial yet further, I caused him to be carried into a sunshiny place, where the beams of a declining sun presently began to make him stir his limbs, and in a short time brought him perfectly to life again.

Exp. V.—April 15. We took one of those shining beetles, called rose-flies, and having inclosed it in a very small round receiver, which we exhausted, and though he that attended the engine, affirmed, it struggled much whilst the air was withdrawing, yet presently after, I could perceive but little motion (and part of that seemed almost convulsive) and afterward going abroad, and not returning to look on the glass till about fix hours after, the fly seemed quite dead, and discovered not any motion upon that of the glass. And within about an hour after, though I let the air rush in; yet no sign of life ensued, neither immediately, nor for a pretty while after. So that suspecting the fly to be really dead, and yet not resolutely concluding it, though I would then wait no longer, yet three or four hours after (viz. about 10 o'clock at night) I returned to the receiver, and found the beetle lively enough. Whereupon I caused the glass to be again exhausted, and secured from the ingress of the air, during which time the animal seemed to be much disquieted by what was done to it, but did not lose its motion before I went to bed, which was soon after.

Exp. VI.—With butterflies I made several trials, the accounts of most of which are lost; but thus much I very well remember, that having observed them not only to live, but to move longer than was expected, I inclosed divers of them in receivers somewhat large, especially that I might see, whether in so thin a medium some or other of them, by the help of their large wings, would be able to fly. But though, whilst the air continued in the glasses, they flew actively as well as freely up and down; and though after the exhaustion of the air, they continued to live and were not motionless; nay though at the bottom of the receiver they would even move their wings and flutter a little, yet I could not perceive any of them fly, by which I mean, perform any progressive motion supported by the medium only. And by frequently inverting the receiver (which I took care should be pretty long to let them fall from one extreme to the other,) they would fall like dead animals without displaying their wings, though just as they came to touch the bottom, some of them would sometimes appear to make some use of them, but not enough to sustain themselves, or to keep their falls from being violent.

The Twentieth Title.

Of the Necessity of Air to the Motion of such small Creatures as Ants, and even Mites themselves.

In the experiments hitherto mentioned, the animals on which the trials have been made, were several of them of a moderate bulk, and others, though small, yet not the least that nature afforded us. Wherefore I thought fit to annex the following experiment, wherein I designed to examine, whether even those minute sorts of animals, whose bulk is thought the most contemptible, have not as well as the greater need of the air, if not to make them live, yet at least to enable them to move.

A number of ants were included in a small portable receiver exhausted yesterday about noon: between six and seven in the afternoon they seemed to be all quite dead, and the rather, because though they were very lively just before they were sealed up, running briskly up and down the bubble they were in, yet they grew almost motionless as soon as the air was exhausted; and a little while after appeared more so; though I then suspected more than I since did, that they were much inconvenienced by some small glutinous substance that seemed to have got into the small receiver from the vapours of the cement. When I looked on them at the time lately mentioned, I opened the glass, whereupon the air rushed in; but no sign of life appeared for a great while in any of the ants; but looking upon them this morning about nine o'clock, I found many of them alive and moving to and fro.

It is said by naturalists, upon the authority of Aristotle, that the animal the Greeks called ἄκαρι is the minutest of living creatures. But those of this sort being very hard, if at all to be met with here, I thought fit to make some experiments upon the least of the terrestrial animals I could procure, and try whether or no mites themselves, which are reputed but living points, and not to be taken notice of by the naked eye to be living, but by motions which even an attentive one can scarcely discover, stand in need of the air; especially because in case they do, it may suggest to us some odd reflections upon the strange subtlety and minuteness of the aërial particles, which must be capable of flowing in and passing out at the invisible and almost unimaginable small pores and other cavities of the parts of an animal, whose entire body is reputed but a physical point.

We conveyed then a number of mites, together with the mouldy cheese they were bred in to nourish them, into three or four portable receivers (which were all of them very small) not much differing in size. From all of these save one we withdrew the air, and then making use of our peculiar contrivance to hinder its return, we took them one after another from the engine, and laid them by for further observation. That one, which I took notice that we had reserved, and in which, to observe the difference, we thought fit to leave the air, was sealed at a lamp furnace, after the usual manner of nipping up glasses there. This done, there remained nothing but to observe the event of our trials, which afforded us the ensuing phænomena.

1. Those mites that were inclosed in the small glass that never came near the engine, continued alive and able to walk up and down for above a full week after they had been put in, and possibly would have continued much longer, if the glass had not been accidentally broken.

2. As soon as ever one of the receivers was removed from the engine, I looked with great attention upon it; and though just before the withdrawing of the air the mites were seen to move up and down in it; yet within a few minutes after the receiver was applied to the engine, I could discern in them no life at all, nor was any perceived by some younger eyes than mine, whereunto I exposed them. Nay by the help of a double convex glass (that was so set in a frame as to serve me as a microscope on such occasions) I was not able to see any of them stir up and down. Nor was any motion taken notice of in the other small receiver of like size and shape with mine, by them that had exhausted it of air. And my occupations not permitting me to attend the observation any longer in the place where it was made, I took the receiver, I had so attentively considered myself, along with me in the coach, and having occasion to make some stay, about an hour after I looked upon it attentively again, but could not perceive any of the mites stir; and the like unsuccessful observation I made when I had an opportunity two or three hours after that. And the place I did it in being one where I thought myself as it were at home, I first let in the air to try if the mites were not quite dead, and though neither upon its rushing in, nor during my stay there, I could perceive any of them to stir, yet I left the receiver unstopped as it was in the window, upon a suspicion that the air might not be able to produce its operation upon them in a short time.

3. And therefore passing by the same place about two or three days after, I called in to look upon my receiver, and found a number of my little animals revived, as an attentive eye might easily perceive by the motion of certain little white specks, when it was helped to observe it by little marks I made on the outside of the glass (which was purposely chosen thin and clear) near this or that mite with a diamond; by the approach to or recess from which marks the progressive motion became (perhaps within a minute) plainly discoverable, especi-

ally if we used the following expedient, (which I found the best of those I tried) namely, that when the eye perceived little white specks that looked like mites, the receiver should be so turned and returned, that the bellies and feet of those little creatures were uppermost, notwithstanding which they would not easily drop down, but continue their motion; which specks being made upon the concave surface of the thin glass itself (to which you may approach your eye as much as you please) are thereby rendered much more easily visible. But I proceed to take notice, that in the newly mentioned receiver the mites did, by stirring up and down, continue to appear alive for two or three days after, if not longer. I should not, I confess, have thought it ridiculous to suspect, that the mites which at first lost their motion, did at last really die, and that those I after saw stirring up and down, were others newly generated in the included mouldy cheese; but I was not apt to think this suspicion probable, not only because of the extreme difficulty of making any living creature to be generated in vacuo Boyliano, but because it did not seem agreeable to what I elsewhere noted about the way and time of the propagation of mites, whose eggs I have several times observed with pleasure, that at a season of the year which was unfavourable (for these things happened in a cold March) newly generated mites should in two or three days grow up to their just bigness, which several of those we observed seemed to have attained.

4. But because it doth not by the third phenomenon appear, whether or no in case our mites had been kept in a motionless state for a much longer time than three or four hours, they would have been recoverable by the admission of the air, I shall add, to satisfy that doubt, that one of the portable receivers above-mentioned, being exhausted and carefully secured from the regress of the air, was kept from Monday morning to Thursday morning, after all which time, our attentive eyes being unable to discover any signs of life among the included mites, the air was let in upon them, and after no long time, had such an operation upon them, that both I and others could plainly see them creep up and down in the glasses again.

An Account of two Books. N° 63, p. 2057.

I. Tracts written by the Hon. Robert Boyle, about the Cosmical Qualities of Things; the Temperature of the Subterraneous and Submarine Regions; and the Bottom of the Sea; together with an Introduction to the History of Particular Qualities. Oxford, 1670, in 8vo.

The main design of the noble author in these, as well as his other physical writings, is, to provide still more and more materials for the History of Nature. He prefixes to these tracts an introduction to the History of particular Qualities, after he has already given us an excellent account both of the nature and origin of qualities in general. And intending now to proceed to qualities in particular, and to consider how far the manner whereby they are produced, and those other phænomena of them, that may upon occasion be taken notice of, will accord with, and by doing so, confirm the doctrine hitherto proposed by him; and whether they will not (at least) much better comport with that, than the opinions either of the peripatetics or the chemists.

But before he descends to mention any of these particular qualities, he thinks it worth while to consider some scruples about the corpuscularian doctrine touching qualities, which unless they be removed, may not a little prejudice the reception of a good part of what he purposes to deliver about particular qualities.

Of these difficulties he insists chiefly upon that grand one, which imports, that it is incredible, that so great a variety of qualities, as we actually find to be in natural bodies, should spring from principles so few in number as two, and so simple, as matter and local motion: And here he endeavours to show, not only that the other catholic affections of matter are manifestly deducible from local motion; but also that these principles, being variously associated, are so fruitful, that a vast number of qualities, and other phænomena of nature, may result from them.

II. Catalogus Plantarum Angliæ et Insularum adjacentium: tum Indigenas, tum in agris passim cultas complectens. Operâ Johannis Raii,* M.A. et Soc. Regiæ Sodalis. Londini, 1670, in 12mo.

* John Ray, so celebrated for his knowledge in various branches of natural history, and particularly of botany, was the son of a blacksmith, and was born at Black Notley near Braintree in Essex, in 1628. He received his early education at Braintree school, and was sent from thence to Catharine Hall, Cambridge, in 1644. Here he continued about two years, and then removed to Trinity college. He took the degree of M. A. and was chosen fellow of that college. In 1651 he was chosen Greek lecturer of the college; in 1653 mathematical lecturer; and in 1655 humanity reader. These appointments show the reputation he had thus early acquired at the university. During his continuance at Cambridge he acquitted himself honourably both as a preacher and a tutor. In 1660 he published his Catalogus Plantarum circa Cantabrigiam nascentium. After this he visited various parts of England, Scotland and Wales, with a view to the promotion of the science of botany; in these excursions he was sometimes accompanied by his pupil Mr. Willughby, and others. After the restoration he entered into holy orders, and continued fellow of his college till the beginning of the Bartholomew act; which requiring a subscription against the *solemn league and covenant* (as it was called,) occasioned him to resign his fellowship; he refusing to sign that declartion. In 1663, accompanied by Mr. Willughby, &c. he travelled through various parts of Europe; and returned in the year 1665. He resided much with Mr. Willughby till the death of that gentleman, when he removed to his native place, Notley in Essex, where he continued to pursue the study of natural history. In 1673 he married the daughter of Mr. Oakly of Launton in Oxfordshire. His works are

The ingenious and industrious author of this book, having some years ago published a catalogue of the plants growing about Cambridge, has now obliged his country, by presenting it with a catalogue of the plants of all England, and of the isles adjacent. In the doing of which, he has spared neither pains nor cost, travelling himself through all the considerable parts of this kingdom, and so viewing and gathering himself almost all the plants here described, some few excepted, which he says he has either taken out of the best authors, or received from very creditable and skilful friends. Neither has he been contented to search and gather them himself, but also diligently compared them with their histories and figures found by botanists; describing those which seemed to have been omitted by others, and adding, with their characteristic notes, those that had been confusedly and carelessly delivered before.

The reader will find in his catalogue, besides the most necessary synonyma of the plants here enumerated, a summary description also of their principal virtues; intermixed with many new observations and experiments, medical and physiological.

Extract of a Letter, written by M. Hevelius, from Dantzick, July 5, 1670: Containing chiefly a late Observation of the Variation of the Magnetic Needle; with an Account of some other Curiosities in those Parts. N° 64, p. 2059.

In the year 1642 I observed the declination of the magnet here, as about the same time at Konigsberg did M. Linnemann, the then professor of mathematics there, and we both found the magnetic needle at that time to decline from the north 3° 5′ westward. But now (Anno 1670) it is far otherwise; for it declines at present, as I have very carefully observed, 7° 20′ to the same quarter; so that in the space of twenty-eight years that declination is increased

so well known that it is almost needless to particularize them: one of the most esteemed is that entitled, *The Wisdom of God manifested in the Works of the Creation.* His great work is the *Historia Plantarum,* which is partly a compilation from Bauhin, &c. intermixed with his own observations and discoveries, and arranged according to his own system. This, it must be confessed, is complex and intricate, and it has been asserted, that, however beautiful in the idea, neither the plan nor the execution is in any degree calculated to facilitate the knowledge of plants. It seems to have been Ray's great object, as of Morison, to collect together as many natural classes as possible, and these being separately investigated, a multiplicity of character and stops was necessarily required to connect them: hence the intricacy before complained of; but, as an attempt to investigate the order of nature, it is highly worthy of commendation. Mr. Ray, after having passed a long life, in the uniform exercise of piety, and the pursuit of science, died in the year 1704, in the 75th year of his age.

4° 15′. In the year 1628, if I remember right, I found it near one degree westward: which declination was affirmed by the learned Petre Cruger (once my worthy preceptor) to have been, about the beginning of this age, or the end of the next foregoing, 8° 30′ eastward; the same Cruger also, making use of that eastern declination in describing all his dials, as may be seen in the tract he has written on that subject; though it be not certainly known, by whom, and in what year that observation was made.

Further, it appears by our more recent observations, that this declination of the loadstone does here, at Dantzic, almost every seventh year, or, to speak more precisely, every sixth year and seven months, increase to one whole degree, and so each year, to 9′ 6″. Which is sufficiently confirmed by the observations made at Limehouse near London, by those three famous Englishmen, Burrow, Gunter, and Gellibrand: Of whom the first found the declination, A. 1580, to be 11° 16′; the second, 5° 36′ 30″, An. 1622; the third, 4° 3′ 30″, An. 1634.

Lastly, it being now certain that the needle's declination varies in one and the same place; the accurate observations of the subsequent years will show how far this deflexion will proceed, and where, and in what distance from the true meridian, the very bounds of this declination really are; especially, whether this libration and variation will be the same, and regular at all times and in all places; or whether, and how long, it will remain stationary. All which particulars, that they may be accurately discovered, is a thing very much desired. Possibly considerable speculations and researches may arise from such observations. As for me, I am almost of the opinion, that this magnetical diversity comes from the motion of the earth. Doubtless, as there is a certain libration in the moon, so it is not absurd to me, to hold a kind of libration in the earth, from the annual and diurnal motion of the same. For that the cause of this declination and variation of the loadstone is inherent in the stone itself, or to be ascribed to æthereal corpuscles, is not imaginable by me; nor can I yet devise any cause of those appearances, except we impute them to the globe of the earth, and the variation of the meridian.

I lately received from a person residing on the side of the Baltic sea, a piece of amber, which is so soft, that I printed my seal on it. It is yellowish as most amber is; transparent and burning as other amber; but its scent stronger, as if it were a kind of glutinous bitumen; and yet it has been cast up from the sea this year, and was found among other pieces. His brother, a very credible person, related at the same time, that he had possessed a small piece of amber, soft on one side, and very hard on the other, wherein lay buried a fly.

Extract of a Letter from Mr. JOSEPH CHILDREY *to the Right Rev.* SETH, *Lord Bishop of Sarum; containing some Animadversions of the Rev. Dr.* JOHN WALLIS*'s Hypothesis about the Flux and Reflux of the Sea, published in N° 16 of these Tracts. N° 64, p.* 2061.

My intention is not to argue against that part of the hypothesis, that relates to the common centre of gravity of the earth and moon, and the diurnal and menstrual vicissitudes of the tides, the author's discourse being, in my judgment, so rational and satisfactory as to those, that I cannot see what clear objection can be made against it. But that which I would beg his leave to except against, till better reason convince me, is his opinion concerning the annual vicissitudes, and the true cause thereof, which he supposes to be quite another thing from the common centre of gravity, namely the inequality of the natural days. For I fear he may be mistaken in the time of the annual vicissitudes, which he contends to be about Allhallondtide and Candlemas: And the reasons of my fear are these:

1. Because, if he dare stand to the general judgment of seamen, he will find very few of our English seamen of that mind, who used to say, either that the time of the year signifies nothing at all, or, if it do, that the highest tides of the year seem to happen rather about the equinoxes, than those two other assigned times, when the natural days are longest and shortest.

2. Whereas he gives an instance or two of very high tides in the Thames in November 1660 and 1665; the truth of which we need not question, and of which there are sundry other the like instances in our English chronicles; I have reason to believe, that those high tides proceeded from another cause than what he supposes. For first, If that which he supposes should be the cause, the like high tides might be expected every November, and they should happen as frequently about February as about November; of which yet he gives not one instance. And though I have perused thoroughly that perfect collection I have of all the high tides in the Thames that our chronicles take notice of since the conquest, I can hardly find one such high tide in the Thames in February, or thereabouts. Secondly, Those high tides in the Thames in November, if we dare credit the London watermen, are caused by the coming down of the land-waters after a very great rain, which being encountered by the tide of the flood from the mouth of the Thames, cannot but swell to an unusual height. Now if the great rains fall so, that the land-waters come down to the flowing part of the Thames, just upon the full or change, when the spring tides happen, as they did (for example) September 30, 1555, and October

22, 1629, (Stow and Howes are my authors,) those spring tides must be the higher, as proceeding from a double cause. But,

3. There is another thing well known by all seamen to be a cause of high or low tides, which I wonder the author has taken so little or no notice of in his essay, namely the sitting of the wind at such or such a point of the compass, and blowing hard. It is the constant saying of all seamen in Kent, that ever I met with, that the north-west wind makes the highest tides in the Thames, Medway, and all the coasts about the south and north Forelands; and likewise on the coast of Holland and Flanders. And the reason they allege for it is, because that wind with equal force blows in the tide of flood at both ends of this island of Britain, that is, from the northward between the coasts of Scotland, Norway, and Jutland; and also from the westward by the coasts of Cornwall, Devonshire, Dorsetshire, &c. up along the sleeve; and, for the same probable reason, they say that a south-east wind deads and hinders the tides in the places near the Forelands. And agreeably to this, I very well remember when I was a boy, and lived at home with my father at Rochester, which is near enough to Chatham to observe how the tides run there, that when the tides were unusually high, the wind was always north-west, and the moon near the full or change. And so confident I am of my memory in this point, that if inquiry be made about Chatham, the hundred of Hoo, and the Isle of Graine, I believe the inhabitants will with one voice say, that they never fear their low marshes being overflowed by the tide, but when the wind is at north-west, or thereabout, upon the spring tides. Here at Weymouth those able and ancient seamen I have talked with, tell me, that a S S E. wind makes the greatest tides, and that, according to the degree of the wind, *cæteris paribus*, the tides rise more or less notably; but that they never observe any extraordinary tides about Allhallondtide or Candlemas, unless the wind be about S S E. And the reason they give for that wind's raising the tides there is very convincing, if we consider the lying of the haven in the map. And, for the same reason, I suppose the wind from the same point may make the highest tides at Southampton; a westerly wind at Bristol and the Severn; an easterly wind at Hull; a north-east wind at Wisbeach and Lynn; a southerly wind upon the opposite coasts of England and Ireland, &c. It is true, March is very often more stormy than February, though seldom so stormy as October and November, which possibly might occasion that opinion, which some hold, (of which number Pliny is one,) that the highest tides are about the equinoxes. And if the thing were found to hit pretty frequently in March, men might not be careful to observe the other equinox; though yet it cannot be denied, that we have blustering weather many times before Michaelmas.

In confirmation of all this, that I have said concerning the influence of the wind's being considerable on the tides, I shall add these following collections of my own out of histories, chronicles, &c.

1250, Oct. 1, (says Holinshed) upon the change of the moon, was a most dreadful inundation of the sea, that did exceeding much hurt to Holland beyond sea, Holland in Lincolnshire, and the marsh ground in Flanders, and drowned Winchelsea. But he tells us withal, that an unheard of tempest of wind accompanied it.—1555, Sept. 30, (says Stow) was a notable inundation of the Thames; but he says withal, that it was by occasion of a great wind and rain that had fallen.—1569-70, March 10, I find this manuscript not in Latin in an ephemerides for that year, over against the day; Septentrionis maxima sævitia: Nivis flocci magni, ingens frigus. Maximè tumescebat æstus maris die et nocte; nam excurrebat in agros latè.—1592, Sept. 6, Wednesday (says Stow) the wind being west and by south, as it had been for two days before very boisterous, the Thames was made so void of water, by forcing out the fresh and keeping back the salt, that men in divers places might go 200 paces over, and then fling a stone to land, &c.—1600, Dec. 8, I find this note written in another ephemerides for that year, over against the day, by an unknown person, who it seems was then at Venice (where a south-east wind makes the highest tides), Inundatio Venetiis 6. ped. temp. Sirocco.—1601, (says Grimston in his Netherland History) the sea being forced in by a strong N. W. wind, did some mischief to Ostend.—1601, Oct. 26, n. s. a great tempest, (says the same author) and the wind west and north-west, and the tide much higher than usual at Ostend.—1603, Feb. 23, 24, n. s. blew a terrible north-west wind, which made the water rise higher than usual at Ostend. Idem.—1604, March 1, n. s. the wind was very great at west and north-west with a furious tempest, the tide at Ostend rising higher than it had done in 40 years before. Idem.

4. There is yet another thing, which seems to have some influence on the tides, and to make them swell higher than otherwise they would do, to wit, the perigæosis of the moon. And this has been my opinion (taken up first on the consideration of the moon's coming nearer the earth) ever since 1652, when living at Feversham in Kent near the sea, I found by observing the tides, that there might be some truth in my conjecture; and therefore in a little pamphlet, published in 1653, by the name of Syzygiasticon instauratum, I desired, that others would observe that year, whether the spring tides after those fulls and changes, when the moon was in perigæo (the wind together considered) were not higher than usual. And since that time I have found several high tides and inundations to happen upon the moon's being in or very near her

perigæum. For example, 1. That famous inundation, mentioned before out of Holinshed, 1250, Oct. 1. was, when the moon was in perigæo, as appears by calculation.—2. 1530, Nov. 5. That inundation on which was made the distich,

Anno ter deno post sesquimille, Novembris
Quintâ, stat salsis Zelandia tota sub undis;

was when the moon was in perigæo.—3. Jan. 13, 1551-2, the sea (says Michell in his chronicle) broke in at Sandwich, and overflowed all the marshes thereabout, and drowned much cattle: the moon in perigæo.—4. 1570, Nov. 1, was a dreadful flood at Antwerp, and on all the coasts of Holland, that made infinite spoil: the moon in perigæo.—5. 1600, Dec. 8, above-mentioned: the moon in perigæo.—6. 1606-7, Jan. 20, was a great inundation in the Severn, mentioned in Howes's chronicle, that did much hurt in Somersetshire, Glocestershire, &c. the moon in perigæo.—7. 1555, Sept. 30, the moon was in perigæo.—8. 1643, Jan. 23, n. s. (says a little Low Dutch chronicle that I have) was a terrible high water-flood in Friesland, &c. whereby much hurt was done to the dykes; and at Gaes by Haerlingen the dead bodies streamed out of the earth: the moon in perigæo.—9. 1651, Feb. 23, n. s. (says the same chronicle) was St. Peter's high flood, whereby much hurt was done to the dykes in Friesland, Embderland, and elsewhere, and not far from Dockum by Oudt-woudumer-ziil is a breach of 42 roods long broken in the dyke: the moon in perigæo.—10. Aug. 2, 1657, at Feversham was a very high spring tide, and yet the wind was at south-east; which deads the tides there: the moon in perigæo.—11. Aug. 22, 1658, at Feversham was a very high tide in the afternoon, though the wind was southerly, and blew very stiff, which the seamen there wondered at: the moon in perigæo.—12. 1661, upon Michaelmas day was a great overflowing of the Severn, that it drowned the low grounds lying by it: the moon in perigæo.—13. The scheme of the weather, printed in the history of the R. S. tells us, that May 24, 1663, was a very great tide at London. But it tells us withal that the same day the moon was in perigæo.—14. Sept. 1, 1669, at Weymouth I observed a very high tide; and so did several seamen in that town, who wondered at it, the weather being very calm and that little wind that was being at north-east, which uses to contribute nothing at all to the tides in that haven: the moon in perigæo.

Further, what inclines me to believe that the perigæosis of the moon is of some concernment in this matter, is, because it is a maxim among our Kentish seamen, that they never have two running spring tides (as they call them) together, but that the next spring tide, after a high running spring, is proportionably weak and slack; which, if true, is very correspondent to my opinion, because, if the moon be in perigæo at this spring tide, she will be in apogæo at the next.

But I conceive the best touchstone to prove the soundness of my opinion is, to have it observed, whether those neap-tides be not apparently higher that happen at the moon's being in perigæo either at the first or last quarter: because it is a received and demonstrable truth in astronomy, that the moon being in perigæo at either quarter, comes then nearer the earth than when it is in perigæo at the change or full. And I could wish, for the further clearing of this matter, that observation were made at Bristol (because there is the most considerable flux and reflux of any port of England,) whether this year, 1670, the tides be not higher when the ☽ passes ♓, ♈, and ♉, than when she passes through the opposite signs ♍, ♎ and ♏; and particularly whether the spring tides be not sensibly higher after the change than after the full in February, March and April; and higher after the full than after the change in August, September, and October; and also, whether the neap tides in May and June rise not apparently higher than expected. I am promised the observation shall be made here at Weymouth for this whole year round; from whence I have already received this account, that this present February, 1669-70, the spring-tides ran very high after the change, though the weather was pretty calm, and that wind that was not very favourable to the tides, and that the spring-tides after the full were very low, and weak; which is exactly according to my conjecture.

Dr. Wallis's Answer to the foregoing Animadversions, extracted from his Letter of March 19, 1669-70. *N°* 64, *p.* 2068.

That the winds have a great influence on the tides of particular coasts and havens, according as they are more or less stiff or slack, and blow from this or that part, I do not at all question; but took for granted, as generally received, and upon good grounds. And the like I say of land waters; which are, as to inland rivers, very considerable: especially as to inundations upon rising of the water: which is rather by checking than promoting the tides. But he need not wonder, that in my essay, though I grant both these, I said so little of either, because it was wholly beside my business; which was to give a statical account of stated periods (diurnal, menstrual, and annual,) arising from regular motions; not of accidental extravagances, such as these are; and therefore I did, in the beginning of that discourse, preclude the consideration of the advantage or disadvantage which should arise from such uncertain contingences, as extrinsical to that business.

His third thing suggested, the moon's perigæosis, is so far from being contrary to my hypothesis, that it is a great part of it. And it is in one of my letters to you expressly mentioned as such. But forasmuch as it does not still fall out at the same time of the day, month, or year, I could not make it a component of any of those noted periods, diurnal, menstrual, or annual.

The account which I give of the diurnal and menstrual periods (from the common centre of gravity of the earth and moon) he doth allow as very rational: and consequently that any acceleration or retardation of the compound motion of the particular parts in the earth's surface, is to give such an accumulation of waters as causes a tide; and the complication of such accelerations and retardations, concurring or interfering from one with another, occasions the perplex varieties in them.

Now as to the two most signal motions of the earth, the diurnal and annual; if we suppose them each in themselves equal, and both perfectly circular and upon parallel axes, though neither of them, singly considered, would give an inequality of motion; yet the compound of both together, being swiftest at midnight and slowest at noon, would give us two tides in each diurnal revolution; but those always at noon and midnight.

If to these we add the menstrual, whereby the earth describes a small epicycle about the common centre of gravity of the earth and moon: and suppose this also equal in itself and circular, about an axe parallel to the rest; neither would this of itself give any inequality, but compounded with the rest it will, at those times, give us two tides.

Now because this coming of the moon to the meridian, above and below the horizon, or (as seamen call it) the moon's southing and northing, doth in a month's time pass round the whole circle of 24 hours; hence it comes to pass that the time of the tides does so also, which I take to be the true account of the menstrual period. And because this composition of the menstrual with the diurnal casts the time at the moon's being in the meridian, and that of the annual and diurnal, when the sun is in the meridian; when both these happen at the same time, as at the full and change of the moon, the tides must needs be the greater; which I take to be the true account of the spring tides and neap tides. And thus far (which is the main of my hypothesis) he concurs with me, as having given at least a very rational and probable account.

Because it has been almost generally received, that there is an observable annual period; I did, for the solving of that, apply not the inequality of the natural days, but those causes from whence that proceeds, the eccentricity of the sun or earth's orb, and the obliquity of the zodiac. The former of these, if singly considered, would cast those annual tides in June and December (the times of the sun's apogæum and perigæum, or rather the earth's aphelium and perihelium, when are the slowest and the swiftest annual motions in the zodiac); the latter, if considered alone, would cast them upon the two equinoxes, and the two solstices. But if both be jointly considered, they must cast these at some intermediate times between the autumnal equinox in September, and the perigæum in December; and again, between this perigæum, and the following vernal equinox in March. I only mentioned, as a thing very notorious, that it does so constantly fall out on the coasts of Kent, and particularly of Romney-marsh, about Allhallondtide and Candlemas.

This account of the annual vicissitude is that only to which M. Childrey does except, opposing first, the judgment of seamen, who used to say, either that the time of the year signifies nothing, or, if at all, it is about the equinoxes. Then, that if this be the cause, it will be constant, and that in February as well as in November. And thirdly, that the seamen about Weymouth have not observed any thing signal about those times.

To the first I answer, if not then, but at the equinoxes, then so much of the hypothesis as concerns the eccentricity may be spared, or allowed to be so little as not to be remarkable; and that of the obliquity alone will give a sufficient account of it. Or if there be no such annual vicissitudes at all, then may that of the obliquity be spared also, and the hypothesis perfect without it. And, till some such be observed and acknowledged, it will be sufficient to say, that, though both the eccentricity and obliquity do cause some inequality in the motion, yet so little as that in the tides it is not remarkable.

To the second, which concerns matter of fact in Romney-marsh, I say, that, according to the best account I can there get, it is constant, hardly missing, or very seldom, any one year, and as well about Candlemas as about Allhallondtide, every year, though not then so high; of which I think a cause may be very rationally assigned. For, if you consult the tables of the inequality of natural days, you will find, that about one of the extremes (in January) the increase and decrease of the natural days fluctuate very much, sometime increasing sometime decreasing, according as this or that of the two causes, thwarting one another, doth prevail; but about the other extreme (in October) it is much otherwise, the increasings and decreasings going on in a continual course for a long time together. And the same causes, applied to the business of tides, may very rationally be supposed to produce as unequal effects.

To the third, that the seamen at Weymouth have not observed any such signal effects about Allhallondtide and Candlemas; it is very possible that they have not, and that nothing signal on those coasts used to happen at those times; for I fix that matter of fact principally on Romney-marsh, and do but by conjecture extend it to the river of Thames, where yet I think you can be my witness, that it has been observed several years to succeed accordingly. What variety is on other coasts I am not certain, but from an account read in the Royal Society in my hearing, about the end of the year 1667, I understand that about Chepstow bridge they observe the like to happen about the beginning of March and end of September, the one as much before the vernal as the other is after the autumnal equinox, like as in our case it happens, which they call by the name of St. David's Stream and Michaelmas Stream; as we do those in Kent, Candlemas Stream and Allhallond Stream; and when seamen take so much notice of particular tides as to give names to them, it is a great presumption, that it is for some remarkable accident usually happening at those seasons.

And if to this of the sun's or earth's be added that of the obliquity and eccentricity of the moon's orbit, it will, if it do no good, at least do no hurt. And I the rather think it may be considerable, because the earth and moon's appropinquation and elongation do really alter the distance of the common centre of gravity, (of the earth and moon) from the earth (rendering the earth's epicycle elliptical) and much favours what M. Childrey observes of the moon in perigæo. And this is the sum of what I thought proper to return you upon those animadversions, being, &c.

An Account of some Books. N° 64, p. 2074.

I. Dissertationes Medicæ Tres: 1. De Causis fluxûs Menstrui Mulierum. 2. De Sympathia variarum Corporis partium cum Utero. 3. De Usu Lactis ad tabidos reficiendos, et de immediato Corporis Alimento. Auth. Francisco Bayle, Doct. Medico. Tolosæ, 1670, in 4to.

Upon the subjects here enumerated this author has no observations that can interest the physicians of the present days.

II. Historiæ Generalis Insectorum, Johannis Swammerdami, Pars prima. Ultrajecti, 1669, in 4to.

This curious and philosophical book, written in the Belgick tongue, treats chiefly of these three things:

First, It lays down the ground of all natural changes in insects, declaring, that by the word change is nothing else to be understood but a gradual and natural evolution and growth of the parts, not any metamorphosis or transformation of them; which growth is here made to resemble, not only the increase of other animals, but also the budding, knitting, and spreading of plants. And here the author, having taken notice, that whereas ancient and famous writers have esteemed and called the nympha among insects the change of that worm, which carries the proper shape of the future little animal, and the chrysalis or aurelia the change of that caterpillar, which shows no parts at all of the animal to come; having, I say, noted this, he desires the reader to observe, that whereas he is able to discern and to show all the parts or members of the future animalcule, as well in the chrysalis as the nympha; he makes no other difference among them but this, that since the parts in a chrysalis are not so plainly discernible to our view as those in a nympha, and because a chrysalis does sometimes appear of a gilt-colour, which he has not hitherto observed in a nympha, he calls the nympha barely by the name of puppet, and the chrysalis by that of gilt-puppet; the distinction of which is made very clear by the neat and accurate cuts annexed, and their explication. In this first part is set forth the manner how the worms and caterpillars turn into puppets, and shown that some insects come perfect out of the egg, and never out of a puppet; that the principal difference of the worm-animalcula that turn into puppets consists in this, that some have feet, some have none; that the breast of the feetless worms is never changed; that the six fore feet of the worms with many or few feet are never changed or transposed; that the wings, horns, feet, &c. grow up under the skin by degrees; that in all worms he can easily show the said parts under the skin, affirming to have done it actually in the presence of Signor Magalotti, and Monsieur Thevenot, two very intelligent and cautious persons, and that even a frog comes forth into a puppet.

Secondly, this book undertakes to make it out, how the true ground of the natural changes, or the knowledge of the nympha and aurelia, has been obscured and marred; showing how it is to be cleared and restored again. Where he affirms, that Moufet and others do err about the aurelia, making it neither an egg nor an animal; and that Harvey mistakes, calling the aurelia (which indeed is the animal itself) an egg, and affirming, that bloodless animalcula are produced out of aurelias by transformation, whereas the change happening in the puppets is nothing else but an evaporation of the superfluous moisture. Further, that Goedart errs, holding that a caterpillar may change before her time; and that, if she so changes, she then produces another animal; contrary whereunto our author affirms, that these animalcula, which the said Goedart mentions as changing against the order of nature, do always come forth in that manner, viz. the male with wings, and the female without them; observing further, that caterpillars early forbearing to eat, come only to turn into smaller bodied animals; and adding that they may change when they will, and that the animals when once changed do never grow larger. And from the knowledge of the propagation of these animalcula he is of opinion, that we may arrive to that of the propagation of the rest of animals; where he declares his sentiment, that there is no generation in nature, but only a production by the growing of parts, adding this assertion, that he is able both to show all the parts of a butterfly in a caterpillar, and to make the change of the caterpillar to proceed leisurely, and so to stop it in its change, that it shall appear half caterpillar and half aurelia; which he says, he has actually performed before the Great Duke of Tuscany.

Thirdly, This author reduces all changes of insects, (some few excepted, which he acknowledges he does not yet well understand,) into four classes or ranks; which are discriminated by four different ways of production, change and growth. The first rank, by him called nympha-animal, has a little animal fully formed in the egg, which after the evaporation of the superfluous moisture, comes forth perfect, and so grows up; such as the louse and flea, &c. The second, called nympha-vermiculus, has the parts of the insect imperfectly shaped in the egg, and after hatching acquires its perfection visibly by outward food; such as the locust and cricket, &c. The third, called nympha-chrysalis or aurelia, obtains (after hatching,) its perfection darkly, and not till the last casting of the skin; such as the emmet and night-butterfly: So that in the second and third classes not a perfect animal, but a worm, precedes the growing up of the parts; yet with this difference, that in the second, the little creature grows up manifestly; which in the third is done obscurely: The fourth, called nympha-vermiformis, remains always shut up in the skin of the worm, without a possibility of discerning the parts, till casting both skins at once, it becomes capable of generation; such as the fly.

In the explication and deduction of all which differences, the author takes notice of many remarkable particulars: E. g. That the insects, which come perfect out of their eggs, change only by casting their skin; and those that come forth imperfect, do, besides skin-casting, grow up by food, to become nymphas or puppets: That those which come perfect or imperfect out of the egg, are in the egg first like puppets, and undergo both of them in the egg all the alterations which any insect undergoes in the puppet: That the parts of puppets protuberate, much like the budding of flowers: That the caterpillar is the butterfly itself, only covered over with a mantle, whereby the parts are kept from our discerning: That the doctrine of Signior Malpighi, in his dissertation de Bombyce,* concerning the change of butterflies, is true: That innumerable insects fly about and feed by night, as well as others do by day: That snails discharge their excrements by the neck, and are each of them both male and female: That from caterpillars, feeding on such and such plants, conjectures may be drawn concerning the agreement of the respective qualities of them; it being very probable, that, if those creatures do eat of several plants (each sort of those insects being esteemed to feed but on one sort of vegetables,) those plants do agree in their nature and virtues, &c.

* See p. 367 of this vol. of our Abridgement.

Those insects, which the author can as yet reduce to no classes, are, cicindela, scolopendra, julus, curculio, scarabæus pilularius, hydrocantharis, hydrocantharis minimus, scorpio.

III. The Creed of M. Hobbes, examined by M. Tenison. London, 1670, in 8vo.

Passing by the several particulars, which concern morality and policy, discussed in this book, as not belonging to these Tracts, which are principally designed to give an account of such occurrences as are of a physical and mathematical nature; we shall only take notice of the confutation made by this author, of what M. Hobbes has delivered concerning the rational soul, and perception in matter; where it is contended, that the soul of man is something else than the organized body in due motion; and that it is altogether inconceivable, that matter should be capable of perception, cogitation, and discourse.

IV. Francisci Josephi Burrhi, Epistolæ duæ ad Thomam Bartholinum. Hafniæ, 1669, in 4to.

Containing nothing entitled to notice.

An Extract of a Letter, written to the Editor from France, Oct. 29, 1670, *intimating two New Anatomical Discoveries. N° 65, p.* 2083.

A friend of mine, a professed physician, has assured me, that at Montpelier, a German has discovered the vessels which convey the chyle to the breasts of nursing women; and showed, that they do issue out of the ductus of M. Pecquet. This is a discovery of a thing, the existence of which has been believed long since, though not clearly proved.* Another person has assured me, that there is certainly another passage of the urine to the bladder than by the ureters; an experiment having been lately made, whereby the ureters of a dog were so carefully tied up, that nothing could pass that way, and yet the urinary bladder was found full of water.

A Narrative of several odd Effects of a dreadful Thunder-clap, at Stralsund in Pomerania, 19-29 *June* 1670; *taken from a Narrative there printed, by Authority, in High Dutch. N° 65, p.* 2084.

June the 19-29, being Sunday, after several less strong reports of thunder, the whole town was strangely surprised with a most terrible flash of lightning,

* Such a discovery was never yet made. The milk is secreted in the breasts, by vessels which have no communication with the thoracic duct. It was probably one or more trunks of the mammary lymphatics, or of the lymphatics going from the axillary glands, which this anatomist traced to the thoracic duct, or to the place where this duct empties itself into the subclavian vein.

and a fearful thunder-clap, which lighted down through the lesser steeple upon the body of St. Nicholas Church, and through the round large hole in the upper vault within the same, in the shape of a black fiery ball, directly upon the altar, causing such a hideous crack, fire-flash, smoke and damp there, as if many fire balls had been thrown down from the said vault, and had burst all at once; causing a dismal consternation among the people in the church, and leaving a bad sulphureous smell behind. The candle on the south-side of the altar was put out by the blow, the other remained burning. Two of the chalices were overthrown, and the wine spilt, and the wafers scattered about; but the empty chalice stood firm. All three were somewhat smutted at the foot, and one of them a little bent, and pierced through in two places, as if by hail-shot; the wafer-boxes were likewise a little smutted towards the bottom. The church book was flung on the inner passage: The covers of the altar were singed in divers parts, as by powder, and somewhat burned and smutted here and there, as also torn in some places. A strong piece of wainscot, with a picture on it, behind the great altar, was split in two. Of the church clock in the west-end, at the same time, both the brass and iron-wires of the whole and quarter-hours hammer were partly broken, and the rest could not be found; and an oaken post fixed in the wall for the support of the dial, was half torn, and beneath the same several bricks were struck out of the two head pillars supporting the steeple. On the top of the southern steeple, an oaken gutter and a strong beam and supporter were shattered, and fell dangerously, but that one part of it held fast yet by one nail.

One of the ministers, though sitting near the altar to the south, had no hurt. Divers of the people seated round about the altar fell down to the ground with the fright. One youth that stood near the minister's pew, not being able to recover his senses, was carried home. On the north side of the altar, four persons fell down, and one of the oaken seats being split, the person sitting on it was much more hurt by it than any other. Some that stood in or by the belfry, near the clock, were slightly hurt here and there; and among them a mariner, leaning on a lined oaken seat there, had his right arm bruised; and another man, though but slightly hurt, yet could not remember how he got home from church.

The church dial was also much smutted in sundry parts, the gilt figures so soiled that they could scarcely be discerned. The gilt weather-cocks, upon both the steeples, were likewise smutted on the one side of their tails, without any other mark. Nor could it be in the least discovered, in either of the steeples, which way the claps entered, by all the search that was made.

Eight persons only were hurt: of these the following particulars were observed: One that stood in the belfry, had the upper back part of his cloth-coat, as also his shirt and skin somewhat torn; but the lining of that coat, which was red frieze, had no hurt. Another sitting in a pew under the organs, and leaning on the door, while the pew-lock, then close to his body, was so violently struck out, that it hung only by one nail, had no damage at all by it himself, nor any other that sat or stood near, when the stroke happened, though they fell all to the ground by the fright, at the instant when it was given. And as for him, that had his arm bruised, it was somewhat strange, that afterwards there was found a hole passing his coat, waistcoat, and shirt on the forepart of his body, without in the least hurting the body; the hole appearing just as shot through. And notice was taken, that the man's waistcoat, being of a red sarsenet, kept its colour every where, but at the place where the arm was hurt; and the waistcoat being edged with a small silver lace, the lace was smutted almost every where, and about the neck too, where the party wore a cravat. The same person had also one half of a shoe torn off, the sole being pierced as with hail-shot; and a piece of his stocking foot on the same foot struck away, near a hand breadth; without any other hurt to either foot or leg, but that for some days that foot was benumbed. Lastly, one of those that sat by the altar, had his breeches and leather-drawers on both sides pierced through, as by hail-shot, and part of it plainly scorched and shrunk up, as by fire; and divers of such small holes in his shirt too, yet without any hurt in his body, save, that he found some pain in his foot. One side of his shoe also was half torn, and the sole sidewards pierced through, as it were, with hail-shot.

Of a New Star discovered in the Constellation the Swan, by M. Hevelius. Also the present Appearance (August 1670) of the Planet Saturn. Translated from the Latin Letter, from Hevelius to the Editor. N° 65, p. 2087.

I write this to announce to you a newly discovered fixed star, almost of the third magnitude, just below the head of the Swan. Its longitude is now 1° 52′ 26″ in Aquarius, and lat. 47° 25′ 22″, as appears by my observations on July 25, an. 1670. There can be no doubt that this is completely a new star, and not visible in the year 1660. For in the years 1659, 1660, 1661, it happened that I made observations on almost all the stars in this constellation with great care, and with proper instruments; observing all those about the neck and head, and measuring their distances from several fixed stars. But I found no star of the third magnitude in that place where this new star has been seen:

though I must have seen it, had it then been there. So that it hence appears certain, that in the years 1660 and 1661 this star was not yet visible: it also clearly appears, from *Bayer's Uranometria*, that this new star did not appear in 1603; consequently not to Tycho, and much less to Hipparchus. For Bayer would have perceived and inserted a star of that magnitude, since he describes one of the sixth magnitude not far from it; as may be seen in his constellation of the Swan. Perhaps you may think this star of Bayer's is the same as my new star; and that, as he did not observe the stars with proper instruments, so he might easily err by a degree or two from the true place. But this cannot be allowed, since that small star still continues in the same place where Bayer has set it, and is still seen a star of the 6th magnitude as he observed it. For, as I have lately found, it is distant from Pegasus's mouth 32° 39′ 0″, and from his right knee 39° 23′ 45″; from hence its longitude comes out 6′ 28″ of ♒, and its latitude 46° 11′ 14″ north, in July 1670. But the new star is distant from the mouth of Pegasus 32° 31′ 25″, and from his right knee 38° 18′ 50″; from which distances its longitude is found to be 1° 52′ 26″ ♒, and latitude 47° 25′ 22″ north. So that this new star is evidently different from that of the sixth magnitude observed by Bayer, though these two are not above two degrees distant from each other. And, from what is above said, it is also manifest, that this new star did not shine among the other stars, either in the year 1603 or 1660.

When I first observed it, it was not inferior in brightness to the star in the breast of the Eagle, except that its light was a little more obtuse. As to its situation, in respect of the other stars, it was placed in a right line with that in the bend of the Swan's upper wing, and that in the shoulder of the Eagle; as also with the bright star of the Harp, and that in the rhombus of the Dolphin, which is the more northern of the middle ones. It made an equilateral triangle with those in the head and back of the Swan.

It however wonderfully decreased in the month of September, so that by the 14th of October it could not be observed at all with an instrument; as appears by a future letter from Hevelius.

The Observation on Saturn's Ring in the same Letter.

The telescope of 50 feet long, which you lately sent me, showed the face of Saturn very distinctly, though the moon was up, the 26th of August 1670, when it and the ring appeared rather long on both sides. The appearance was very different from that seen by you, and by M. Huygens, in 1666, and also by the Paris observers in 1668. For the ring which encompasses Saturn was now found to be much narrower than at those periods; his path being now much more oblique in respect of the earth.

An Account from Paris, in two Letters to the Editor, dated July 5 and July 19, 1670, concerning the earlier Discovery of the same New Star, which is described in the preceding Letter. N° 65, p. 2092.

The letter of July 5, 1670, states, that a Carthusian of Dijon, on the 20th of December, 1669, discovered a star of the third magnitude, beneath the head of Cygnus, situated in the section of the two straight lines, one of which goes from Lyra to the nearest of the quadrangle in the Dolphin, and the other from the Eagle to the star which is on the top of the upper wing of Cygnus.

That of July 19, 1670, gives this account, viz. That there was much conversation at Paris about the new star near the beak of Cygnus first discovered by a Carthusian of Dijon, called Anthelme, who sent the news of it to M. L'Abbe Mariotte, one of the Royal Academy there, who had communicated it to the rest: that they all agree, it is a new star; though Mons. B. opposed it at first, affirming it to be in Bayer's tables. They prove also the star in Bayer to be another; giving, for distinction, these measures:

The bright star ad Rostrum Cygni its ascensio recta	289°	22′	0″
Declinatio Borealis	27	19	20
But this new star's Ascensio recta	293	33	0
Declinatio Borealis	26	33	20
Longitudo	1	55	♒
Latitudo	47	28	10
Its distance from that ad Rostrum Cygni towards jaculum .	3	47	0

Some Communications, confirming the present Appearance of the Ring about Saturn. By M. HUYGENS and Mr. HOOK. N° 65, p. 2093.

The former writes, in a letter dated the Hague, Oct. 31, 1670, n. s. That he observed Saturn with his telescope of 22 feet, a little before he came out of France, which was the last summer of this year 1670, and saw his figure to be quite conformable to what it should be according to his hypothesis; viz. the ansæ or arms to be very narrow, so that their opening appeared but very obscurely.

The latter has observed the same in London, September 16, o. s. having the same appearance.

An Extract out of a lately printed Epistolary Address, made to the Grand Duke of Tuscany, touching some Anatomical Engagements, of LAUR. BELLINI, *Ord. Anat. Prof. at Pisa.* *N° 65, p.* 2093.

The mechanical notions of this Italian anatomist being now for the most part rejected by physiologists, a detail thereof ceases to be interesting.

A Narrative of a Monstrous Birth in Plymouth, Oct. 22, 1670; *with the Anatomical Observations, taken by* WILLIAM DURSTON, *M. D. and communicated to Dr.* TIM. CLERK. *N° 65, p.* 2096.

This birth, (as you see fig. 1, pl. 13.) had two heads, and two necks, as also the eyes, mouths, and ears suitably double. Four arms with hands, and as many legs and feet. There was to both but one trunk; though two back bones, from the clavicles to the hypogastrium, and from the shoulders down to the bottom of the loins they were not distinct, but cemented and concorporated after this manner: The right clavicle or channel bone of the right hand child (being long) joined with the left clavicle of the left hand child. The ribs on the face side of both of them, by the cartilages or gristles were united without any intervening sternum or breast bone; and so made a common chest to them both: and the ribs of both on the back part were united by the gristles; and from the clavicle down to the hypogastrium or bottom of the belly, they were so conjoined, that they made but one common belly, with one navel string to them both; but from the hypogastrium downwards they were divided, and became two, each having the perfect parts of females.

Having with some difficulty obtained the father's leave to dissect it, we first weighed this birth; the weight whereof was eight pounds and a quarter; the circumference of the left head was about eleven inches, that of the right being half an inch less. The circumference of the trunk was about sixteen inches and a quarter; and the length of both, from head to foot, was full eighteen inches and a half.

We found one navel vein, and one liver, but that was very large, with the bladder of gall seated in its usual place: but there were two urinary bladders, two wombs, four kidnies, and one stomach, with the œsophagus or gullet perforate and open from the mouth of the left head; but the œsophagus from the mouth of the right head descended no lower than a little above half an inch off the midriff, and there it ended. No further could we follow it with the probe, but doubting a failure in that experiment, we made an essay with a blow pipe, and thereby we found, that the wind would go no further than the place above-mentioned. Whence it may be concluded, that the right-handed child must have received its nourishment by and from the left child.

There was but one colon or colic gut, which terminated in two intestina recta. So there was but one midriff, and above that we could find little or no appearance of lungs; but only a very large heart, with two auricles, the figure of which was not conical, but like a soldier's pera or knapsack, or the ventricle or stomach; and lying near under the clavicles, transverse as the stomach lies under the midriff and liver. We also observed two ventricles, with the tricuspid and sigmoid valves; as also the vena cava and aorta dependant, [descending] and also the aorta ascending and bifurcate towards each neck, and then bifurcate again.

These twins were exactly like one another; very well featured, having also pretty neat and handsome limbs. They had their hair more than ordinarily thick, and about half an inch long, and the nails full grown.

Observations on Insects lodging themselves in Old Willows. Produced before the Royal Society, by Dr. EDMUND KING, July 14, 1670. N° 65, p. 2098.

About the beginning of May last, a piece of old willow wood being sent me from Sir John Barnhard from Northamptonshire, was produced before the Society, in which were lodged many insects curiously wrapt up in green leaves, in several channels or burrows, each with 12, 14, or 16 leaves round the body, and several of them with as many little round bits of leaves at each end, to stop them up close. These, thus made up, are near an inch long, put one after another into a bore made in the wood, fit for their reception. They are in the manner of cartridges of powder, wherewith pistols are wont to be charged, or like long slugs of lead, as are sometimes used in some parts of those burrows; they are placed so near one another as to touch; in others at some considerable distance. These insects observe this method in placing themselves, that sometimes they make a direct way into the length of the wood, sometimes they bore out into the side, and run another way, those channels being not unlike the burrows of rabbits; all which they fill up with these round appearances of wrapt leaves, all regularly wrought: In which I find either something alive, or appearances of something that has died there and is putrified: In some a great number of mites, of a dark ash colour, shaped not unlike common mites; in others I find seeming excrements of some small insect, with the decayed parts of the dead insect; in others white maggots. Some of these maggots I took out of their theca or bag, and put them in warm places in the sun; and they thereupon grew something larger, but changed not shape nor colour, but died. The rest I kept close in a box till the 8th of July present: then I took one of them out of the wood, and opened the leaves, and felt something stir, hearing also a humming noise like that of a bee; and as soon as I had opened the theca, a perfect bee flew out against my window, as strongly as a common bee out of a hive, having much of the colour and size of those when they are new flyers. Being pleased at the sight, I took five more, being all I had left, and put them in a box into my pocket, to show them to Lord Brouncker, before they were taken out, or had eaten their way out; but his Lordship not being at home, I brought them back again; and they being disturbed, they all eat themselves out; and coming home they were all creeping about my box, and are since dead there. They have all stings as other bees have. I had some thoughts at first sight, by the yellowish circles under their bellies, that they might be a sort of wasps; but now am of opinion, that they are common bees.*

Extract of two Letters, written by FRANCIS WILLUGHBY, *Esq. to the Editor, from Astrop, August* 19, *and from Midleton, Sept.* 2, 1670, *containing his Observations on the Insects and Cartridges described in the preceding Account.* *N*° 65, *p.* 2100.

I had the good luck to find a great many of your cartridges in a rotten willow; and, by the shape of the maggot, was most confident they would produce insects of the bee tribe. And this I should most certainly have foretold you, had I ever received those you sent me by Mr. Le Hunt: But having only that which you sent me before, I was so choice of it, that I durst not open it. I think now I have found out the whole mystery; and if you please to send me Dr. King's account, and one of your bees, I may perhaps add something; and shall be glad to be instructed in any thing that has escaped me. I desire one of the bees, because all mine being of a late hatch, and none of them yet turned into nymphas, I fear I shall not see their last metamorphosis this year.

* The species of bee mentioned in these papers is most accurately described by the ingenious Mr. Kirby, in his work, entitled *Monographia Apum Angliæ.* Mr. Kirby names it *Apis Willughbiella.* A. fulvescente-pubescens; abdomine brevi, ano nigro, antennis *masculis* articulo extimo majori, manibus dilatato-ciliatis.

It is principally found in old willows.

In a garden near a willow, I found where they got their leaves for their cartridges; which are not willow but rose leaves. I will now proceed no further.

At my coming home I found the long expected cartridges, and some of the bees hatched; so that now we want nothing to complete their history. I will trouble you only with those particulars which are not mentioned in Dr. King's paper, to whom we owe the knowledge of their productions, and whose observations concerning them our own experience has since confirmed. Mr. Snell, an ingenious gentleman, brought some of them to the wells at Astrop, who directing me to the place where he got them, I found plenty in the trunk of a dead willow. Beginning to unfold some of them, Mr. Ray immediately judged them to be made up of pieces of rose leaves, and remembered that in this very spring a friend of his, Mr. Francis Jessop, brought him a rose leaf, out of which he saw a bee bite such a piece, and fly away with it in her mouth. Whereupon searching the rose trees thereabout, we found a great many leaves, with such pieces bitten out of them as these cartridges are made of. The cuniculi or holes never cross the grain of the wood, excepting where the bee comes in, and where they open one into another. From the place of entrance, they are wrought both upwards and downwards; so that sometimes the bee-maggot lies under her food, and sometimes above it. That end of the cartridge which is next the entrance, is always a little concave; the other, which is furthest from the entrance, a little convex, and is received into the concave of the next beyond it. The sides of the cartridges are made up of oblong pieces of leaves, and pasted together; the ends, of round ones: And wherever they do not lie close one to another, the intermediate space is filled up with a multitude of these little round pieces, laid one upon another.

The cartridges contain a pap or batter, of the consistence of a jelly, or something thicker; of a middle colour between syrup of violets and conserve of red roses; of an acid taste, and unpleasant smell. In each of these, at the concave end, there lies one bee-maggot, which feeds upon the before-mentioned matter, till it grows to its full largeness, and then makes and incloses herself in a theca or husk, of a dark red colour, and oval figure; in which she is changed into a bee. The remainder of her food you may find dried into powder at the convex end; and her excrements at the concave without the theca.

The bees I found in your box (which are the only ones I have yet seen) were of a shorter and thicker shape than the common honey-bee, more hairy, &c. But the surest mark to distinguish them is, that the forcipes or teeth of these are larger, broader, and stronger; in shape like those of a wasp or hornet; from which she also sufficiently differs in having a tongue like a bee.

They made their way out along the channel through all the intermediate cartridges, and not through the solid wood. Of the corruption of the matter within the cases, when the bee-maggots or nymphæ happen to miscarry, are bred little hexapods, which produce beetles; and maggots, which produce flies; also mites, &c.

From what has been observed concerning this bee, and by a great many more parallel instances, we may answer the question of some, that have written of bees, whether it be the old bee, or the bee-maggot that covers the cells before the change? And say that it is the maggot and not the old one. For here the old bee, when she has left provision enough with an egg, closes up the cartridge; and has no more to do: The maggot a great while after making the theca, which is analogous to the cover of the cells.

Pl. 13, fig. 2, represents the leaf, out of which a long piece, as fig. 3, and a round piece, as fig. 4, were bitten. Fig. 5 shows the cartridge itself; and fig. 6 the theca.

A Confirmation of what was formerly printed in Number 50 of these Tracts, about the Manner of Spiders projecting their Threads. Communicated by Mr. John Ray. N° 65, p. 2103.

Concerning the manner of spiders projecting their threads, I received the following account from Dr. Hulse, from whom I must acknowledge I had the first notice of this particular, which was after communicated to me by Mr. Martin Lister, whose letter I formerly sent you to be imparted to the Royal Society. Nor is it any great wonder that inquisitive persons, applying themselves to observe and consider the same subjects, should make the same discoveries.

I have (says he) seen them shoot their webs three yards long before they begin to sail; and then they will, as it were, fly away incredibly swift. Which phenomenon somewhat puzzles me, as the air seldom moves a quarter so fast as they seem to fly. In general they project their threads single, without dividing or forking at all to be seen in them: Sometimes they shoot the thread upward, and will mount up with it in a line almost perpendicular; and at other times, they project it parallel to the horizon; as you may often see by their threads that run from one tree to another, and likewise in chambers from one wall to another. I confess this observation at first made me think that they could fly, because I could not conceive how a thread could be drawn so parallel to the horizon between two walls or trees, as above-mentioned, unless the spider flew through the air in a straight line. The way of forking their threads is expressed by fig. 7, pl. 13.

They often fasten their threads in several places to the things they creep over: The manner is, by beating their tails against them as they creep along; which may be understood by the line, fig. 8, pl. 13. By this frequent beating in of their thread among the asperities of the place, where they creep, they either secure it against the wind, that it be not easily blown away; or else, while they hang by it, if one stick breaks another holds fast; so that they do not fall to the ground.

An Account of some Books. N° 65, p. 2105.

I. De Corporum Affectionibus cum Manifestis tum Occultis, Libri Duo: seu Promotæ per Experimenta Philosophiæ Specimen. Auth. J. B. Du Hamel,* Ecclesiæ Bajocensis Cancellario. Parisiis, 1670, in 12mo.

The learned author of this treatise having represented in the preface that the Grecian philosophy concerning nature has been so far from being able to increase, that it is rather esteemed to have degenerated and decayed; gives here a specimen of natural philosophy improved and advanced by observations and experiments; not only endeavouring to explain, from the principles of modern philosophers, the qualities and powers of bodies, but also giving an account of the more notable experiments made in this age in divers places, as England, France, Italy, Germany, &c. In the performance of which he treats,

In his first book, of the origin and nature of qualities in general; then of heat and cold; of fluidity and firmness, and other tactile qualities; of tastes, smells, sounds, light, and colour. In the second, of medicaments in general; then of the virtue and use of preparing alterative, purgative, and topical medicines; as also of poisons and antidotes. In the same, he proceeds to consider magnetism, electricity, gravity in general, and the accelerated motion of heavy bodies: concluding the whole with the consideration of librated liquors, and the weight of the air.

II. Elementa Physica, sive Nova Philosophiæ Principia; ubi Cartesianorum

* John Baptista du Hamel, a French philosopher, was born at Vire, in Lower Normandy, in 1624, and studied at the universities of Caen and Paris. At the age of 19 he printed an explanation of Theodosius's Spherics, to which he added a neat Tract on Trigonometry. He entered among the fathers of the oratory, but after eight years left them for a curacy. This he also resigned after three years. He was the first secretary to the Royal Academy, being chosen in 1666 to that office, which he held till 1697, when at his own request he was succeeded by M. de Fontenelle. He travelled in England and several other countries, cultivating every where the acquaintance of the most eminent philosophers. Du Hamel died at Paris, by the gentle and gradual decays of old age, in 1706, in the 83d year of his age. He was author of a great many works on a variety of subjects, as mathematics, philosophy, (among which should be mentioned his Regiæ Scientiar. Academiæ Histor.) metaphysics, divinity, &c.; and was celebrated for his readiness and accuracy in the use of the Latin language.

Principiorum falsitas ostenditur, ipsiusque errores ac paralogismi ad oculum demonstrantur ac refutantur, à Francisco Wilhelmo Libero Barone de Nuland, &c. Hagæ Comitis, 1669, in 12mo.

Though the author of this small treatise lays down an hypothesis of natural philosophy, yet will he not be understood to be resolved to adhere unchangeably to it, but rather judges it more useful to employ great care and much time in observing the effects of nature; highly commending for that method the two lately founded philosophical academies in England and France, which by observations and experiments, faithfully made, labour to attain the knowledge of truth.

Concerning M. Descartes, though our author professes a high esteem for his ingenuity, yet is he of opinion, that the fondness which that great man had for his system of the world, so blinded him, that he could not see his errors in its contrivance.

III. A Discourse concerning Local Motion, from the French. London, 1670, in 12mo.

This discourse undertakes to demonstrate the laws of motion, and to prove that, of the seven rules delivered by Descartes on this subject, he has mistaken six. In doing which the author particularly insists on considering the communication of motion in percussions; declaring, that, though this subject has been handled by very eminent men, yet he takes it otherwise in hand than they have done, forasmuch as, without making any particular hypothesis, he makes it his business to search into the very sources of nature, the causes of all the effects we find in motions, and undertakes to give the demonstrations of them. He is not ignorant of what has been lately published by some famous mathematicians of the Royal Academies of London and Paris. Neither does he contest with those persons about that which they pretend to, of having found the secret of the laws of motion. He only says, that it is now three years since he published what he delivers in this discourse; and that, his rules being compared with theirs, there may possibly be found conformity enough to make men believe, that he has lighted together with them upon the truth, but that yet there will always be found difference enough to make men judge, that he has not learnt it from them. Besides, says he, they have done no more than merely proposed their rules without proving them; whereas he undertakes to demonstrate all those he advances.

The chief heads explained in this discourse are, that, 1. A body is in itself indifferent to rest or motion. 2. If a body be once at rest, it will ever remain therein. 3. And if it be once in motion, it continues to move always. 4. That rest is not a mere negation; and that there is as much positive action in rest as in motion. 5. The bodies which we move do cease to move because they are impeded. 6. A body successively receiving many determinations remains only affected with the last. 7. A free body cannot be determined to move in a curve line, nor with unequal celerity. 8. Every body that moves about a centre endeavours to recede from it. 9. How a body may be moved circularly. 10. One body moving against another body gives it its whole motion. 11. In the meeting of two bodies there is made a percussion which is mutual, and equally received in both. 12. A moving body meeting another body that is quiescent, gives it all its motion, and remains itself motionless. 13. What is meant by absolute and respective velocity. 14. The percussions are as the respective velocities. 15. Two bodies meeting one another, turn back, making an exchange of their velocity. 16. Two bodies moving toward the same places, continue after their encounter by exchanging their velocities. 17. A hard body striking another body that cannot be shaken is reflected with its whole motion. 18. The angle of reflection is equal to the angle of incidence. 19. It may be imagined that the oblique motion is composed of two motions. 20. A general rule for all percussions. 21. There is always an equal quantity of respective motion. 22. The midst or centre of gravity of two bodies is always uniformly moved in a direct line. 23. All these rules are true, whether the bodies be equal or unequal. 24. A body moves in pleno as freely as in vacuo. 25. The percussions of equal bodies are made in pleno as in vacuo; but when the bodies are unequal, the percussions are made in pleno otherwise than in vacuo. 26. The percussions of unequal bodies cannot be reduced to one general rule. 27. Of refraction. 28. An appendix containing a review of this discourse.

IV. Congietture Physico Astronomiche della Natura del Universo, da Pietro M. Cavina; in Faenza, 1669, in 4to.

These conjectures are raised by the author upon some celestial observations about the fixed stars at Faenza. The whole tract consists of three parts, considerations, observations, and reflections, all of an erroneous and extravagant nature.

V. Dimostratione Fisico Matematica delle sette Propositioni, che promesse Donato Rosetti. In Firenze, 1668, in 4to.

The seven propositions which the author pretends to have here demonstrated are these:

1. What is the true physical cause of equilibriums? 2. The doctrine of Archimedes, importing, that a floating body sinks beneath the level of the water so far, as that a mass of water, equal to the part immersed, does absolutely weigh as much as the whole floating body, is false. 3. It is very probable that there is no æther, and that consequently there is a vast vacuum. 4. There is a

very easy, short, and infallible way, exactly to know how much is the absolute weight of the air that is impendent over any particular place. 5. With little less easiness and brevity, but with the same infallibility, may be weighed any one part of the said air, for example, a cubical foot. 6. The only way of measuring the height of the atmosphere. 7. How it may be experimented, whether light at the distance of 40 or more miles be moved in any observable time?

These propositions have occasioned very warm disputes in Italy, where they were first stated; as may appear by what has been published against this author by Signor Montanari and Signor Finetti, to which we must refer the reader.

Extracts of two Letters, written by Mr. Adam Martindale, from Rotherton in Cheshire, Nov. 12, and Nov. 26, 1670, concerning the Discovery of a Rock of Natural Salt in that Country. N° 66, p. 2015.

In Cheshire there has been lately found a rock of natural salt, from which issues a brine stronger than any of the springs before used in our salt works.

This rock of salt is between 33 and 34 yards distant from the surface of the earth; about 30 whereof are already dug, and they hope to be at the flag, which covers the salt rock, about three weeks hence. I doubt it will be several months before I can accommodate you with a parcel of it; that which the auger brought up being long since disposed of, and the workmen not daring to remove the flag, till the frame be finished and well settled for securing the work from the circumjacent earth. That parcel of natural salt, which the instrument brought up, was as hard as alum, and as pure; and when pulverized, became an excellent, fine, and sharp salt.

An Account given by J. Battista Donius, a Florentine Patrician, concerning a Way of restoring the Salubrity of the Country about Rome. Extracted from the ninth Italian Giornale de Letterati; and translated as follows. N° 66, p. 2017.

The champagne of Rome, (which is that tract of land that is destitute of inhabitants and trees, and extends itself for many miles, taking in Latium, and part of the ancient Sabines and of Tuscany) would be of great use to the state, and of subsistence to the people, if it could be inhabited without that great danger to health, which now it is so much noted and feared for, physicians judging that from the summer solstice to the autumnal equinox, the air of it is so bad and noxious that it would be great rashness to dwell there; a malignity which in former ages it was free from, at least in that of Cicero, who, as appears by his letters, lived there in the summer months, went from one place to another, and returned to Rome, without making mention of any such danger. Wherefore that country not being unwholesome of its own nature, but from adventitious causes, it is probable that those may be removed, as has been proposed by J. Battista Donius in his book De Restituenda Salubritate Agri Romani.

The cause of the extreme unhealthiness of the air in those parts, is ascribed to the noxious vapours from the stagnant waters and marshes. And the chief means recommended for curing the evil are, draining the low lands, encouraging people to settle there, and to till and cultivate the land.

An Experiment concerning the Progress of Artificial Conglaciation, and the remarkable Accidents therein observed by the Florentine Philosophers, and published in their Saggi di Naturali Esperienze. N° 66, p. 2020.

The first vessel we used (say those eminent academists) for this experiment, was a globe of crystal, whose diameter was $\frac{1}{8}$ of a braccio,* with a long straight neck of about a braccio and a half, graduated into small parts. Having filled it with common water up to the sixth part of the neck, we put the globular part into ice and salt, after the usual manner of artificial freezing of liquors, and began very attentively to observe all the motions of the water from its level. It was sufficiently known before, that freezing works in all liquors a contraction; as also that in the passage, which the water makes from being simply cold, to the leaving of its fluidity, and taking a consistency and hardness by congelation, it not only returns to the bulk it had before it was frozen, but swells to a greater; since we see that vessels, not only of glass but of metal, are forcibly broken thereby. But what might be the limits and period of these various alterations, which the cold works therein, we as yet did not know; nor is it possible to attain that knowledge in opaque vessels. Therefore, that we might not want that insight, which appeared to be the soul of all these experiments, we had recourse to crystal and glass, hoping that by the transparency of that body we should be informed of the whole progress; as at every motion which should appear in the water of the neck, we might quickly take the globe out of the ice, and observe the alterations correspondent thereto. But the truth is, that we took more pains than we can express, before we could find out any thing certain touching the periods of these accidents.

In the first immersion of the globe, as soon as it touched the icy water, there was observed in the water of the glass's neck a small rising, but that sufficiently

* Near three English inches.

quick; after which, with a motion regular enough, and of middle velocity, it retired back to the globe, till being come to a certain mark, it descended no further, but stopped there for a while motionless. Afterwards little by little it was seen to begin to rise again, but with a very slow and in appearance even and regular motion; from whence, without any proportioned acceleration, it suddenly and furiously started upwards; in which time it was impossible to follow it with our eyes, as it ran up with this impetuosity, in an instant as it were, through several tens of the marked degrees. And as this violence began in a moment, so in a moment it ended; for as from this very great velocity it suddenly passed to another degree of motion, which though quick enough, was yet incomparably less than the preceding; and going on to rise in this degree, it went to the top of the neck, and at last ran over.

While this was going on, there were at times seen on the top of the water some bubbles more or less, either aërial, or of another more subtle matter. This separation was not noticed till the water had begun to take a brisk cold; as if the force of such a cold had the power of straining such matter, and severing it from the water.

Being desirous to see whether those alterations kept among themselves any kind of analogy, we began to reiterate the conglaciations, and no sooner was one ice destroyed, but we set it to freeze anew: And the water went to congeal again in the same order of alterations; which yet did not every time return to the self-same points or degrees in the neck: Which made us believe that they had no constant and stable period, as reason seemed to persuade us they had. Meantime, in repeating these experiments, having once unawares let the water of the globe freeze near to the neck, the globe burst: Whereupon another being taken, of a less size, in order that the cold might more speedily and more easily get into all the water, and the neck of it being two braccios long, that it might not run out; it was filled with water up to the 160th degree, and then put into the ice. Here observing it as attentively as we could, we found first, that all the accidents of subsiding, rising, resting, starting upwards, running, retarding, always followed in the same points of the neck of the globe, that is, when the surface of the water stood at the same degrees; provided, that in the act of setting it in the ice, care were taken to put it to the very same degree where it was when put into the ice the time before, that is to say, to the same temper of heat and cold: In which case the whole vessel might be considered as a very nice thermometer, by reason of the great capacity of the globe, and the exceeding straightness of the neck. This being provided for, we began to take notice of the precise time of congelation; which to find aright after every little space of time we took up the globe out of the ice; but how frequently soever we made such observations, we never could so hit it as to see even the least vein of frost, but always it was either all fluid or all frozen. Whence we conjectured, that the work of congelation was done in a very short time, and that he who should have the luck to take the globe out of the ice in that nick of time when the water should receive so sudden a change, would certainly find something very notable thereby. And because by the so often taking out and putting the globe into the ice, the whole period of its changes was disordered; we let it return to just the same mark as it was at first, and then placing it into the ice, we fixed it to that degree in which it was wont to take that very impetuous motion, and half a degree before it arrived at it, we took it out. Then looking carefully on the water in the globe, which by reason of the transparency of the crystal was plainly seen to be yet altogether fluid and clear, the water, though now out of the ice, did by the operation of the introduced cold (after it had attained to its due point with a swiftness imperceptible to the eye, the transparency within the globular part being lost, and itself in an instant, as it were, deprived of its motion) totally conglaciate. Which experiment we tried over and over again, and found it always succeed alike.

An Eclipse of the Moon; a Conjunction of Venus and the Moon, &c. By M. Hevelius. Translated from the Latin. N° 66, p. 2023.

Sept. 29, 1670, in the morning, the beginning of the moon's eclipse was about 2 hours 22 minutes; though it could not be very accurately observed, on account of the earth's shadow being very faint; for during the whole eclipse, the shadow was so thin and dilute, that all the principal spots could be seen through it with a telescope of 20 feet, and even a shorter. The greatest obscuration was about 3h. 50m. the end of the eclipse was at 5h. 21m. the whole duration therefore was 2h. 59m. and the quantity of it was scarce more than 9 digits.

According to the Rudolphin tables.	h.	m.	sec.	According to Riccioli's tables.	h.	m.	sec.
The beginning	2	37	5	The beginning	2	14	47
Greatest obscuration	4	2	50	Greatest obscuration	3	55	37
The end	5	28	35	The end	5	36	27
Whole duration	2	51	30	Whole duration	3	21	40
Quantity of digits	9	4	0	Quantity of digits	11	43	0

About the middle of this eclipse, at 3h. 40m. I clearly observed an unknown telescopic star, covered by the moon near the lacus niger major. After the eclipse was over, it was a pretty agreeable sight to observe both luminaries at once above the horizon, for the sun rose before the moon was set.

Oct. 11, N. S. about eight o'clock in the morning, after sun-rising, I also observed at Dantzic, a conjunction of Venus and the moon. According to the Rudolphin tables, there must have been an occultation of at least 23″ by the inferior limb of the moon. I had furnished myself with a 20-foot telescope, and though the air was not entirely clear, there being here and there a few clouds, yet Venus and the moon were clearly observed. But there happened then no occultation, Venus being at the distance of three or four minutes from the inferior limb of the moon, but only a close transit. This was a very agreeable sight in the day-time, and in sun-shine, for the moon was very small and slender, hastening to her conjunction, and her horns were very sharp; and Venus was almost full, with her body much diminished: on the same day, at 7 h. 40 m. three parhelia appeared.

The new star, under the head of Cygnus, which at first appeared of the third magnitude, was in a surprising manner diminished in the month of September, so that on the 14th of October it could be no longer observed. The other new star in the neck of Cetus was almost, till the middle of October, equal to the star in the jaw, and nearly exceeding it in brightness and magnitude; so that this year it was of the second magnitude, and larger than it was the preceding years, excepting that in 1660, it exceeded the star in the jaw; at other times, I did not find it surpass a star of the third magnitude. It is therefore certain, that it is not always of the same magnitude and brightness when largest; it was lately much diminished.

An Account of three Books. N° 66, p. 2034.

I. Origo Formarum et Qualitatum; juxta Philosophiam Corpuscularem Considerationibus et Experimentis Illustrata, à Roberto Boyle, Nobili Anglo, è Societ. Regia. Oxon. 1669, in 12mo.

Having formerly, (Number 11) given the contents of this instructive treatise, we again take notice of it here, only to let foreigners know, that the same is now printed in the Latin tongue, after it had been public in the English since the year 1666.

II. Metallographia, or an History of Metals; by John Webster, practitioner in physic and surgery. London, 1670, in 4to.

The author of this history has, with much diligence and curiosity, here delivered the signs of ores and minerals, both before and after digging; the causes and manner of their generation; their kinds and differences, and a description of sundry new metals, or semi-metals, as he calls them; together with a discourse on their vegetability, and the discussion of many difficult questions belonging to chemistry. Collected out of the most approved authors, that have written in Greek, Latin, High Dutch and English: with some observations and discoveries of his own.

III. Lettera di Francesco Redi sopra alcune Oppositioni alle sue Observationi intorno alle Vipere. In Firenze, 1670, in 4to.

Mons. Charas having endeavoured in his book, entitled, Nouvelles Experiences sur la Vipere, not long since translated into English (of which an account was given in Number 54;) first, by experiments to confute the opinion, that the venom of vipers resides in the yellow liquor contained in the bag about the vipers' teeth, and then to maintain on the contrary that it consists in their vexed and enraged spirits; this author being concerned in this controversy, and having made out the former of these opinions, as he thinks, by numerous experiments, affirms now in this letter, that he repeated his former trials again and again, and is thereby altogether confirmed in what he then delivered about the seat of the vipers' poison; concluding with more assurance than ever, that the venom of the Italian vipers does not consist in an imaginary idea of revengeful choler, but in that yellow liquor above-mentioned; which, he says, if it chance to shed or spread itself over the mouth and palate of the viper, may make that saliva poisonous which moistens the throat of that animal. On which account he proposes that the authors of the French experiments should make new observations; which might prove, if conformable to the opinion already published, that the venom of French vipers consists in a choleric and vindicative idea of the fancy; but that of the Italian has its seat in that yellow juice so often spoken of: and that, if this latter prove true, it will be no untruth to affirm, that, if a viper by biting should have consumed both all the provision, which is lodged in the bags of her teeth, and that also, which from the neighbouring parts may be furnished, that then her bitings would not be mortal: which he says he has heretofore affirmed, and doth so still; although M. Charas deny it, declaring that one only viper being enraged is able to kill as many animals as she shall bite.

In this vindication our author alleges many new experiments to maintain his opinion; and also some very remarkable observations, to show that vipers kill some animals more easily than others; that their biting in some places is more dangerous than in others; and that a plentiful effusion of blood after the biting is capable of saving the bitten animal, &c.

Among the many new experiments, he relates one which is not to be passed by; viz. That he had put up in a glass vessel the poisonous liquor taken out of two hundred and fifty vipers, with a design to make various trials as often as he should have leisure; but that being diverted by many avocations from accomplishing his purpose, it happened that that juice turned to a kind of glue, of the colour of amber, and after the lapse of thirty days, it became dry and friable, and easily reducible to powder, and that having pulverized it, out of a desire to see whether this powder, being put into wounds, made by him in pullets and pigeons, kept the same force of poisoning; he found, that in a little time all the pullets and pigeons into whose wounds he had put a quantity of this powder died.

An Account of several Experiments made and communicated by Dr. Erasmus Bartholin, on a Crystal-like Body, sent to him from Iceland. N° 67, p. 2039.

The inhabitants of Iceland and our own merchants inform us, that this kind of crystal is found in divers places of that conntry; but chiefly dug out of a very high mountain, not far from the bay of Roerfiord, which lies in 65 deg. latitude. That the mountain has its whole outside made up of this substance, without a necessity of digging deep for it. That it is cut out by iron tools, in the size of a cubic foot, or more; and that out of its corners there is sometimes found grown a harder matter capable of cutting glass, of a figure different from that of the whole mass, and approaching to that of diamonds.

The figure of this crystal stone is like a rhomboid prism. Nor has the whole body that form only, but even the parts of it when broken into small pieces; except that in some cases the ground, whence it is dug, yields such as are of a triangular pyramidal figure.

This substance is electrical, attracting when heated, straw, feathers, &c.

It is not so hard as to endure polishing: nor is it easily consumed; nor reduced into a calx but by a strong fire, by which it will turn into a substance like unslacked lime, which will heat a wet finger, and when sprinkled with water, will bubble up and become like common lime.

Aquafortis being dropped on it, it was corroded, and the superficial parts were put into motion with some noise. When I pulverized it in a mortar, aquafortis poured on it, made it boil till all was dissolved, and the menstruum tinged of a yellowish colour. Then putting it into a thermometer furnished with a hollow glass ball, it considerably showed the difference of heat and cold. The powder being dissolved in aquafortis, I dropped some spirit of vitriol upon it, to separate the thick from the thin, and to precipitate the white calx to the bottom.

The sides of this body are very smooth; which is then easily obtained, if a thinner piece be nimbly broken asunder with your finger. But if you strike it with a hammer, the percussion has not the same effect, nor is equally resisted from every part; whence the smooth sides of this mineral become often scabrous. The whole body is rather clear than bright, of the colour of limpid water; but that colour, when it has been immersed in water and dried again, becomes dull. Hence it is, that in its native place the upper surface is darkish; because of the rains and snows fallen upon it. Sometimes there appear also some reflections of colours, as in the rainbow. The angles are not pointed alike, all the flat sides being obliquely inclined to one another. The opposite plains are parallel.

In this crystalline prism, two of the plain angles are always acute, and the two other obtuse; and never any of them is equal to the collateral angles of the inclinations.

The objects seen through it, appear sometimes, and in certain positions of the prism, double: where it is to be noted, that the distance between the two images is greater or less, according to the different size of the prism, so that in thinner pieces this difference of the double image almost vanishes.

The object appearing double, both images appear with a fainter colour, and sometimes one part of the same species is obscurer than the other.

To an attentive eye, one of these images will appear higher than the other.

In a certain position the image of an object, seen through this body, appears but single, as through any other transparent body.

We have also found a position, wherein the object appears sixfold.

If any of the obtuse angles of this prism be divided into two equal parts by a line, and the visual rays pass from the eye to the object through that line, or its parallel, both images will meet in that line, or in another parallel to it.

Whereas objects, seen through diaphanous bodies, are wont to remain constantly in the same place, in what manner soever the transparent body be moved, nor the image on the surface move except the object be moved; we have observed here, that one of the images is moveable, the other remaining fixed; although there be a way also to make the fixed image moveable, and the moveable fixed in the same crystal; and another, to make both moveable.

The moveable image does not move at random, but always about the fixed, which while it turns about, it never describes a perfect circle, but in one case.

Dioptrics teach, that a diaphanous body, having one only surface, sends from one object but one image refracted to the eye; and having more surfaces than one, it represents one image in each; but whereas in our substance there occurs but one plain superficies to the eye, and yet a double image of one object; it concerned us to consider, whence this double image might be caused. Two ways offered themselves to us, reflection and refraction. How reflection could perform it, was difficult to find. For, having dulled the clearness of the two plain sides of our crystalline prism, to make them unfit for reflecting the light; the rays being directed through its upper and lowermost superficies, the image still appeared double. Again, two species appearing through a great prism, upon breaking the same into pieces, and so reducing it into divers smaller ones, through each of these lesser portions the same object was seen always double. Whence I collected, that if it should be said, that one of the images proceeded from the reflection of the plain sides, the former of these experiments would discountenance that assertion. But then if another should derive the cause from some internal reflection of the surfaces of this body, certainly the same effect would not have been found in every one of its parts, but the double appearance, that was exhibited in the smallest portion, would have been multiplied in a greater bulk.

Reflection therefore not satisfying, we recurred to refraction. But as it is known, that no image can pass through two diaphanous bodies of a different nature, but by refraction, and that one image supposes one refraction; it follows, that if refraction were made the cause of this phenomenon, there would be a double refraction for a double image. And forasmuch as the appearances of our Iceland crystal are not of the same kind, but one of them is fixed, the other moving, we shall also distinguish the refractions themselves, which refract the double rays arriving to the eye, and call the one, which sends the fixed image refracted to our sight, usual; the other, which transmits the moveable to the eye, unusual. And hence, namely, from this peculiar and notable property of the double refraction in this Iceland stone, we have not scrupled to call it disdiaclastic.

This being supposed, it will not be irrational to suspect, that these two refractions proceed from different principles. For, since it is commonly known from dioptrics, that an object, by visual rays affecting the eye, exhibits some image on the superficies of the diaphanous body, which image is single, as long as the superficies is so, and the upper plain parallel to the lower; as also, that if the eye remaining steady, the diaphanous body be moved, that image remains alway fixed, as long as the object, whence it comes, remains unmoved. Wherefore in this transparent substance, the image which appears fixed may proceed according to the ordinary laws of usual refraction; but that which moves, and is carried about according to the motion of the diaphanous body, while the object remains fixed, shows an unusual kind of refraction, hitherto unobserved by dioptricians.

Hence, that I might examine the nature and difference of both, I put upon some object, as the point A, the prism of double refracting crystal NPRQTBS, (fig. 9, pl. 13,) and the eye M, being perpendicularly posited over the upper plain of the prism, I noted whether there was any refraction of the point A, for the usual laws of refraction teach that there is none. But the perpendicular ray of the eye was observed to pass not through the moveable but the fixed image; thereby being conformable to the rules of usual refraction, as striking the eye unrefracted, so that the eye, the image, and the object, were seen in the same line. But when in the same situation of the eye, the object A did also exhibit the other image X, at no small distance from the former; I took notice, that this object A was not seen unrefracted by the means of the image X, though the eye M remained perpendicular over the plain; and that consequently this unusual refraction is not subject to the received axiom of dioptrics, which imports, that a ray falling perpendicularly on the superficies of a diaphanous body, is not refracted, but passes unrefracted.

Next, I so placed the eye in O, that the ray from the object A, arriving to the eye, might be parallel to the lines RT and QB, of the plane RQTB, &c. then it appeared that the rays were trajected from the object A, without refraction, through the moveable image Q; the object A, the moveable image Z, and the eye O, being in the same line; and that the same object A did transmit to the eye O, remaining in the same position, yet another species Y, through the refracted ray AYO. Whence it was manifest, that this unusual refraction had for its rule the parallel of the sides of this double refracting crystal, while the usual refraction was directed according to the perpendicular of the superficies.

But since the place of the point, appearing through this diaphanous body, cannot easily be determined, as being only obvious in the uppermost part; we shall add the way whereby we have found its diversity, by drawing on the subjacent table, a straight line through that point; the place of which line will be determined by the one eye through this crystal, and by the other eye without the crystal. For, in the same figure, through the object A, let be drawn on the table a straight line BC. The eye being in M, that double line HD and IE will appear, the species being cast on the upper surface: And if you attend well, you will observe one of the images, viz. the fixed HD, to be congruent to the subjacent line BC, while the other, namely, the moveable EI, tends towards R. But if afterwards the eye be posited in O, the same object BC, will not only be represented double by the images KF and LG; but also the moveable image GL be congruent to the inferior line BC; while the fixed FK is not so, but tends towards N.

After the author has given us these experiments, he undertakes to determine the quantity of the refractions in this double refracting body; and he finds, after several trials, the angle of inclination to the angle of refraction in this Iceland crystal, to be as 5 to 3.

*An Extract of a Letter from a French Gentleman, concerning a Way of making Sea-water sweet.** N° 67, p. 2048.

Mons. Hauton's secret of making sea water sweet, consists first in a precipitation, made with oil of tartar, which he knows to draw with small charges.

* As a knowledge of the means whereby fresh or sweet water may be procured from salt water is of the utmost importance to seafaring men, we shall here offer some further remarks on this subject.

Sweet water may be obtained from salt water by two methods, by freezing such water or by distilling it.

When sea water is exposed to a degree of cold somewhat below the point at which fresh water freezes, its power of holding muriate of soda and other saline substances in solution, is in part destroyed; ice is formed on the upper surface, while the fluid portion underneath becomes a concentrated brine. This ice when melted yields a water, which contains so little saline matter as scarcely to be distinguished from fresh water by the taste, or indeed by chemical tests. It is evident, however, that this method can only be resorted to in certain latitudes or at certain seasons of the year.

The other method, therefore, viz. that of distillation, is greatly to be preferred, being feasible (with a proper apparatus) at all times and in all situations, and, when properly conducted, yielding a water as pure and as sweet as that procured by congelation. It was formerly supposed that in order to obtain fresh water from sea water it was necessary to add to this last, before the distillation, calcareous earth, potash, or certain other substances, for the purpose of absorbing and retaining a bituminous matter, which all sea water was supposed to contain in greater or less quantity, and to which was ascribed the unpleasant empyreumatic taste of the water distilled from it, especially if too strong a fire is employed, or the distillation is pushed too far. Dr. James Lind, however, has proved that such additions are useless, since pure rain water contracts in like manner a burnt taste by distillation; which shows that it is derived from the action of the elementary water on the heated metallic vessels. This disagreeable flavour, however, goes off, for the most part, on exposing the distilled water to the air. Nothing more is requisite, then, for obtaining fresh water from salt water, than to be provided with a common still; or with still-head covers made to fit the coppers used for boiling provisions on board of ship; and a worm-tub or cooler for condensing the steam. (See Dr. Lind's Essay on preserving the health of Seamen. Also, the Appendix to his Essay on Diseases incidental to Europeans in Hot Climates.) Some years after this discovery was made known by Dr. Lind, [It would appear however that the simple distillation of sea water, for the purpose of procuring fresh water, was practised by Sir Richard Hawkins, in the reign of Queen Elizabeth. See the Bishop of Llandaff's (Dr. Watson's) Chemical Essays, vol. ii.] an improvement was suggested by Dr. Irving, in the mode of distillation; wherein he substituted, for the condensation of the steam, a large open pipe kept constantly wet with mops, in place of the small slender pipe passed through a tub of cold water, in the usual way. This, from being applied to larger coppers than the common method ever had been in the distillation of sea water, yielded in a given time, and with the same quantity of fuel, a larger quantity of fresh water.

Next he distils the sea water; in which work the furnace takes up little room, and is so made, that with a very little wood or coal he can distil 24 pots of water in a day: For the cooling of which, he has this new invention, that instead of making the worm pass through a vessel full of water, as is the ordinary practice, he makes it pass through one hole, made on purpose out of the ship, and to enter in again through another: So that the water of the sea performs the cooling part: By which means he saves the room, which the common refrigerium would take up; as also the labour of changing the water, when the worm has heated it. He then adds filtration, thereby perfectly to correct the malignity of the water. This filtration is made by means of a peculiar earth, which he mixes and stirs with the distilled water, and at length suffers to settle at the bottom. Paris, Feb. 22, 1670.

An Extract of another Letter written by the same, concerning two Experiments made for finding another Passage of the Urine, besides the known one. N° 67, p. 2049.

Having been not long since employed in searching after the passage of the urine* in animals, I made for that purpose a dog drink a quantity of water, and thereupon caused his ureters to be well tied about, and emptied his bladder. After two hours I found the bladder empty, and the ureters were not tumid above the ligature. Surprised at that, I believed that the cause might consist in the too much cooling of the inward parts, that had all this while been exposed to the air; the section having been made crosswise in the usual manner. To avoid this inconvenience, I repeated the experiment on another dog, by causing a small opening to be made on each side, sufficient to find and to tie the ureters, and to squeeze the urine out of the bladder, by pressing it with

Whether the distillation be made after Dr. Lind's method, or with the more simple contrivance of Dr. Irving, the operator should be careful not to continue the process too long, but to stop when three-fourths or only two-thirds of the water shall have been distilled; as the water which is obtained afterwards is less pure, and the brine sometimes becomes so strong as to corrode the copper-boiler. [It appears from the Bishop of Llandaff's experiments (see his Chemical Essays before quoted) that the water distilled from salt water is not wholly free from saline particles; but that it probably contains them in so small a proportion as not to injure its salubrity in any sensible degree.] When too much fire is employed it is possible, especially towards the end of the operation, that some muriatic acid may be disengaged, the action of which upon the metallic vessels it must be desirable to prevent. This might perhaps be effected by adding some potash to the sea water, before the distillation is begun. This important subject has at various times occupied the attention of many ingenious persons, (several of them members of the Royal Society) as will be seen when we proceed to the 17th, 48th, 50th, and 62d. vols. of these Transactions.

* See N° 65, p. 526.

the hand. This done, I had these openings sowed up again; and then having made the dog drink plenty of water, I left him for near three hours in the least violent posture that his ligatures would permit. Afterwards having opened both the holes, and the bladder being pressed with the hand, there issued out of it a quantity of urine, and the ureters seemed to be a little swelled above the ligature. This operation was made with great exactness; but yet as it is of importance for discovering the way of the urine, I would have repeated it often, if I had not been obliged hastily to come away from the place where I then was, and I am not at present at leisure to try it again.

Of the abundance of Wood, found under Ground in Lincolnshire. Communicated by a Friend, well acquainted with that Country. N° 67, p. 2050.

That fenny tract, called the Isle of Axholme, lying part in Lincolnshire and part in Yorkshire, and extending a considerable way, has anciently been a woody country, witness the abundance of oak, fir, and other trees, of late frequently found in the moor, whereof some oaks are five yards in compass and sixteen yards long; others smaller and longer, with good quantities of acorns near them, lying somewhat above three feet in depth, and near their roots, which do still stand as they grew, viz. in firm earth below the moor. The firs lie a foot or 18 inches deeper, more in number than the oak, and many of them thirty yards long; one of them being, not many years since, taken up of 36 yards long besides the top; lying also near the root, which stood likewise as it grew, having been burnt and not cut down as the oak had been also. The number of these trees is reported by Mr. Dugdale, in his book on draining the Fens in England, to be so great, that the inhabitants have, for divers years last past, taken up many cart loads in a year.

Of the original overflowing of this woody level no account is given, that I know of. Even Mr. Dugdale only says, that the depth of the moor evinces that it has been so for divers hundreds of years, since that could not grow to the thickness it is of, in a few ages. The cause thereof he concludes to have been the muddiness of the constant tides, which, flowing up the Humber into the Trent, left in time so much filth, as to obstruct the currents of the Idle, Done, and other rivers, which thence flowed back, and overwhelmed that flat country.

Of the Stone Quarry near Maestricht. Communicated by a Person acquainted with that Country. N° 67, p. 2051.

There is an excellent quarry, within cannon shot of Maestricht, on the very brink of the river Maese, lying in a hill, where there are about 25 fathoms of rock and earth over head; the length of the hill being of some miles, extending along the river towards Liege; and near Maestricht having in breadth some half or three quarters of a mile, but more farther off. This quarry has one entry towards the river, where carts can pass with great ease, and unload the stones on the brink of the river, the quarry within lying parallel to the horizon or level, and elevated but very little above the river.

This quarry, which has almost undermined the whole hill, affords one of the most surprizing prospects, when lighted with many torches, that one can imagine. For there are thousands of square pillars in large level walks, and those almost every where above 20, and in some places many more feet high, and all wrought with so much neatness and regularity, that one would think it had been made rather with curious workmanship for an under-ground palace, than that those pillars and galleries were made by quarriers, that did it only for getting stone to build above ground.

This quarry serves the people, that live thereabout, for a kind of impregnable retreat, when armies march that way. For being acquainted with all the ways in it, they carry into it whatsoever they would have safe, as well their horses and cattle, as their moveable furniture, till the danger be over; there being so much room, that 40,000 people may shelter themselves in it. And he that would attempt to seek them out in this vast wilderness of walks and pillars, without an expert guide, would not only be in hazard of losing his way, but of being knocked on the head at the corner of every pillar, where people lurking in the dark with their carabines and fowling pieces, would have fair opportunity to shoot them by the light of their own torches.

An Account of some Books. N° 67, p. 2052.

I. Tracts written by the Honourable Robert Boyle, of a Discovery of the admirable Rarefaction of the Air, even without Heat: New Observations about the Duration of the Spring of the Air: New Experiments touching the Condensation of the Air by mere Cold; and its Compression without mechanical Engines: And the admirably differing Extension of the same Quantity of Air rarefied and compressed. London, 1670, in 4to.

The main drift of these excellent tracts is, to invite the curious to observe the stupendous mutability of the air, as to rarity and density, whereby the same quantity of air, being sometimes compressed, sometimes dilated, may change its dimensions to a degree that seems almost to transcend the power of nature and art, and might be thought incredible, if it were abruptly proposed, or by a person of only common skill in these matters.

It will then appear, by the experiments and calculation made by our noble author, that according to the least estimate of any recited in them, the extension of the same quantity of air, is as 1 to 2744: and if, instead of the least there be taken the greatest expansion of the air, being as about 13000 to 1, when the uncompressed air was highly rarefied, that number being multiplied by 40, because of the greatest compression of the air effected by cold, will amount to 520000, for the number of times by which the air at one time exceeds the same portion of air at another time.

II. Elementa Geometriæ Planæ. Authore Ægidio Francisco de Gottignies Bruxellensi, Soc. Jesu in Collegio Romano Matheseos Professore. Romæ, 1669, in 12mo.

One of the numerous deviations that have been made from the method of Euclid, in the form of the principles of geometry.

III. Synopsis Geometrica; cum Tribus Opusculis, De Linea Sinuum et Cycloide; De Maximis et Minimis, Centuria; Et Synopsis Geometriæ Planæ. Auth. Honor. Fabry,* S. Jesu. Lugduni Galliarum, 1669, in 12mo.

The author, in this Geometrical Synopsis, has endeavoured to render geometry clearer and easier, by delivering such demonstrations as prove the thing in hand by direct and intrinsic principles, not such as are indirect, and leading *ad absurdum et impossibile*, whereby it is only concluded, that the thing cannot be false, but not shown why it is and must be true.

IV. Dialogi Physici; quorum Primus de Lumine; Secundus et Tertius De Vi Percussionis et Motu; Quartus de Humoris Elevatione per Canaliculum; Quintus et Sextus de Variis Selectis. Auth. Honor. Fabry. S. Jesu. Lugduni Galliarum, 1669, in 8vo.

This learned Jesuit in those dialogues writes against Grimaldi, Borelli, and Montanari, who in several things differ from what he has written: Against the first, concerning light, and that great controverted point, Whether it be a body? And whether reflection and refraction prove it to be such? &c. Against the second, about motion and percussion; where many things are discussed; as, Whether motion be produced, or traduced? Whether the impetus of the

* Honoré Fabri, a learned Jesuit, was born at Bellay, near Lyons, in 1607. He wrote many elaborate works on theology and philosophy, beside the above article. It has even been said that he discovered the circulation of the blood before Harvey; but there are proofs to the contrary.

least body may move the greatest body? Whether the force of percussion be a certain action of compression in the impellent body? What are the laws of two projected bodies; equal or unequal; of equal or unequal velocity? Whether the times of the vibrations of different pendulums are in a subduplicate proportion of their lengths? Whether a body impelled, being reflected by a springy and compressed body, restoring itself, be moved and carried back with that impetus only which it received from the same? Whether a body horizontally projected will at the same time come to the ground, as if it had of itself fallen down perpendicularly? Why bricks are broken by the percussions of a hammer, though they remain whole under the weight of a vast and bulky body? And why a hatchet cleaves wood, whereas a very heavy weight laid on a hatchet fixed in wood, does not? What is the principle of the motion of restitution? and many more. Against the third he writes, about the ascent of liquors in slender tubes; to wit, whether that rising proceeds from the gravitation of the air, or from its compression only, prescinded from its weight? Whether liquors do rise equally high, in longer and shorter tubes, but of equal bore? &c.

V. Antonii Molinetti, Phil. et Med. Veneti, &c. Dissertationes Anatomicæ et Pathologicæ de Sensibus et eorum Organis. Patavii, 1669, in 4to.

Upon the subjects treated of in this work there is nothing sufficiently interesting to be extracted.

Extract of a Letter from Mr. John Ray, *Jan.* 13, 1670, *concerning some uncommon Observations and Experiments made with an Acid Juice found in Ants.** *N° 68, p.* 2063.

What I now send you concerning the juice of ants, I received not long since from Dr. Hulse and Mr. Samuel Fisher. The observations by Mr. Hulse

* From the experiments of Neumann and others, it appears that the acid of ants has a great analogy with that of vinegar, though in some respects it is different. It dissolves with great effervescence coral, chalk, and quicklime, and concretes with them all into crystals, which do not deliquate in the air. It does not precipitate silver, lead, or mercury, from the nitrous acid, nor quicklime from the marine; hence it appears to have no analogy with the marine or vitriolic acids, the first of which constantly precipitates the metallic solutions, and the other the earthy. It does not act upon filings of silver, but, like vegetable acids, it totally dissolves, by the assistance of heat, the calx of silver precipitated from aquafortis by salt of tartar. It does not dissolve calces of mercury, as vegetable acids do, but revives them into running quicksilver. It acts very weakly on filings of copper, but perfectly dissolves copper that has been calcined; the solution yields beautiful compact green crystals. It dissolves iron filings with violence; the solution, duly evaporated, shoots into crystals more readily than that made in distilled vinegar. It scarcely acts at all upon filings of tin. It does not, according to Margraaf, corrode filings of lead, but dissolves, by the assistance of heat, the red calx of lead. It

are these. Lately (says he) consulting Langham's Garden of Health, I met with this passage: Cast the flowers of cichory among a heap of ants, and they will soon become as red as blood. Note, That Langham was not the first that made or published this observation: I find it delivered by Hieronymus Tragus, Hist. Stirp. l. 1. c. 91. Naturæ miraculum in hoc flore observare licet; siquidem cumulo Formicarum abditus, cœruleum colorem in rubrum mutat, ac si terrore illarum erubesceret. And before him it was taken notice of by Otho Brunsfelsius, as Johannes Bauhinus observes. I presently got some cichory flowers, and made the experiment, and find it to be true; only he takes no notice of the manner how the flowers become stained: which therefore I now send you. Bare an ant-hill with a stick, and then cast the flowers upon it, and you shall see the ants creep very thick over them. Now as they creep they let fall a drop of liquor from them, and where that falls it leaves a large red stain. Sometimes they will not discolour them immediately, and at other times they will do it in an instant. At first I guessed that being vexed by stirring their hill, they might thrust their stings into the flowers, and through them convey that acid liquor: But by bruising them, and rubbing the expressed juice against the flowers, I find they will be equally stained. It is a thing well known that ants, if they get into people's clothes, and so to their skin, will cause a smart and tingling, as if they were nettled; which I conceive is done by letting fall the before-mentioned corrosive liquor rather than by stinging.

To what sort of liquor to refer this juice, I know not. I dropped spirit of salt and oil of sulphur on the flowers, but they did not cause them to change colour, at least not till the flowers were bruised. I likewise put salt of tartar on them, and dropped thereon a little spirit of salt, which caused a sufficient fermentation, but prevailed not to change the colour of the flowers in the least.

This observation holds true not only in cichory flowers, but I suppose all others of a blue colour. So far the Doctor.

dissolves zinc with vehemence, and shoots, upon duly evaporating the solution, into inelegant crystals, not at all like those produced with distilled vinegar. It seems to have little effect on bismuth, or on regulus of antimony, either in their metallic form, or when reduced into calces.

Neumann's Chemical Works, by Lewis, v. 2, p. 328. N.

Mr. Ray's correspondent appears to have been the first who proved the existence of an acid liquor in ants, a fact which was afterwards confirmed by various chemists, among whom Neumann and Margraaf deserve particularly to be mentioned. A new and extensive set of experiments on this subject was undertaken in 1777 by Mr. Arvidson, who, in his Dissertation de Acido Formicarum, published that year at Upsal, endeavoured to show that these insects contain a specific animal acid, which was accordingly denominated the formicine, and afterwards the *formic acid.* From recent analysis, however, it would appear that this acid (as indeed several chemists had long suspected) is not a specific animal acid, but an acid agreeing in its properties with some of those which are obtained from vegetable substances; that it is, in fact, a compound of the acetic and malic acids.

On reading these passages, I called to mind an experiment, which some years since Mr. Samuel Fisher of Sheffield had made me acquainted with, viz. If you stir a heap of ants so as to rouse them, they will let fall on the instrument you use a liquor, which if you presently smell to, will twinge the nose like newly distilled spirit of vitriol. Considering this, and likewise that a few drops of the oil or spirit of vitriol will soon turn the bluish syrup of violets into a bright red; and, as I am credibly informed, the juices and tinctures of any other flowers or fruits of that or the like colour, I was easily induced to think, that this juice of ants might be of the same nature with the oil of vitriol and other acid spirits. And thereupon I inquired of Mr. Fisher, what trials he had made of it, who returned me the following account.

A weak spirit of ants will turn borage flowers red in an instant: Vinegar a little heated will do the like. Ants distilled by themselves, or with water, yield a spirit like spirit of vinegar, or rather like the spirit of viride æris. Lead put into this spirit, or fair water, with the animals themselves being alive, makes a good saccharum saturni: Iron put into the spirit, affords an astringent tincture, and by repetition a crocus martis. Take saccharum saturni thus made, and distil it, and it will afford the same acid spirit again, which the saccharum saturni made with vinegar will not do, but returns an inflammable oil with water, and nothing that is acid. Saccharum saturni made with viride æris, does the same with that made with spirit of ants.

When you put the animals into water, you must stir them to make them angry, and then they will spirt out their acid juice. No animal that we ever distilled, except this, yields an acid spirit, but constantly an urinous; and yet we have distilled many, both flesh, fish, and insects.

Extract of a Letter from Mr. MARTIN LISTER,* *Jan.* 25, 1670-71, *relating partly to the same Argument with that of the former Letter, and alluding to another Insect likely to yield an Acid Liquor; and partly to the Bleeding of the Sycamore. N° 68, p.* 2067.

Concerning the acid liquor of ants, I have very lately received from Mr. Ray the account sent him from Mr. Fisher and Mr. Jessop; wherein these two

* Martin Lister, an ingenious and learned physician, and one of the best naturalists of the 17th century, was born in Buckinghamshire in 1638, and is said to have been educated under his great uncle Sir Martin Lister, Knt. physician in ordinary to Charles the First, and president of the College of Physicians. He was afterwards sent to St. John's College, Cambridge, where he took his first degree in arts in 1658. He proceeded to the degree of M. A. in 1662, and applying himself closely to physic, travelled into France in 1668, to improve himself farther in that faculty. Returning home,

gentlemen make this further inquiry, whether there be any other insect, or animal, flesh or fish, that will afford an acid juice; as they have with great industry tried many species amongst insects, and other animals, without discovering the like acid liquor. I am of opinion there are; and a ready way to find such out, may be, that having observed that an ant bruised and smelt to emits a strange fiery and piercing odour, like the leaf of the herb called flammula, broken at one's nostrils; by this means I have, since Mr. Ray put the question to me, found an insect which I suspect may yield an acid liquor as well as the ant; and that is the long and round bodied red coloured julus,* distinguished from all other multipeds, as their innumerable legs are as small as hair, and white, and in going they move like waves; not rare among dry rubbish; no scolopendra, it being a harmless insect, and that armed with dangerous forcipes. The body of this julus being bruised strikes the nostrils pungently; but I have not yet had an opportunity to furnish myself with any quantity of them for further trials.

As to the bleeding of the sycamore; the last year I wintered at Nottingham, where I pierced a sycamore about the beginning of November; the turgescence of the buds inviting me thereto, and some hopes of improving the notion of winter bleedings, so happily discovered by Mr. Willoughby and Mr. Ray. This succeeded so well with me, that I afterwards engaged myself in keeping a journal throughout the whole winter; from which I may note; 1. That the wounded

he settled in 1670 at York, where he followed his profession many years with much reputation, and at the same time took all opportunities of prosecuting his researches into various branches of natural history, the study of antiquities, &c. He was a benefactor to the Ashmolean Museum at Oxford, which he enriched with several altars, coins, &c. &c. He became a fellow of the Royal Society, and in 1684, removing to London, was created Doctor of Physic by diploma at Oxford, by the recommendation of the chancellor himself. Soon after this he was elected fellow of the College of Physicians. In 1698, he attended the Earl of Portland in his embassy from King William to the court of France. On his return he published his *Journey to Paris,* a work of sterling merit, though ridiculed by Dr. King, in a paper entitled *A Journey to London*. In 1709 he was appointed physician in ordinary to Queen Anne, in which post he continued to his death, which happened in 1711-12. Lister was the author of several very ingenious and learned works, viz. 1. *Historiæ Animalium Angliæ tres tractatus, &c.* 1678. 2. *Goedartii Historia Insectorum cum notis,* 1682. 3. *De Fontibus medicinalibus Angliæ,* 1682. 4. *Exercitatio Anatomica, in qua de Cochleis agitur,* 1694. 5. *Cochlearum et Limacum exercitatio Anatomica,* 1695. 6. *Conchyliorum bivalvium utriusque aquæ exercitatio Anatomica tertia,* 1696. 7. *Exercitationes Medicinales, &c.* 1697.

The most celebrated production however of Dr. Lister is his *Synopsis Conchyliorum,* which, though merely consisting of plates with their inscriptions, may be considered as by far the best work on the subject at the period of its publication, containing almost all the then known shells, accurately represented, and not injudiciously arranged. The plates were engraved by his two daughters Susanna and Anna Lister. In the different copies of this work are observed some occasional variations.

* Julus terrestris. Linn.

sycamores never bled, either in November, December, January, February, or March, (which yet they did above 40 several times, that is, totally ceasing and then beginning anew,) unless there preceded a sensible and visible frost; for I had no other way of recording the temper of the air. 2. That the frost did not always set a bleeding the wounds, which were made before it came, though sometimes it did; but upon its breaking up, or very much relaxing the wounds either made in that instant of time, or many months before, never failed to bleed more or less. 3. That particularly on the breaking up of the two great and long frosts (the first of which happened on the 3d of January, the second about the 12th, 13th and 14th of February,) all the wounds ran most plentifully: so that such times may be considered as the most proper season for gathering great quantities of juice from this tree. Removing into Craven the latter end of March, and thence to London, my journal was discontinued; I had yet, upon my return from London to Craven, some leisure to prosecute it. Those which I wounded the latter end of May did not bleed, either in the remaining part of that month, or the following months, June, July, but had the orifice of the wounds, made with a small augur, quite grown up, so as scarcely to admit a pigeon's feather. Wherefore on the 30th of July I cut out a square piece of about two inches of the bark of a large and well grown sycamore, about my height in the body of it: This wound began to run the next morning about nine o'clock, so as to drop, and that was all, and dried up by eleven in the morning. I made a similar cut in a young sycamore on the 8th of August; which bled the next morning, but stopped before nine o'clock. It did so for two or three days, but then totally drying. Afterwards removing to York the first of November, I here pierced, and otherwise wounded, two sycamores: but I have not perceived that they have stirred to this day. Since Mr. Ray has assured me, that those of Warwickshire bled the 16th of November last past copiously; and since the walnut-tree also.

Extract of another Letter from Mr. Ray, of Feb. 8, 1670-1, containing some Experiments about the Bleeding of the Sycamore, and other Trees; as also, a considerable Note of Pliny about the Mulberry Tree. N° 68, p. 2069.

Concerning the bleeding of the sycamore, let me acquaint you with the following experiment. The first instant it froze, the wind at north; the frost and wind continued (some little snow and rain falling) the 2d, 3d, 4th, 5th, 6th, until the 7th in the morning, when the wind came about to the south-east, and the weather broke up fast. The sycamores bled not all this while, but the 7th about noon all trees of that kind bled very freely, both at the twigs and body, and I struck above a dozen.

At this critical season I was willing to repeat the experiment on other trees; and to this end I forthwith struck the hawthorn, hazel, wild rose, gooseberry bush, apple tree, cherry tree, blather nut, apricot, cherry-laurel, vine, walnut; yet none bled but the last; and that faintly in comparison of the sycamore. This is consonant to our former experiments: and if it did happen that these sycamores bled not all this winter before at the wounds made the first of November, I now think, that if new wounds had been still made at every break of frost, some signs, at least of our Yorkshire bleeding, might have been discovered before now.

In all the monuments of the ancients, collected by the great industry of Pliny, I find but few instances of this nature. Amongst those few, one is registered with two or three remarkable circumstances to our purpose. He tells us, that the physicians of old, when they had a mind to draw the juice of the mulberry-tree, were wont to strike it skin deep only, and that about two hours after sun-rise. This experiment is twice mentioned by him, and in both places as a strange phenomenon. We might make our comment upon the places, but for this time are content only to transcribe the texts. Lib. 16. c. 38. Mirum; hic (cortex) in Moro, Medicis succum quærentibus, ferè horâ diei secundâ, lapide incussus manat, altius fractus siccus videtur. Lib. 23. c. 7. Mora in Ægypto et Cypro sui generis, ut diximus, largo succo abundant, summo cortice desquamato, altiore plagâ siccantur; mirabili naturâ.

Some Observations concerning the Variety of the Running of Sap in Trees, compared with a Weather Glass; made in April, 1670. *With some Ways of ordering Birch Water. By Dr.* EZ. TONGE. *N°* 68, *p.* 2070.

I am this day very much confirmed in my apprehension, that trees and other plants, if we could contrive them, as I have projected in my Sap-wiser to that purpose, would far better indicate the alteration of weather, as to heat, cold, moisture, drought, than any weather glasses I have yet tried. For though my weather glasses continued at the same station all this day, (April 13) my trees have altered their temper so much, that 24 of them which ran tolerably this forenoon, yield not a pint of sap this afternoon; and though one of them ran most part of the day, the rest ceased about one or two o'clock in a fair clear sun-shiny season, retarded only by a western wind; though that be reputed mild and cherishing.

These trees ran above two quarts in the morning. The weather glass continues the same, viz. about 11 inches water, these two days. Thursday the 14th it was 9½ only. Friday the 15th, my weather glass at noon was advanced from 9 to 10½, yet the quantity of birch water this day exceeded my former from these trees, for I had above 2½ quarts before noon.

But for cold, I find that the air, when any whifling blast of cold wind stirs, stays my birches. I want a wind-fane.

Saturday the 16th, these 24 birches began to run presently after sun-rise, and ran about three quarts, and ceased about two o'clock in the afternoon, having till then continued to run. Sunday the 17th, it rained so that we could make no observations what sap these trees might spend; neither did rain and all amount to much above a gallon and a half. Monday the 18th they ran until noon. Tuesday and Wednesday the 19th and 20th, wherein was expected greater store of sap, after the rain, the trees spent not a drop. Saturday the 23d, my weather glass stood at 7½, it being a rainy and boisterous morning, the rain not allaying the wind. At 9 o'clock of that forenoon, my birch water worked in the barrel *per se*: Which seems to verify Mr. Souton's relation from his brother, a Swedish merchant, importing that birch water in Sweden works alone; perhaps collected in great quantity. Only I put a very few cloves into my sap. Boiled to a third or less, it keeps well, especially when boiled with the buds or sprigs of the same tree, as I have been informed.

April 16, 17, 18, in the year 1670, birch sap mixed with rain water at the tree, fermented with rosemary sprigs steeped in spirit of wine: which warmed the stomach as strong wine, and pleased the palate; though the taste in the mouth was somewhat waterish.

A Letter by Dr. Tonge, about the retarding the Ascent of Sap; with some other Queries relating to that Subject. N° 68, p. 2072.

Last night Sir Robert Moray did me the favour to acquaint me in discourse with some particulars about the gathering of sap in fruit trees, and the retarding the ascent thereof; which he had received from an eminent planter in Glocestershire: concerning which, I thought it fit to communicate some reflections of mine.

It was proposed to me by way of quere; How to gather every drop of sap that should rise in any fruit tree? This, I said, I thought not feasible, by what I had hitherto experienced, though deserving of farther inquiry hereafter.

Some further Inquiries by Dr. TONGE, concerning the Running of Sap in Trees; the keeping of such Sap, and brewing with it; a Way of colouring of Leaves, Fruit, &c. As also about multiplying Crab-stocks, and propagating Trees by Layers, &c. N° 68, p. 2074.

Containing queries only, without answers or any information whatever.

An Account of two Books. N° 68, p. 2077.

I. Miscellanea Curiosa Medico-Physica Academiæ Naturæ Curiosorum. Lipsiæ, 1670, in 4to.

This is a work very lately begun in Germany by a society of ingenious philosophers, called Academia Naturæ Curiosorum (an academy of curious inquirers into nature) which some years ago established themselves in that country, with a design chiefly to improve physic and natural philosophy. For which purpose they have undertaken each of them to consider and write upon determinate subjects, thereby to penetrate into the nature and qualities of particular bodies, as of the vine, tobacco, aloe, scurvy-grass, wormwood, scorzonera, St. John's wort, iron, brass, the unicorn, the stag, crabs, the blood-stone, eagle-stone, musk, civet, amber, &c. Besides which they have attempted, in the manner adopted in France, Italy, and England, to write and publish Ephemerides, or a Journal, or Transactions of a Philosophical Nature, intending therein to present the curious yearly with such medical, anatomical, botanical, pathological, chirurgical, therapeutical, and chemical observations, as they shall by their correspondence, either with the physicians and philosophers of their own body and nation, or with those of other societies and countries, procure and digest. And of this we have a specimen in the book of which an account is now to be given, out of the copy which was sent by the learned Dr. Sacks, (a member of that body) to the editor. In it are contained 160 observations and 19 plates, being the sum of what they have collected for the first year, 1670.

1. A relation of snakes kept tame. 2. A memoir to prove that part of the chyle is by the thoracic duct conveyed into the subclavian, and so into the cistern of the breasts, whence by certain pipes it is carried into the glands or kernels, there to be elaborated, either for the aliment of the breasts themselves, or the food of fœtuses.* 3. An observation of a polygonum cocciferum. 4. A description of an anti-pleuritic medicine made of the rinds of the longer sort of Italian pumpions, boiled in very old oil of olives, and strained through a sieve. 5. A description of a remedy for an atrophy of the eye. 6. A narration of a serpent petrified in the stomach of a stag. [A bezoar-stone having a serpentine

* An anatomical error. See note at p. 526 of this volume of the Abridgement.

appearance.] 7. An account of a horny substance grown out, near the last two short ribs, to a considerable length, resembling the horn of a young deer. 8. A way of making a person born deaf to hear. 9. Of an egg naturally bred in an egg; of stones naturally found included in stones, &c. &c. 10. Of a child born dropsical. 11. Of a child of one year old so diseased with the hydrocephalus, that when opened there were taken out of his head 36 ounces of clear but saltish water. 12. Of worms let out by venesection and urine. 13. Of the periodical head-ach. 14. An observation concerning the vincetoxicum or swallow-wort, and its use in scrophula and dropsies. 15. Of a fœtus tinged yellow by saffron in the uterus. 16. Of several stones come away together with the blood of a man 72 years of age, when blooded in the arm. 17. Of a sort of craw-fish, which taste bitter when boiled. 18. Of a flame broken out of several stomachs, and burning like spirit of wine, both in dead animals and living men. 19. A way of dwarfing men, by anointing their back-bones in their very infancy with the grease of moles, bats, and dormice; together with an intimation of the art used at Bononia to dwarf their dogs, by often washing (from the first day they are whelped) their feet and back-bone; thereby drying and hardening those parts, and so hindering their extension. 20. An abscess in the abdomen cured by an incision made about four inches under the ribs. 21. Of many odd examples of women and men greedy to eat very strange things. 22. Of making chylous water, without any disease in the kidneys or bladder, in an atrophy of the whole body. 23. Of an American aloe, planted in Silesia, and remaining steril for 31 years, but after that time shooting out very many branches and flowers; its chief stem 18 feet high, bearing 21 branches, whereof some had above 200 flowers. 24. Of two rare symptoms about the sight. 25. An observation of a muscus terrestris (ground moss) which being gathered in August and September, and reduced to powder, fulminates when held or blown into a flame. 26. Of a nobleman in Silesia, who was so strong, that with his hands alone he could break a horse-shoe and a rix-dollar, and hold at one and the same time three men, two under his arms, and one by his teeth fastened in his clothes, &c. &c. 27. Of a bezoar-stone encompassing a dart, found in the emperor's repository. 28. Of eels being viviparous. 29. Of an hermaphrodite stockfish, having both melt and rowe, as also of two crocodiles of both sexes. 30. Of a strange antipathy to music in a man. 31. Of light inbred in the insect called scolopendra, shining in the dark, and sparkling when compressed. 32. Several observations concerning the milk diet against the gout. 33. Of mercury crouded into the crural vein of a dog, to the quantity of half an ounce, without any hurt appearing in the animal. 34. Of a jasper-stone wormeaten.

II. Physica, in decem Tractatus Distributa, Auth. Honorato Fabri, S.J. Lugduni Galliarum, 1669, in 4to.

Of this vast work the author published, An. 1666, at Paris, two treatises; the first, De Plantis et Generatione Animalium: the latter, De Homine; by a kind of retrograde method opposing the order of publishing to that of composing; for, whereas in the composition, he proceeded from the simpler things to the more complex, in the publication and communication, he thought fit to produce the sum of all nature, that elaborate piece, man; in whom, besides the sensitive and vegetable faculties, and the powers of mixts and the elements common to all bodies, there shines out a supereminent principle, the rational soul. In the preface the author gives an account of the method used by him in this whole work, and of his performance in this very volume.

END OF VOLUME FIFTH OF THE ORIGINAL.

A Solution, given by Mr. John Collins, *of a Chorographical Problem, proposed by* Richard Townley, *Esq. N° 69, p. 2093. Vol. VI.*

Problem. The distances of three objects in the same plain, being given, as A, B, C; the angles made at a fourth place in the same plain, as at S, are observed. The distances from the place of observation to the respective objects are required.

The problem has six cases. See pl. 13, fig. 10, 11, 12, 13, 14, 15.

Case I.—If the station be taken without the triangle made by the objects, but in one of the sides thereof produced, as at S, in fig. 10, find the angle ACB; then, in the triangle ACS, all the angles and the side AC are known, whence either or both the distances SA or SC may be found.

Case II.—If the station be in one of the sides of the triangle, as at S, in fig. 11, then having the three sides AC, CB, BA, given, find the angle CAB; then again in the triangle SAB, all the angles, and the side AB are known, whence may be found either AS or SB, geometrically, if you make the angle CAD equal to the observed angle CSB, and draw BS parallel to DA, you determine the point of station S.

Case III.—If the three objects lie in a right line, as ACB, fig. 12, suppose it done, and that a circle passes through the station S, and the two exterior objects A, B; then is the angle ABD equal to the observed angle ASC (by 21 of the 3d book of Euclid) insisting on the same arch AD; and the angle BAD in like manner equal to the observed angle BSC; by this means the point D is determined. Join DC, and produce the same; then a circle passing through the points ABD intersects DC, produced at S, the place of station.

Calculation. In the triangle ABD, all the angles and the side AB are known, whence may be found the side AD. Then in the triangle CAD, the two sides CA and AD are known, and their contained angle CAD, whence may be found the angles CDA and ACD, the complement whereof to a semicircle is the angle SCA; in which triangle the angles are now all known, and the side AC; whence may be found either of the distances SC or SA.

Case IV.—If the station be without the triangle, as fig. 13, made by the objects, the sum of the angles observed is less than four right angles. The construction is the same as in the last case, and the calculation likewise; saving that you must make one operation more, having the three sides AC, CB, AB, thereby find the angle CAB, which add to the angle EAD, then you have the two sides, viz. AC, being one of the distances, and AD, found as in the former case, with their contained angle CAD given, to find the angles CDA and ACD, the complement whereof to a semicircle is the angle SCA; now in the triangle SCA, the angle at C being found, and at S observed, the other at A is likewise known, being the complement of the two former to a semicircle, and the side AC given; hence the distances CS or AS may be found.

Case V.—If the place of station be at some point within the plain of the triangle, as in fig. 14, made by the three objects, the construction and calculation is the same as in the last, saving only that instead of the observed angle ASC, the angle ABD is equal to the complement thereof to a semicircle, to wit, it is equal to the angle ASD; both of them insisting on the same arch AD; and in like manner the angle BAD is equal to the angle DSB, which is the complement of the observed CSB, and in this case the sum of the three angles observed is equal to four right angles.

In these three latter cases no use is made of the angle observed between the two objects A and B, that are made the base line of the construction; yet the same is of ready use for finding the third distance or last side sought, as in the 4th case, in the triangle SAB, there is given the distance AB, its opposite angle equal to the sum of the two observed angles, and the angle SAB attained, as in the 4th case: hence the third side or last distance SB may be found.

And here it may be noted, that the three angles CAS, ASB, SBC, are together equal to the angle ACB; for, the two angles CSB and CBS are equal to ECB, as being the complement of SCB to two right angles; and the like in the triangle on the other side. Ergo, &c.

Case VI.—If the three objects be A, B, C, and the station at S, as before, it may happen, according to the former constructions, that the points C and D may fall close together; and so a right line, joining them, shall be produced with uncertainty; in such case the circle may be conceived to pass through the place of station at S, and any two of the objects, as in fig. 15, through B and C, wherein making the angle DBC equal to the observed angle ASC, and BCD equal to the complement to 180 degrees of both the observed angles in DSB; thereby the point D is determined, through which, and the points C, B, the circle is to be described; and joining DA, produced when necessary, where it intersects the circle, as at S, is the place of station sought.

This problem may be of good use for the due situation of sands or rocks, that are within sight of three places upon land, whose distances are well known; or for chorographical uses, &c. Especially now there is a method of observing angles very accurately by the telescope; and it was therefore thought fit to be now published, though it be a competent time since it was delivered in writing.

Some Observations on the Mines of Cornwall and Devon; describing the Art of Training a Load; the Art and Manner of Digging the Ore; and the Way of Dressing and of Blowing Tin. Communicated by a Person acquainted with those Mines. N° 69, p. 2096.

It is supposed by the miners, that there has been a great concussion of earth in that separation of "the waters from the waters" at the creation, or in Noah's flood, or at both times, whereby the waters moved and removed the then surface of the earth. That before this concussion, the uppermost surface of mineral veins or loads did in most places lie even with the then real, but now imaginary surface of the earth, which is termed by the miners, the shelf, fast country, or ground that was never moved in the flood.

That in this concussion of waters the surface of the earth, with the uppermost of those mineral veins, were then loosed and torn off, and by the descending of the waters into the valleys, both the earth or grewt, and those mineral stones or fragments, so torn off from their loads, which are constantly termed shoad, were together with and by the force of the waters carried beneath their proper places, and from some hills even to the bottoms of the neighbouring valleys; and thence by land floods, many miles down the rivers; in others more or less distant in the sides, according to their declivity and to the impetuosity of the waters.

Now these three general rules, on which seem to depend the grand reasons of this art, being supposed and premised, we thus proceed to training a load, or tracing a vein of ore.

Where we suppose any mine to be, we diligently search that hill and country, its situation, the earth, or grewt, its colour and nature, and what sort of stones it yields; that we may the better know the grewt and stones, when we meet with them at a distance in the neighbouring valley; for mineral stones may be found 2, 3, 4, 5 miles distant from the hills or loads they belong to.

After any great land flood, we go and diligently observe such frets as are usually made by those floods, to see if we can discover any metalline stones in the sides or bottoms thereof, together with the cast of the country (i. e. any earth of a different colour from the rest of the bank) which is a great help to direct us, as to which side or hill to search into. Neither will it be amiss in this place to subjoin the few, but sure characters of mineral stones, by which we know the kind of metal, and how much it yields. The first way is by its ponderousness, which easily informs us whether it be metal or no. The second is its porosity; for most tin-stones are porous, not unlike great bones almost thoroughly calcined; yet tin sometimes lies in the firmest stones. The third is by water, which we term vauning, and that is performed by pulverising the stone, or clay, or what else may be suspected to contain any mineral body, and placing it on a vauning shovel; the gravel remains in the hinder part, and the metal at the point of the shovel, whereby the kind, nature, and quantity of the ore is guessed at; and indeed most commonly without any great deception, especially if the vauner have any judgment at all.

But if no shoad may be found or discovered in such frets, then we leave that place, nor do we trust to any metalline stones found in the common river, for the reason before-mentioned, and because they rather cause distraction, than guide us to the finding out the load, especially if they are smooth, without protuberances, and asperities, such as are usual to stones newly broken: for then they plainly show they have been brought a great way, and in their tumblings thither are worn smooth. Then we go to the sides of those hills most suspected to have any loads in them, where there may be a conveniency of bringing a little stream of water, the more the better, and cut a leat, gurt, or trench, about two feet over, and as deep as the shelf, in which we turn the water to run two or three days; by which time the water, by washing away the filth from the stones, and the looser parts of the earth, will easily discover what shoad is there. If we find any, we have a certainty of a load, or at least a squat, in the upper parts of the hill. Squats are certain distinct places in the earth, not running in veins, differing from bonnys in this only, that squats are flat, bonnys are roundish.

Sometimes shoad may be found upon the open surface of the ground, being thrust up by moles in their hillock, or turned up by the plough, or by some other accident; for it is seldom found on the open surface of the ground, unless brought thither by an accident since the flood, especially in cultivated places; seeing that the corruption of vegetables and other creatures have in the long tract of time produced a new surface, heightened in some places above a foot, in others more or less.

When all these ways have been attempted for finding shoad, if we find any, it makes us proceed with the greater confidence, having an assurance of a load; but otherwise we must go by guess. And here is all the difference as yet between finding and not finding shoad. For in the next place we sink down about the foot or bottom of the hill an essay hatch, an orifice made for the search of a vein about six feet long and four broad, as deep as the shelf. And it is observable, they are always to be as deep as the shelf, for this reason, that otherwise you may come short of the shoad; but if we meet with none before, or when we come to the shelf or fast country, there is none to be expected; yet sometimes the shoad is washed away clean, when you come within two or three feet from the load, and then the load is a foot or two further up in the hill. If we find any shoad in this first essay hatch, our certainty is either increased if any shoad were found before, or begun. Neither does it add a little to make a right conjecture, how high up the hill, or far off, the load, string, or bonny is, carefully to mark how deep from the surface of the earth our shoad lies: for this is held an infallible rule, that the nigher the shoad lies to the shelf, the nigher the load is at hand, *et vice versâ.*

Although we find no shoad in this first hatch, having found some before by the ways before-mentioned, or having found none, we are not as yet altogether discouraged; but ascend commonly about 12 fathoms, and sink a second hatch as the former: And in case none appear in this, we go then as many fathoms on each hand at the same height, and sink there as before, and so ascend proportionably with three or more hatches, as it were in breast, till we come to the top of the hill, and if we find none in any of these hatches, then farewell to that hill.

But if we find any shoad in any of these hatches, we keep our ascending hatches in a direct line; and as we draw nearer the load, the deeper the shoad is from the surface, but nearer the shelf; as suppose it be seven feet deep, and but half from the shelf, then we presently conclude the load to be within a fathom or two of us, and so we lessen our first proportion accordingly, as of that of 12 fathoms to 6, 4, 2, 1; as our conjecture guides us.

Sometimes we overshoot a load, that is, get the upper side of it, and so we lose it; for which we have another good rule, viz. that finding shoad lying near the shelf in this hatch, and finding none in the next ascending, we have overshot our load. The remedy is easy, which is to sink nigher the hatch wherein we last found shoad.

At other times we find a new shoad, that is, two different shoads in one hatch, as suppose in this hatch we find our shoad eight feet deep, in the next we hope to find it at ten feet; but at two or four we meet with a new shoad and grewt, which we diligently observe, and at ten we meet with our first shoad: then we have a certainty of another load above the former, and it may be in training up to the second, we meet with the shoad of a third. Neither is this dissonant to the opinion and practice of the ancient tinners, who affirm that seven loads may lie parallel to each other in the same hill, but yet one only master load; the other six (three on each side) being the lesser concomitants. So may five lie in like manner; three are common.

Every load has a peculiar coloured earth or grewt about it, which is found likewise with the shoad in a greater quantity, the nearer the shoad lies to the load, and so lessened by degrees about a quarter of a mile's distance; farther than which, that peculiar grewt is never found with the shoad.

A valley may so lie, as at the feet of three several hills; and then we may find three several deads, *i. e.* common earth, or that loose earth which was moved with the shoad in the concussion, but not contiguous to the load in its first position, (which is also termed by us the run of the country,) with as many different shoads in the midst of each.—And here the knowledge of the cast of the country, or each hill, in respect of its grewt, will be very necessary, for the surer training of them one after the other, as they lie in order according to the foregoing rules of essay hatches: for the uppermost will direct you as to which hill to begin first.

It may be, that after we have trained up the hill, instead of a load we find nought but a bonny or squat; which likewise have their shoad, whose form is about two or three fathoms long and half as broad; few larger, most less: which communicates with no other load or vein, neither does it send forth any of its own; but is entire of itself, whose extremities terminate without running out into little innumerable strings, not lying within walls as loads; although they are in the shelf, whose surface is equal every where with that of the imaginary shelfy one, and may go down five or six fathoms deep, more or less, and there terminate; which squats are constantly wrought out with good advantage to the workers when found; neither is the tin of the baser sort.

The Art and Manner of Digging up the Ore.

The difficulty of this is not considerable, in comparison to that of training. When we have found our load, the last essay hatch loses or rather exchanges its name for that of a tin-shaft, or tin-hatch, which we sink down about a fathom, and then leave a little long square place, termed a shamble, and so continue sinking from cast to cast, *i. e.* as high as a man can conveniently throw up the ore with a shovel, till we find either the load to grow small, or degenerate into some sort of weed, which are divers; as mundic, or maxy (corrupted from marchasite) of three sorts; white, yellow, and green: daze, white, black, and yellow, being a kind of glittering stone, enduring the fire: iremould, black, and rusty: caul, red: clister, blood-red, and black.

Then we begin to drive either west or east, as the goodness of the load, or conveniency of the hill invite; which we term a drift, three feet over, and seven feet high; so as a man may stand upright and work; but in case the load be not broad enough of itself, as some are scarce half a foot, then we usually break down the deads on one side of the load, and after we begin to rip the load itself. [By deads here are meant that part of the shelf which contains no metal, but encloses the load as a wall between two rocks, and not as that which was mentioned in the concussion, as in training.]

The instruments commonly used in mines, that serve for ripping the loads, and breaking the deads, and landing both the ore and deads, are; 1. A beele or Cornish tubber (*i. e.* double points) of 8lb. or 10lb. weight, sharpened at both ends, well steeled and holed in the middle. It may last in a hard country half a year, but new pointed every fortnight at least. 2. A sledge, flat headed, from 10lb. to 20lb. weight; will last about seven years, new ordered once a quarter. 3. Gadds or wedges of 2lb. weight, four square, well steeled at the point, will last a week; then sharpened after two or three days. 4. Ladders. 5. Wheelbarrows, to carry the deads and ore out of the drifts or adits to the shambles.

The proportion of men is, two shovelmen, three beelemen, which are as many as one drift can contain, without being a hinderance to each other. The beelemen rip the deads and ore; the shovelmen carry it off, and land it by casting it up with shovels from one shamble to another, unless it be where we have a winder with two keebles (great buckets made like a barrel with iron hoops, placed just over the then termed wind hatch,) which as one comes up, the other goes down.

A great deal of this skill consists in the exact knowledge and observation of the loads dipping; for which we have this general rule: That most of our tin loads, which run from west to east, constantly dip towards the north; sometimes they underlie, that is, slope down towards the north, three feet in eight perpendicular; which must be observed, that we may exactly know where to sink an air-shaft, when occasion requires; yet in the higher mountains of

Dartmoor there are some considerable loads, which run north and south: these underlie towards the east.

Four or five loads may run parallel to each other in the same hill, and yet (which is rare) meet all together in one hatch, as it were in a knot, (which well tins the place;) and so separate again, and keep their former distances. Such a knot has been observed, and wrought on Hingston, a known mineral down or common in Cornwall.

The master loads may generally be from three to seven feet broad, seldom more; unless at certain places, and where several loads may chance to make a knot, or send forth strings or veins; neither retain they their usual breadth in all parts: for they may be six feet at one place, scarce two at another; nay sometimes scarce half an inch over; but that is to be understood of strings and the narrowest places of the concomitant ones.

The load is usually in a hard (*i. e.* in a rocky or shelfy) country, made up of metal, spars, and other weeds, and as it were all along a continued rock; but has many veins and joints, as we speak; but in some softer countries the tin may lie in a softer consistence, as that of clay in a manner petrified, whereby it may rationally be expected, that they make more speed and show in their drifts, and the same number of beelemen employ more shovelmen.

Concerning water, we have these observables; that in most places we meet with it at some feet deep from the loady surface, in others not at many fathoms deep. It runs commonly through the heart of the load, not in a direct continued channel, but windingly in and out, insensibly through the veins and joints of the load.

When we are come at any depth, and find the water begin to annoy us, as it quickly will if any be in the work, we descend to the bottom of the hill, where we have that conveniency, and at the lowest place begin as little a drift as the conveniency of working or driving will permit (scarce half so large as that of the load) on a level, till we come up to our work. And here the use of the dial is needful, which we term plumming and dialling, (either to know the exact place of the tin work, where to bring our adit; or where to sink to bring down our air shaft even with the desired place, perpendicularly; or to know which way our load inclines, when any flexures happen;) which is to be performed in this manner, viz.

A skilful person, with an assistant, pen, ink, paper, compass, and long line, after his guess of the place above ground, descends into the adit or work, and there fastens one end of the line to a fixed thing; and then lets the incited needle rest, exactly observing with his pen at what point it stands; then he goes farther in, the line still fastened, and at the next flexure in the adit makes a mark on his line, by knot or otherwise, and sets his dial down again, and there likewise notes down that point on which the needle stands, at the second position; and so proceeds from turning to turning, still marking down the points, and his line, till he comes to the intended place; which performed and exactly set down, he ascends, and begins at the orifice of the adit or work, and repeats above ground what he did in the work; brings his first knot, or mark in his line, to such a place, as the needle will stand at the same point it did under ground at the knot, and so proceeds till he come exactly over the intended place in the mine.

But to return to the water, if this conveniency of an adit may be had, then our water annoys us but a little, as long as we keep on that level with the adit; for we drive not always on one and the same level: As for instance; at five fathoms we make a drift both ways, and sinking five fathoms more, we make another drift at ten fathoms, and as deep as we please. Now when we once pass that level on which our adit runs, and the water begins to trouble us, we have this remedy; either with a winder and keebles, or leathern bags, pumps, or buckets to get it up to the adit level, and so we are forced to do to the very top, where we have not the convenience of an adit, as in plains. Some, but very few works may be dry.

We observe, that if we have water we never want air sufficient for respiration, and our candles to burn in; but yet this caution must be annexed, that in a soft loose quagmire clayey country, by the falling of the deads after us, yet not in such measure as totally to stop us up, although we have water, yet our air is rather too copious, or so much condensed, as that it becomes in a manner a damp, and requires an air-shaft for vent, which damps are sometimes enlarged by working of the mundic with the ore.

In case the country be not strong enough (as being over-soaked with water from above) to support its own weight, we under-prop our drifts with stemples, and wall plates, placed much like a carpenter's square, on the one side and over head.

The Manner and Way of Dressing Tin.

After the ore is landed, and the greater stones broken at the top of the mine by the shovelmen, it is brought on horses to the stamping or knocking mills, and unloaded at the head of the pass (i. e. two or three bottom boards with two side boards slopingwise) in which the ore slides down into the coffer; but that it may not tumble down all at once, there is placed a hatch nigh the lower end of the pass (i. e. a thwart board to keep up the ore) beneath that comes in the cock water in a trough cut in a long pole, which with the ores fall down into the coffer, (i. e. a long square box of the firmest timber, three feet long and one foot and a half over) wherein the three usual lifters, placed between two strong broad lones, having two braces or thwart pieces on each side to keep them steady as a frame, with stamper heads, weigh about 30 or 40lb. a piece, of iron, which serve to break the ore in the said coffer; these lifters, about eight feet long and half a foot square, of heart oak, having as many in-timbers or guiders between them, are lifted up in order by double the number of tappets, (fastened to as many arms passing diametrically through a great beam, turned by an over-shoot water wheel on two boulsters) which exactly, but easily, meet with the tongues so placed in the lifters, as that they quickly slide from each other, suffering the lifters to fall with great force on the ore, thereby breaking it int small sand, which is washed out by the cock water through a brass grate, holed very thick, placed within two iron bars at one end of the coffer into the launder, (i. e. a trench cut in the floor, eight feet long and ten feet over) stopped at the other end with a turf, so that the water runs away, and the ore sinks to the bottom, which when full is taken up with a shovel. The launder is divided into three parts, i. e. the forehead, the middle, and the tails. That ore which lies in the forehead, i. e. within one foot and a half of the grate, is the best tin, and is taken up in a heap apart. The middle and tails in another, accounted the worst.

The latter heap is thrown out by the trambling buddle, i. e. a long square tie of boards or slate, about four feet deep, six long, and three over; wherein stands a man bare-footed with a trambling shovel in his hand to cast up the ore, about an inch thick, on a long square board just before him as high as his middle, which is termed the buddle head, who dextrously with the one edge of his shovel cuts and divides it longways in respect of himself, about half an inch asunder, in which little cuts the water coming gently from the edge of an upper plain board carries away the filth and lighter part of the prepared ore first, and then the tin immediately after, all falling down into the buddle, where with his bare foot he strokes and smooths it transversely to make the surface the plainer, that the water and other heterogeneous matter may without let pass away the quicker.

When this buddle grows full we take it up, here distinguishing again the forehead from the middle and tails, which are trambled over again; but the forehead of this with the forehead of the launder are trambled in a second buddle in like manner; the forehead of this, being likewise separated from the other two parts, is carried to a third, or drawing buddle, whose difference from the rest is only this, that it hath no tie, but only a plain sloping board, whereon it is once more washed with a trambling shovel, and so it new-names the ore, black tin, *i. e.* such as is completely ready for the blowing-house.

We have another more curious way, termed sizing, that is, instead of a drawing buddle, we have a hair sieve, through which we sift, casting back the remainder into the tails, and then new tramble that ore. After the second trambling we take that forehead in the second buddle, and dilve it (*i. e.* by putting it into a canvas sieve, which holds water, and in a large tub of water lustily shake it) so that the filth gets over the rim of the sieve, leaving the black tin behind, which is put up into hogsheads covered, and locked till the next blowing.

The tails of both buddles, after two or three tramblings, are cast out into the first strake, or tie, which is a pit purposely made to receive them; and what over small tin else may wash away in trambling. There are commonly three or four of them successively, which contain two sorts of tin; the one, which is too small, the other too large. The latter is new ground in a crazemill (in all respects like a greistmill with two stones, the upper and the nether) and after that trambled in order. The former is dressed on a reck, with the shovel and water, and made fit to be used. A reck is a frame made of boards about three feet and a half broad, and six long, which turns upon two iron pegs fastened in both ends, and the whole placed upon two posts, so that it hangs in an equilibrium, and may like a cradle be easily removed either way.

The Manner and Way of Blowing Tin.

Conceiving it sufficient to say, that our furnace is no other than an alman furnace, I shall proceed (only taking notice, that our lime, though the strongest I ever yet heard of, as being made of the hardest marble, will not endure the fire in our hearth, but we must use a particular kind of clay) to describe a tin-kiln, whose structure is four square. At the top is a large moor stone about six feet long, four broad; in the middle thereof is a hole made about half a foot diameter. This stone serves as a head or cover to another like stone, placed about a foot beneath it, but is not so long by half a foot as the upper, because it must not reach the innermost or back part of the wall, which is the open place through which the flame ascends from a lesser place below that, where a very strong fire of furze is constantly made, and another little square hole on the outside, for a purpose soon to be mentioned; the fore part is like a common oven, and has a similar chimney in the fore part.

Now when we perceive much mundic in our tin, (which spoils it by making it brittle and not malleable) which we easily discern before knacking, we are necessitated to burn away this weed in the kiln, thus: All the black tin (brought to the blowing-house in little canvas bags on horses) that is to be burned, is laid on the top stone, the kiln being thoroughly heated before, and at the hole above-mentioned cast down on the second or bottom stone; at the mouth of which stands a man with an iron coal-rake, to give notice when enough is let down to cover the stone all over about three or four inches thick, which he performs with his rake: The hole at the top is immediately covered with green turfs, that the flame may reverberate the stronger. The rakeman, after this, constantly moves the tin with his rake, that all parts of the mundic may get uppermost of the tin, and so be burned away; which we know by the flame, as it will then become yellow, and the stench lessen; for while the mundic burns the flame is blue. Then with his rake he thrusts it down, at the open place behind, into the open fire, and then receives a new supply of tin from above, as before. Now when the place beneath, where the fire is made, grows full of tin, coals, and ashes, with his rake he draws it forth with the coals on the little square hole on the one side near the back, where the ore, fiery hot and red, lies in the open air to cool; which will scarce be in three days, because of the coals that lie hid in it: But in case we cannot stay so long, then we quench it with water, and is like mortar. Although we let it cool of itself, or with water, we must new tramble it or wash it, before we put it into the alman furnace. And because I have set down the proportions of ore and fire already, in the answers to the mineral queries, I will not repeat them here, but only add an observation or two, and then dismiss this subject. Moor-tin (*i. e.* such as is dug up in the moors) runs or melts best with moor-coal, charked: But our tin which lies in the country, runs best with an equal proportion of all charcoal peat (*i. e.* moor-coals) for the first running; but when we come to remelt our slags, then we use charcoal. When all is melted down and remelted, there sometimes remains a different slag in the bottom of the float, which we term mount-egg; and that it is mostly an iron body, though of a tin colour, I accidentally assured myself by applying one of the poles of a loadstone to it, which quickly attracted it, yet not so strongly by far as that of iron.

An Account of two Books. N° 69, p. 2114.

I. Prodromo overo Saggio di alcune Inventioni nuove premesso all Arte Maestra di P. Francisco Lana della Campagnia di Jesu, in Brescia, 1670, in 4to.

This introduction is premised to a work, designed by the author to show the value of the principles and knowledge of natural philosophy, by some of the more considerable inventions and experiments hitherto found in authors, and other new ones of this writer himself.

II. Joh. Henr. Meibomii* de Cerevisiis, potibúsque et ebriaminibus extra Vinum aliis Commentarius, annexo libello Turnebi de Vino. Helmestadii, 1668, 4to.

This author shows how and by whom, after the first use of water and milk, were introduced the drinks made out of vegetables; as the vine, barley, wheat, maize, millet, oats, rice, apples, pears, pomegranates; and the various juices tapped out of trees, as the palm, birch, sycamore, maple, and many more. To which he adds those drinks that are prepared out of roots, berries, sugar, and sugar-canes, and the like. Then he proceeds to that luscious drink, which is made out of that animal substance honey, called mead, and is mentioned in some of the most ancient writers, amongst whom he cites Orpheus:

> Ἔυτε ἄν δὴ μιν ἴδηαι ὑπὸ δρυσὶν ὑψικόμοισιν
> Ἔργεισι μεθύοντα μελισσάων ἐριβόμβων
> Δῆσον——
>
> *Postquam igitur cernes ipsum sub quercubus altis*
> *Depositum, Succo ex Apibus mox vincula membris*
> *Injice* ——

Which relates to the fable, in which the night is represented to have counselled Jupiter, designing to dethrone and castrate his father Saturn, that he should, by making him drunk with mead, cast him into a deep sleep, and so do with him what he pleased.

To these he subjoins many intoxicating drinks, made with narcotic and other herbs; as also the various stupifying fumes, among which tobacco is now so famous and common; to which he applies what Virgil sings of Cacus;

> *Faucibus ingentem fumum, mirabile dictu,*
> *Evomit, involvitque domum caligine cæcâ,*
> *Prospectum eripiens oculis, glomerátque sub auras* †
> *Fumiferam noctem, commistis igne tenebris.*

* Meibomius was born at Helmstadt in 1590. After studying at various universities in Germany he visited Italy, and on his return was made professor of physiology in the university of Helmstadt; but was obliged to quit that place in 1627, in consequence of the troubles in which the country was then involved. He removed first to Schwerin, and afterwards to Lubeck, where he died in 1655. Meibomius was a man of great erudition. Besides the treatise above-mentioned, and various academical dissertations, he wrote Commentar. in Hippocrat. jusjurandum, 1643; De usu Flagrorum in re Medica, 1643; Cassiodori formula comitis Archiatrorum. This was published after his death, in 1668, by his son. Other works were also written by this author, too numerous to be particularised here. Meibomius had invitations, accompanied with the offer of a handsome salary, at one time from Copenhagen, at another time from Stockholm, to accept the appointment of physician to those courts, but he declined both.

† Sub antro. Virg. Æn. VIII. 254.

He concludes with the various modern ways of brewing beer.

The annexed tract of Turnebus is both elegant and considerable for the observations therein alleged, to show the noxious effects of wine in those people that drink much of that liquor; preferring good water, milk, beer, and especially good cyder, far above it; and taking notice, that wine drinkers have generally more deformed, lower and drier bodies, as also shaking limbs, and a more hasty decay of sight than others.

Extract of divers Letters, written by MARTIN LISTER, *Esq. to the Editor; touching some Inquiries and Experiments of the Motion of Sap in Trees, and relating to the Question of the Circulation of the same. N° 70, p. 2119.*

To continue our experiments concerning the motion of the sap in trees, (from N° 68) Feb. 11, all was here covered with a white frost betwixt 9 and 11 in the morning. The weather changing, I made the experiments which follow, on the sycamore, walnut, maple. A twig cut asunder would bleed very freely from that part remaining to the tree; and, for the part separated, it would be altogether dry and show no signs of moisture, although we held it some pretty time with the cut end downwards; but if this separated twig was never so little tipped with a knife at the other end, it would forthwith show moisture at both ends. The same day, late in the afternoon, the weather very open and warm, a twig cut off in like manner as in the morning, would show no moisture at all from any part. These experiments we repeated many times with constant and like success on all the trees above-mentioned.

I shall long to hear the success of your experiments in the question of the circulation of the sap. I have many years been inclined to think, that there is some such motion in the juices of vegetables. The reasons which induced me are; 1. Because I find that all the juice of a plant is not extravasate and loose, and like water in a spunge; but that there are apparent vessels in plants, analagous to veins in animals: which is most conspicuous and clear in such plants whose juice is either white or red, or saffron coloured; for instance, in each kind of juice we propose lactuca, atractylis, chelidonium majus. 2. Because that there are many plants whose juice seems never to be at rest, but will spring at all times freely, as the blood of animals on incision.

The way of ligature by metalline rings, is an expedient I have not used; but other ligatures I have, upon a great number of our English plants, not without the discovery of many curious phænomena. The success of an experiment of this nature upon cataputia minor Lobel. was as follows: I tied a silk-thread upon one of the branches of this plant, as hard as might be, and not break the skin: there followed no greater swelling, that I could discern, on the one side of the silk more than on the other; although in often repeating the experiment, some silks were left hours and days unloosed, and yet the dimple which the thread had made in the yielding branches, had a little raised the immediate sides, but both alike: the plant in like manner would bleed very freely both above and under the tie. This was also, I thought, very remarkable, amongst other things, in this experiment, that in drawing the rasor round about the branch just above or below the tie, the milky juice would suddenly spring out of infinite small holes, besides the orifice, for more than half an inch above and below the tie: which seems to argue, that though there was no juice intercepted in appearance from any turgescence, (as in the process upon the members of a sanguineous animal) yet the veins were so over-thronged and full, that a large orifice was not sufficient to discharge the sudden impetus and pressure of a someways straightened juice.

That I might satisfy myself about some of my doubts, I have been most concerned, according to former thoughts and inclinations, in examining the truth of these queries, viz. Whether saps are not to be found at all seasons of the year in a much like consistence and quantity in the respective parts of a vegetable; and what communication one part of a plant may have with another in relation to the ascent and descent of sap?

Now, because sap is then said to ascend from the root, when it is found to move in tapping; I lopped off certain branches of a sycamore, the morning betimes of a hard frost (Feb. 21) before they would bleed, or show any sign of moisture. This I did to vary the efficient, not willing to wait the change of the weather, and the sun's heat; but brought them within the air of the fire: And by and by, as I expected, they bled apace, without being sensibly the warmer.

The experiment repeated afforded me several phænomena, which follow; and almost proved an universal way of bleeding all sorts of trees, even those which of themselves would not show any signs of moisture.

Poles of maple, sycamore and walnut, cut down in open weather, and brought within the warmth of the fire, did bleed in an instant. Also willow, hazel, cherry, woodbine, blathernut, vine, elder, barbery, apple-tree, ivy, &c. Whicking and egg-berry tree (*i. e.* padus Theophrasti) tried in the same manner in Craven.—Briar and raspberry-rods were more obstinate. Ash utterly refused, even heated hot.—Branches, that is, poles with their tops entire and uncut, bleed also when brought to the fire side; but seem not so freely to drink up their sap again when inverted, as when made poles.—The same willow-poles, left all night in the grass-plot, and returned the next day to the fire-side, bleed afresh.—Maple and willow-poles bleed and cease at pleasure again and again, if quickly withdrawn and balanced in the hand, and often inverted to hinder the falling and expence of sap: Yet being often heated, they will at length quite cease, though no sap was at any time sensibly lost. And when they have given over bleeding, that is, showing any moisture, by being brought within the warmth of the fire, the bark will yet be found very full of juice.—A hard ligature made within a quarter of an inch of the end of a wood-bine rod, did not hinder its bleeding at all when brought within the warmth of the fire.—Maple and willow-poles, &c. quite bared of bark, and brought to the fire, will show no moisture at all in any part.—One barbery, or pipridge-pole, bared of its bark, brought to the fire, showed moisture from within the more inward circles, though not any from the outward. Maple and willow poles, &c. half bared of bark, would bleed by the fire, from the half only of those circles which lay under the bark.—Maple and willow-poles, split in two and planed, would not show any moisture on the planed sides, but at the ends only.—A pole of ivy did of itself exude and show a liquid and yellowish rosin from the bark and near the pith; but when brought to the fire-side, it bled a thin and colourless sap from the intermediate wood circles.—A pole of willow (for example) bent into a bow, will ooze its sap freely, as in bleeding either spontaneously or by the fire.

Extract of a Letter from FRANCIS WILLOUGHBY, *Esq., March* 16, 1670-1, *relating to some Particulars above-mentioned in M.* LISTER'S *Communications.* N° 70, *p.* 2125.

We have reviewed our old notes, and made some few experiments, and find that branches of willow, birch, and sycamore, cut off and held perpendicularly, will bleed without tipping; and that the cutting off of their tops does not sensibly promote the bleeding. We doubt not of Mr. Lister's diligence and veracity, but wonder that our experiments should differ.* The trials we have made this year confirm those we communicated to you formerly, viz. The sycamore bleeds upon the first considerable frost, after the leaf is fallen, as it did plentifully the 16th of November last: And both that walnut and maple bleed all winter long after frosts, when the weather relaxes, and the sun shines out; but walnut and maple begin not so soon as the sycamore. The birch will not bleed till towards the spring. This year it began something sooner than ordinary about the beginning of February.

* See this difference removed below, by Mr. Lister himself.

We cut off pretty thick branches of birch, and, having tipped the ends, inverted them, and fastened a limbus or ring of soft wax to the great ends, which we held upwards; making with the plane of the end a vessel of about an inch deep, whereinto we poured water, which in a few minutes sunk into the pores of the wood, and running quite through the length of the branch, dropped out of the ends considerably fast, continuing so to do as long as we poured on water. The like experiment we made by fastening such rings of wax to the lesser ends, and pouring in water, which run through the wood, and dropped out of the greater ends, as fast or faster. This we tried once upon a sycamore without success.

Extract of a Letter from Mr. Lister, *written April* 8, 1671, *both in relation to the further Discovery of the Motion of Juices in Vegetables, and removing the Difference noted in the foregoing Letter. N° 70, p.* 2126.

One or both ends of the pith of a willow pole, sealed up with hard wax, will yet freely bleed by the warmth of the fire.

March 23, was the greatest frost and snow we have had this winter in these parts about York. Some twigs and branches of the very same willow tree, as formerly, and likewise of many other willow trees, taken off this morning, March 23, when brought within the air of the fire, would show no moisture at all, no, not when heated warm, and often and long turned.

March 24, the same willow branches, which yesterday would not bleed, and were thrown upon the grass-plot all night, as well as those newly cut down by the fire-side, freely showed moisture, and bled this morning upon the breaking up of the frost.

Ash poles and branches this day as well as yesterday would by the fire be no more moist than when I formerly tried them.

The same morning, March 23, a twig of maple, which had had the top cut off the 7th of February last, and which then bled, this day being quite taken off from the tree, and brought within the air of the fire, and held with the formerly cut-end downwards, did not run at all at the end, but held on in that posture, it did run apace at the other new-cut end uppermost, so as to spring and trickle down.

Note, That this doth well agree with my experiments made the last year at Nottingham, where I observed wounds of some months standing to bleed apace at the breaking up of every hard frost. For first, in these parts there has been no hard frost this year, not comparable to the last year. Again, those Nottingham trees I wounded in the trunk, and they stood against a brick wall, and the wounds were on the side next it; and besides had horse dung stopped in all of them for some reasons, which things undoubtedly defended them much from the air and winds, and kept the wounds still green and open; whereas the tops of these maple twigs, spoken of in the last experiment, were exposed in an open hedge to the air and winds.

Concerning the bleeding of poles and entire branches held perpendicular, Mr. Willoughby is in the right, and some experiments in my last to you of March 17 confirm it. Yet it is very true what I observed, though the cause I did not then well take notice of, when I first made the experiment and sent you an account of it. For I held the twigs which I had cut off aslant, joining and holding up the cuts together in my left hand, that I might the better observe which part or cut would bleed or not bleed the faster, and because I found that the cut of the separated twig did not in that posture (holding it upwards, as I said, for the advantage of my eye) bleed at all, when as the cut of the branch remaining to the tree did freely bleed; I therefore inverted the separated twig, and held it perpendicular with the cut end downward, and found that the little they were exposed to the air in an upright posture, had so very much checked the motion of the sap, that I concluded they would not bleed at all, and yet striking off their tops, and making poles of them, I found that some of them, if not all, showed moisture; but I am convinced since, that it was rather some unheeded accident, as violently bending them, or perhaps the warmth of my hand and season, or place, which caused this new motion of sap, than merely the striking off their tops.

Some Communications about an early Swarm of Bees. As also concerning Cyder; Descent of Sap; the Season of Transplanting Vegetables. Sent to the Publisher out of Herefordshire by RICHARD REED, *Esq. in a Letter dated March* 14, 1670-1, *at Lugwardine. N°* 70, *p.* 2128.

On Thursday last, the 9th instant, there was at the next house to mine a swarm of bees. It was a very fair day to entice them. I had it from the owner, one Parry, now in my work, and I inquired of him, whether they did not all leave the hive, as sometimes they do unseasonably, either for want of food or out of distaste? he told me, no, but there are as many left behind as came forth. But I conceive that poverty drew them abroad to seek their fortunes; the Infinite Wisdom having imparted such a providence to that little commonwealth, as to send part of their company abroad to shift, before their whole stock of food shall be consumed, to the destruction of them all.

I do commend, for the advancing of cyder in richness both for taste and colour, a new cask, provided it be made of timber very well seasoned, otherwise it may spoil it utterly. I have often tried it, and found that sort of cask to improve cyder.

The best cyder I ever had was red streake grafted on a gennet moyle stock. For as those kinds agree best, and the trees so grafted seldom canker, so the fruit is far milder, and being ripe, both rich and large, and good to eat, and the cyder is smoother, and abates in strength and harshness of that on the crab, and needs less of mellowing before making, the stock in some degree altering and reclaiming the nature of the fruit. For as an apple is best grafted on a crab, which gives acrimony and quickness to the fruit, so a crab (and the red streake is no other) grafted on an apple, receives thence gentleness, softness, size, and an excellent alloy to the sharpness, and (as Mr. Evelyn calls it) the wickedness of the fruit. I have by certain observation found, that crab-stocks grafted with some sorts of fruit, which the soil likes not, they, not the soil, will all canker, not only in the graft but the stock also, which if you graft again upon the former graft, with a fruit liking to the soil, will all heal and so become trees. And it is certain by my observation that twenty pear stocks being wild, grafted young with the same sort of pear, and twenty with another; the roots of each of those of one sort will grow alike; and so those of the other. Generally those that naturally grow high, as the bare-land pear, root deep, and all do so; those whose heads are bushy and thick, as the summer bon-chretien, their roots run wide, and are matted below, and all are so. This diversity of the way of growing of the root must be by grafting, and could not be but by the intercourse of sap, which it receives from the graft, and that cannot be but by the return of the sap.

But in this I desire rather the judgment of others, than give mine own, because it is of a constant use to me, to be well assured herein. For if the sap returns not, then may I prune or lop my trees in any time of the year without loss of sap, which I take to be their blood, and that in which their life consists.

Concerning the season of transplanting, some advise October: but of late years I have never begun to plant till Valentine's day, though a mild and good winter. And I approved of late rather than early planting; and as yet have lost fewer by miscarriage. The cold in the winter kills more than the drought in summer; only the cold does the work, and we impute it to the drought; because they languish until summer from the fatal blow they receive by the cold in winter, and then die. For, either we take our stocks out of woods, or out of nurseries; in either place they lie warm. If you then in October transplant them, you expose them on a sudden to an open air, and adventure them, being weak, to a long and perhaps cold winter; which they cannot bear no more than we can the heat when unused to a voyage beyond the line. I can also relieve them against the drought, by watering and covering the ground, to keep it cool: but there is no fence against the frost; which often gets into the roots and kills, so that they never spring; or, if they do, yet weakly, and die in the spring; or, if they survive, as many do, yet come on very slowly. For the bark cleaves to the wood by reason of the cold, which dries and clings them together, that like a hide-bound horse, they will not admit the sap, which the root would send up; and other suckers grow out at the earth, and the tree grows dry and turns red: all which discovers the obstruction in the receiving the sap, which would come from the root; and then we are forced to score and loosen the bark as we can. Now on the other side, if the summer prove moist, the danger and fear of late setting is over, and they will thrive and come forward first; if otherwise, I seldom see but they always keep green and fresh, being maintained in life and verdure by the sap they receive in the beginning of the spring, before they be transplanted.

In the dead of winter I prune and cut the tree I intend to transplant, as I would have it be, to the end to lose nothing of its strength when I transplant. Then I suffer it to abide untouched by the spade till Valentine's day, and then remove it, after it has taken in somewhat of the spring. This, I think, will cause it to take better and grow better.

In transplanting I am very careful to preserve and set the roots as large as I can, supposing the larger the root, the more of strength and sap it contains, and so will advance the more the growth of the tree; since every thing grows in proportion to the root beneath: but I am doubtful in this, whether I do well or ill, and desire the judgment of others. For I have heard from some planters, that roots cut short do best, as sending forth new roots, which draw sap and nourishment best. And we see that moyles set on slips that have no roots, come to a tree sooner. And I have often observed, that a moyle transplanted after it has taken root does not live so certainly, or thrive so well, as a slip newly set.

Some Observations touching Colours, in order to the Increase of Dyes, and the Fixation of Colours. Imparted by the Author of the four above-mentioned Letters, who annexed them to that of February 15, 1670. *N*°70, *p*. 2132.

Two things, I conceive are chiefly aimed at in the inquiry of colours, the one, to increase the materia tinctoria, and the other to fix, if possible, those colours which we either have already, or shall hereafter discover for use. As to the first, animals and vegetables, besides other natural bodies, may abundantly furnish us. And in both these kinds some colours are apparent, as the various colours of flowers, and the juices of fruits, &c. and the sanies of animals; others are latent, and discovered to us by the effects the several families of salt and other things may have upon them. Concerning the apparent colours of vegetables and animals, and the various effects of different salts in changing them from one colour to another; we have many instances in Mr. Boyle. And if we might, with the permission of that honourable and learned person, range them after our fashion, we should give you at least a new prospect of them, and observe to you the conformity and agreement of the effects of salt on the divers parts of vegetables: viz. 1. That acid salts advance the colours of flowers and berries, that is, according to the experiments of Mr. Boyle, they make the infusions of balaustium or pomegranate flowers, red roses, clove-gilly flowers, mezerion, pease bloom, violets, cyanus flowers, of a fairer red; also the juices of the berries of ligustrum, of black cherries, buckthorn berries, of a much fairer red: and to the same purpose acid salts make no great alterations upon the white flowers of jessamin and snow drops. 2. That urinous salts and alkalies, on the contrary, quite alter and change the colours of the same flowers now named, and the juices of the same berries also, from red to green; even jessamin and snow drops. 3. Again, that in like manner urinous spirits and alkalies advance, at least do not quite spoil the colours of the juices of leaves of vegetables, of their wood and root. Thus Mr. Boyle tells us, that urinous spirits and alkalies make the yellow infusions of madder roots red; of brazil wood, purplish; of lignum nephriticum, blue; the red infusion of logwood, purple; of the leaves of senna, red. 4. That, on the contrary, acid salts quite alter and change the said infusions from red or blue, to yellow.

In the next place, we would note to you the effects of salts on animals in the production and change of colours; but the instances are very few or none that I meet with in any author; the purple fish being quite out of use, and cochineal and kermes are by most questioned, whether they are animals or not; but, I think, we may confidently believe them both to be insects, that is, worms or chrysalids of respective flies in proxima fœtura. We find then, and have tried concerning cochineal (which of itself is red,) that upon the affusion of the oil of vitriol, that is, an acid salt, it strikes the most vivid crimson that can be imagined; and with urinous salts and alkalies, it will be again changed into an obscure colour between a violet and a purple. Pliny somewhere tells us, that the Gauls in his time could dye with vegetables, what the Romans with so much danger and pains sought for in the bottom of the sea. Indeed, we find many plants mentioned by the same author, which either are not now known to us, or at least neglected.

We will now add some of our own considerations and trials. And first, concerning the apparent colours in flowers, we think we may insert; 1. That generally all red, blue and white flowers are immediately, on the affusion of an alkali, changed into a green colour, and then in a short time turned yellow. 2. That all the parts of vegetables, which are green, will in like manner strike a yellow with an alkali. 3. That what flowers are already yellow, are not much changed, if at all, by an alkali or urinous spirit. 4. The blue seed husks of glastum sylvestre long gathered and dry, diluted with water, stain a blue which upon the affusion of lye strikes a green, which green or blue being touched with the oil of vitriol dyes a purple; all these three colours stand. 5. On the tops of fungus tubulosus, so called by Mr. Ray in his late catalogue of the plants of England, are certain red knots; these upon the affusion of lye, will strike a purple, and stand.

As for the latent colours in vegetables and animals; to be discovered to us by the affusion of salts; they likewise, no doubt, are very many. We will set down only a few instances in both kinds, which have not been, that we know of, discovered or taken notice of by others. Latent vegetable colours, 1. The milky juice of lactuca sylvestris costâ spinosa, and sonchus asper et lævis, upon the affusion of lye, will strike a vivid flame colour or crimson, and after some time quite degenerate into a dirty yellow. 2. The milk of cataputia minor, upon the affusion of lye, especially if it be drawn with a knife, and has at any time stood upon the blade of it, will strike a purple or blood red colour, and by and by change into an ignoble yellow. Latent animal dyes, 1. The common hawthorn caterpillar will strike a purple or carnation with lye, and stand. 2. The heads of beetles and pismires, &c. will with lye strike the same carnation colour, and stand. 3. The amber-coloured scolopendra will give with lye a most beautiful and pleasant azure or amethystine, and stand.

Lastly, as to fixing of colours for use; we may use the following obvious inferences, 1. That in all the instances above-mentioned, whether vegetable or animal, there is not one colour truly fixed; however there may, I conceive, be some use made of them, as they are. I say truly fixed, that is, proof of salt and fire; for, what seem to stand and be lye proof, are either wholly destroyed by a different salt, or changed into a much different colour; which must needs prove a stain and blemish, when it shall happen in the use of any of them. 2. That both the apparent and latent colours of vegetables are fixable: an instance whereof we may observe in the seed husks of glastum, and the use dyers make of the leaves after due preparation. 3. It is probable from the same instance, that we may learn from the colour of some part of the fruit or seed, what colour the leaves of any vegetable and the whole plant might be made to yield for our use. 4. That the latent colours of vegetables are pre-existent, and not produced; from the same instance of woad, and likewise from this, that the milky juice of lactuca sylvestris affords itself a red serum. 5. That the change of colours in flowers is gradual and constant. 6. That the colours of flowers, which will not stand with lye, seem to be wholly destroyed by it, and irrecoverable: Thus it happens, in the experiment, that one part of a violet leaf, upon the affusion of lye, is changed very soon into yellow, and will never be revived into a red by an acid salt; but if another part of the same leaf be still green, it will be revived. 7. That the dryness seems to be a means, if not of fixing, yet bringing the vegetable colour into a condition of not wholly and suddenly perishing by the otherwise destroying alkali. 8. That those plants or animals that will strike different and yet vivid colours upon the affusion of different salts, and stand, as the cochineal and glastum, are probably of all others to be reckoned as the best materials.

In the concluding part of this communication, the author observes that he has found out a colour most exquisitely black, and comparable to the best ink, even for the pen, and which will not change by fire or salt.

An Account of some Books. N° 70, p. 2136.

I. Theodori Kerkringii, M. D. Anthropogeniæ Ichnographia, sive Conformatio Fœtus ab ovo usque ad Ossificationis principia, in supplementum Osteogeniæ Fœtuum. Amstelodami, 1671, 4to.

After that this author had the last year published, together with a spicilegium anatomicum, his osteogenia fœtuum (of both which an account was given No. 54, p. 413 of this volume;) in the latter of which he described the formation of the bones of the human body, from the second month after conception to the very time of the infant's birth; he considered, that there were two things yet left behind, necessary to the perfect knowledge of ossification; viz. 1st. What might be the rudiments and form of a human body, before it came to have any firmness of bones. 2dly. How after an infant's being born, the soft bones acquire by little and little both their hardness and magnitude. Waving for the present the latter of these two, he undertakes in these sheets to deliver the first elements, as it were of our body, from and even before the time of conception; affirming,

1. Non tantùm in nuptis et fœcundis mulieribus, sed etiam in virginibus esse non minùs quàm in Gallinis ova ponentibus etiam citra Galli consuetudinem, ova quædam pisi viridis magnitudine, in quibus humor latet intus, qui, uti aliorum ovorum albumen et vitellus, dum coquitur, indurescit: porro, ova illa mulierum, pelliculis extrinsecus circumdari, quæ postquam in uterum prolapsa sunt ova coitû fœcundata, in Amnion et Chorion brevi commutentur; ova autem ipsa, duorum vel trium dierum spatio ad cerasi nigri majoris magnitudinem excrescere.*

2. That he once met with and opened a fœtus of but three, or at most four days old, and found in a little globule that nature had already formed therein the rudiments of a human body, so as that he could plainly distinguish the head from the mass of the body, and see in the head (though but obscurely) several pricked marks for organs; the body in the mean time being nothing but an undigested lump.†

3. That in a fœtus fifteen days old, he could distinguish the eyes, nose, mouth, ears, arms and feet.

4. That in another twenty-one days old, he was able to separate the matter prepared for flesh and skin, from that which was to be hardened into bones. As also, that the head seemed nothing but a membrane distended with wind and spirits; but its arms and hands were figured, and the fingers and toes themselves distinct. Besides, that in that cartilaginous part, designed for the bones, he could number the ribs.

5. That in one of the age of a month, he discovered some more consistency, and the upper and lower jaw bone represented by two bony puncta; besides the claviculæ formed, and almost all the ribs distinct, the shoulder blade, and elbows, thigh bones, and both the leg bones, called focilia; where, by the by, he intimates that a certain acid spirit in the world is the efficient, as of all firmness and solidity, so of that in bones.

6. That, lastly, in a fœtus of six weeks he found that it only differed in size from that of two months, by him formerly described in his Osteogenia; but that the main thing he took notice of in this was the distinction of six small bones in the lower jaw, which after the production of the fœtus into the world do coalesce into one.

All this the author has represented by figures, and endeavoured to confirm by answering what may be objected against his observations.

* † It has been already mentioned, at p. 413 of this volume, that these observations are not to be relied on.

II. Philosophia Veterum, é mente Renati Descartes breviter digesta, ab Antonio le Grand*. Londini, An. 1670, in 12mo.

This epitome of the Cartesian philosophy, digested by the author for the use and advantage of those that have inclination to initiate themselves in the doctrine of that famous philosopher, begins with the main rules, by him esteemed necessary to the acquisition of truth. Thence he proceeds to those simple notions, of which our cogitations are compounded, and concludes this part with a short doctrine of the syllogism.

Having succinctly dispatched this, he passes on to treat of physiology, and exploding the materia prima, the substantial forms, the real accidents, (as these are vulgarly taught) he proceeds to prove, that there are bodies extended in length, breadth and depth, to which belong figure, motion, scite, &c. no otherwise than as some distinct modes. After this, he considers the heavens, earth, water, air and fire, and of what parts they are constituted. Next, he explains the fabric of man; giving an account how he comes to move and have perception. And he closes all with a demonstration of the existence of a God.

III. Traité de Physique, par Jaques Rohault.† A Paris, 1671, in 4to.

After the author has, in this ingenious treatise, assigned the causes why natural philosophy has been steril for so many ages, and found them to be these, viz. the too servile addiction to authority; the resting in metaphysical, abstract, and general speculations; the severing of reason and experience; and the neglect of the mathematics; he divides it into four principal parts.

In the first, he treats of the body natural, and its chief properties, divisibility, motion and rest; as also, of the elements and the sensible qualities; where he insists at large on the explication of the nature and qualities of vision.

In the second part he treats of the system of the world, according to the three celebrated hypotheses of Ptolemy, Copernicus, and Tycho, but gives the preference to the Copernican, as the plainest and the most rational; considering mean while, that, as to the situation of the parts of the universe, Tycho agrees

* Le Grand, a noted Cartesian philosopher, sometimes called the abbreviator of Descartes, was born at Douay in France. He wrote several books on the principles of the Cartesian philosophy. Also notes on Rohault's physics, and other works.

† Rohault, a celebrated Cartesian philosopher, was born at Amiens, 1620. Having made some progress in mathematics, he went to teach that science and philosophy at Paris, where he died in 1674, near 55 years of age. His works are chiefly, Natural Philosophy, as above, in 4to; Elements of Mathematics; A Treatise on Mechanics, published after his death. His attachment to Descartes's philosophy lead him into numerous errors and absurdities, in astronomy, mechanics, and philosophy, as may be seen by the above account of his treatise on physics.

with Copernicus, except that he makes the firmament to have the earth for its centre. He observes further, that the Copernican system, rightly understood, (or rather as he wrongly understood it) attributes no motion at all to the earth; for motion being taken for nothing else than for a successive application of a body to the several parts of the immediately encompassing and neighbouring bodies, what is called the diurnal motion of the earth belongs rather to the mass composed of the earth, the seas, and the air, than to the earth in particular, which is to be esteemed in a perfect rest, forasmuch as she is carried away by the torrent of the matter wherein she swims; just as we say, that a man is at rest that sleeps in a ship, while the ship is indeed in motion; and so that which is called the annual motion of the earth, does not all appertain to her, nor even to the composed mass of earth, water, and air, but to the celestial matter which carries this mass about the sun. After this he discourses of the nature of the stars and their influences. Next he renders an account of gravity and levity, and makes gravity nothing else but a less levity. And lastly, he concludes this part with the doctrine of the flux and reflux of the sea, as depending on the pressure of the moon.

In the third part he explains the nature of the earth, and of the bodies that are either contained in it or are about it, as the air, water, fire, salts, oils, metals, minerals, and meteors. Where, among many other strange remarks, he declares that though the transmutation of baser metals into silver or gold be not absolutely impossible, yet morally it is, forasmuch as men not knowing in particular, what is the figure and size of the little particles that enter into the composition of metals, nor the shape and size of the other ingredients, that may be necessary to effect this transmutation, nor have yet found the secret to unite them together; that therefore it may very well be concluded, that if it be true what is said of some chemists having formerly converted lead into gold, it has happened by so great a chance, as if a handful of sand being let fall from on high upon a table, the grains had so orderly ranged themselves, as to make one read distinctly a page of Virgil's Æneid.

In the fourth he has endeavoured to comprise all that he thinks is hitherto with any certainty known of man; where, as usual, he utters a number of odd and whimsical fancies.

IV. Novæ Hypotheseos de Pulmonum Motu et Respirationis usu Specimen. Londini, 1671, in 8vo.

This anonymous tract contains nothing that requires particular notice.

Some Considerations upon Mr. REED's Letter, printed in N° 70, showing in what Sense the Sap may be said to descend, and to circulate in Plants, and the Graft to communicate with the Stock; as also, what Choice of Apples for the Delicacy of the Liquor in peculiar Seasons, and for easy and speedy Propagation; Pears for some Lands proper; their Choice for manifold Uses, especially for pleasant or for lasting Liquor, and how to be planted and ordered for the best Advantage. The best Season for Transplantation. An Apiary, or Discipline of Bees, recommended for public Benefit. All by Dr. JOHN BEAL, in a Letter to the Publisher, of May 13, 1671. N° 71, p. 2144.

This title and account of contents are all that is necessary to retain of this long and unimportant paper.

Extract of a Latin Letter from Signor MALPIGHI to the Editor, concerning the Structure of the Lungs of Frogs, Tortoises, &c. and the more perfect Animals; also, on the Texture of the Spleen, &c. N° 71, p. 2149. Translated.

After requesting Mr. Oldenburg to convey his thanks to Dr. Thruston for having in his treatise, De Respiratione,* defended his (Malpighi's) opinion concerning the structure of the lungs, he expresses his surprise that the author of the Animadversions† should not have discovered, in his dissection of tortoises, lizards, and frogs, the communication between the bronchia and lungs, which he calls vesicles formed from or arising out of the loose exterior membrane of the lungs (vesiculas à laxitate exterioris pulmonum membranæ obortas); whereas if a pipe be introduced into the trachea, and air be blown into it, the lungs which are appended to the said trachea will become distended in various places about the heart; this distension, indeed, the animal itself can produce at pleasure. Now after the lungs have been thus inflated, if they are tied with a ligature and suffered to become dry, on cutting into them, the cells and vesicles, evidently membranous, will be clearly discerned. And although in frogs the processus or ramification of the bronchia is short, nevertheless the two branches going from the larynx, and composed of several semicircular rings, terminate in membranous vesicles; and thus respiration and expiration are performed. But

* See p. 420 of this vol. of the Abridgement.

† The author of the animadversions or objections here alluded to was Dr. Ent.

in the tortoise, in lizards, and the like, the oblong trachea, divided into two branches, supplies the pulmonary vesicles with air. I know that in frogs there are two vesicles near the mouth, but separate and distinct from the lungs; these oral [or supra-guttural] appendages are every now and then distended by the air driven from the lungs into the cavity of the mouth, during the act of expiration.

The lungs, as above described, are invested with a reticular muscle, of which I have elsewhere rudely delineated the fleshy plexuses, sinuses, and vesicles. The admirable structure of this muscle is particularly conspicuous in frogs and lizards; its numerous longitudinal fleshy fibres are mutually intersected by transverse muscular fibres, and the interstices are occupied by reticular fleshy plexuses much in the same manner as is seen in the leaves of trees. The aforesaid small intermediate spaces are furnished with a set of straight fibres, or, as it were, short tendons. This remarkable muscle is not only spread over the whole surface of the lungs, but also invests all the interior vesicles and sinuses; so that every part of the lungs being compressed by its action, expiration and sound are produced. A similar structure is observed to a certain degree in the lungs of more perfect animals, and is particularly apparent in the extreme lobules of lambs, when those lobules are distended with air, and so long as they continue moist and soft.

The fibres of the spleen, concerning which such various opinions and conjectures have been offered, are not nervous (as, indeed, I myself once imagined) but fleshy [muscular] in such manner, that from the outer fleshy covering, and the fibres that run in a transverse direction, a peculiar muscle is formed, which compresses the cells of the spleen, whereby the blood is propelled into the splenic branch; after a structure and manner not unlike that which we see in the auricles of the heart.

Nature has bestowed upon the testicles of a horse a structure somewhat similar; for their inner coat contains a set of fleshy fibres, or a muscle pervading the meditullium, together with the varicose vessels (unà cum varicosis vasis). These fibres having different directions like those of the spleen, and running crosswise, so as to form a net-work, support and compress the congeries of [seminal tubes, or] canals.*

Bologna, February, 1671.

* There is something of imagination intermingled with these observations of Malpighi, especially in what relates to the spleen and testes. In the latter he seems to have taken portions of cellular membrane for muscular fibres.

Some Observations lately made by certain Missionaries in Upper Egypt; in a Letter written from Cairo, January 6, 1670. By F. Brothai. N° 71, p. 2151.

I have spent three months time in the voyage in Upper Egypt, accompanied with my brothers, Charles and Francis, always ascending on the Nile as high as 300 leagues above this city, being two days journey on this side of the cascade of the Nile, and where no Frenchman has been within the memory of man. I there admired many idol temples yet entire, with very ancient palaces filled with statues and idols. I counted in one place alone 7 obelisks, like those at Rome, and about 120 columns in one hall, of the thickness of five brasses, full within and without, from the top to the bottom with hieroglyphic letters, and with figures of false deities. I found statues of white marble, and some of black, of the size of three persons, with a sword on their side, and of a hard stone; namely, a man and a woman, at the least of the height of eight fathoms, though seated in chairs, but well proportioned; and two others of black marble, representing women, with globes on their head, and extravagant coverings thereon, which were two feet broad from one shoulder to the other.

We lighted only on two places, where antiquities were to be seen, one whereof is called Lozor, and the other Candion, which is a very ancient castle, esteemed by the tradition of the country to have formerly been the residence of a king. In the avenues of the castle are a great number of sphinxes standing in a row, and turning their head towards the alley. This is an idol having the head of a woman and the body of a lion, which was once a famous deity among the Egyptians. They are distant from each other about two paces, and are 20 feet long. I walked in four alleys, ending at four gates of the castle; and for aught I know there may be more of them, as I went but half round the castle, which is very spacious. I reckoned 60 of them on one side of one alley, and as many over against it, and 51 in another alley; all well proportioned. The alleys are of the largeness of a pall-mall; the gates of the castle are of an extraordinary height, covered with most excellent stones. Measuring one, which makes the height of one of them, I found it 26½ feet long, and proportionably thick. I believe that there are above a million of figures, all in profile, none in front; I speak of those that are graven on the walls and pillars. It would have required a whole month to observe all the particulars of that place. I contented myself with drawing only the postures of a dozen of the most extravagant demons, with their adorers of both sexes; and some frontispieces of temples, which are not very rich in architecture, but built of very fine stone. What most pleased me was the ground, where the azure and the other colours, which are like enamel, appear as fresh as if they had been laid on but a month before. There are temples so spacious that 3000 people may stand on the roof with ease. In the same castle there is a pond, the water whereof is bitter, set about with fine stones. This water is said perfectly to whiten linen all alone; which I tried not, but we dipped our handkerchief in it, which kept the scent of soap for four or five days.

These are the only curiosities I can send you of our voyage, which is not the fiftieth part of what was to be seen; but our time being limited we only could stay in two places, our design not being curiosity, but to satisfy the charge of the mission among the Christian Cophts of that country, which are in great number there, and have many monasteries and ancient churches, but poor.

Extract of a Letter from F. Jaques Paul Babin, a Jesuit, concerning the irregular Flux and Reflux of the Euripus. N° 71, p. 2153.

The Euripus is a strait of the Ægean Sea, so narrow, that a galley can scarce pass through it, under a bridge, built between the Citadel and the Donjon of Negropont. Not only this place, where the bridge is, is called the Euripus, but also ten or twelve leagues on each side of it, where the channel being wider, the inconstant course is not so sensible as at the foot of the castle. For three or four leagues on each side there are found six or seven gulfs, wherein this water shuts itself up, to issue from thence as often as it enters there; and the situation of these gulfs contributes to the oddness of this flux and reflux; of which the moon seems to be the principal cause.

There are 20 days of each moon, in which the course of the Euripus is regular, and ten, in which it is irregular; that is, five days before and five days after the new and full moon, the course of it is regular and strong; and then you see there the like phænomena with those of the ocean at Bourdeaux. The sea has two fluxes and refluxes in almost 25 hours. But there are nine or ten changes of the course of the water during the remaining ten days of inequality; unless it blow hard, for then the course changes not above six or seven times. I once stayed on the mill, which is under the bridge, 1½ hour, and I saw the course of the water change thrice, though the wind was pretty high; and the wheels of the mill turned as often divers ways.—In the Euripus the water rises not much above a foot. In the ocean it is observed, that the water in its rising flows into the ports and towards the land, and in its fall runs into the main; but in the Euripus, when the water rises it runs then into the sea, and when it sinks, it flows into the channel going towards Constantinople. The small gulfs, that are on the left side of the port of Negropont, are filled when the water rises; and emptied, running towards Thessalonica or Constantinople, when it descends. I wrote to the F. Vabois, desiring him to observe; and he took notice of the same at Constantinople, viz. that the waters of the Black Sea, that come from Constantinople, drive the Euripus in its rising towards the main sea, and that thereafter the waters retire themselves towards the same place again whence they came. That swelling of the Euripus which is irregular, lasts not above a quarter of an hour, and the sinking three quarters; though then the water ran with more rapidity, and seemed to us to come away in thrice as great plenty, as when it rose.—Between the ascent and descent there is a little interval, wherein the water seems to be at rest and stagnating; so that, if there be no wind stirring it, bits of wood and straw lie still upon the water without motion.

From what I have said, it is not difficult to reconcile the authors that have written so differently of the Euripus. For those who have said that there is nothing in it but what is seen in the ocean, viz. Two fluxes and refluxes in 24 hours, have only observed it in those 20 days of its regularity. And the ancients have not delivered a falsehood, when they say that there are seven reciprocations in one day, because that happens when the winds trouble and retard the course of the water: and I assure you, by often reiterated observations, that when it is still weather, the flux and reflux is made even to nine or ten times in a natural day.

A Relation of two considerable Hurricanes in Northamptonshire. By Mr. JOHN TEMPLER *of Braybrook. N° 71, p. 2156.*

October 30, 1669, between five and six o'clock in the afternoon, the wind being westerly, at Ashley, in Northamptonshire, there happened a formidable hurricane, scarce bearing sixty yards in its breadth, and spending itself in about seven minutes of time. Its first observed assault was on a milk-maid, taking her pail and hat from off her head, and carrying her pail many scores of yards from her, where it lay undiscovered some days. Next it stormed the yard of one Sprigg, in Westthorp, a name of one part of the town, where it blew a waggon body off the axle-trees, breaking the wheels and axle-trees in pieces, and blowing three of the wheels so shattered over a wall: this waggon stood somewhat cross to the passage of the wind. Another waggon of Mr. Salisbury's was driven with great speed on its wheels against the side of his house. A branch of an ash-tree, so large that two stout men could scarce lift it, was blown over Mr. Salisbury's house, without hurting it; and yet this branch was torn from a tree 100 yards distant from the house. A slate was carried near 200 yards, forced upon a window of the house of Samuel Templer, Esq. and very much bent an iron-bar in it. Not to take notice of its stripping of several houses; one thing is remarkable, which is, that at Mr. Maidwell's it forced open a door, breaking the latch; thence passing through the entry, and forcing open the dairy door, it overturned the milk vessels, and blew out three panes or lights in the window: next it mounted the chambers, and blew out nine lights more. It tore off a great part of the roof of the parsonage-house. It blew a gate-post, fixed two feet and a half in the ground, out of the earth, and carried it many yards into the fields.

The other instance was October 13, 1670, at Braybrook, likewise in Northamptonshire, about eleven o'clock; when the wind, in a storm, assaulted a pease-rick in the field, uncovering the thatch of it, without touching another only 20 yards off. Thence it proceeded also to the parsonage, by a narrow current, scarce 8 yards in breadth, blowing up the end of a barley-rick, therewith some stakes in it of near five feet long; without touching a wheat-hovel, within six yards of the barley-rick. It beat down a jack-daw from the rick with that violence as forced the guts out of the body, and made it bleed plentifully at the mouth. Thence it went in a right line to the parsonage-house, and took off the cover of all the house in its compass. From hence it passed over the town without any damage, the rest of the town being low in situation, and went on to a place called Forthill, where it uncovered so much of a malt-house as lay within its line and breadth.

Braybrook stands in a valley, environed by hills on three sides, at three quarters of a mile distance from it. There is also a hill called Clackhill, within a mile of it, and exactly in that point of the compass in which the wind then blew: no other hill in its way till the wind had passed over all the places it damaged. There have also been two earthquakes in this town within these ten years, with little or no wind.

A Narrative of two Petrifications [calculous Concretions] in Human Bodies. Communicated by Mr. Christopher Kirkby, in a Letter from Dantzick, dated April 8, 1671. N° 71, p. 2158.

A woman of 56 years of age, unmarried, whose whole course of life had been extremely sedentary, was troubled, some years before her death, with great pains in her back, especially towards the right side, and a continual vomiting; whose urine, for some time before, was turbid, and as it were mingled with blood; yet totally void of salsuginous matter. She was under the care of the best doctors in this place, who adjudged that symptom of bloody water to have proceeded *ex præmaturâ cessatione mensium* (which left her in the fortieth year of her age;) thereby perhaps deceived, because there was never either stone or gravel voided by her. But her last doctor (from whom I have this relation,) adjudged it to proceed *ab affectû nephritico et quidem gravissimo*. This person, when dead of these distempers, was opened by this her last physican, and among many other common phænomena he found the left kidney filled with large stones, but the right wholly petrified, covered with the ordinary skin without any flesh; the half of which (the other being broken by injurious dissection) representing still the kidney, I have seen, which was both massy and ponderous, so concreted by the closer coalition of minute sand, which might be rubbed off by your finger.

The other was a lad about nineteen years old, who from his cradle was disposed to a consumption, accompanied with continual coughing, great emaciation and continual heat, so that he was reduced to a skeleton, and labouring under this distemper died. Being opened, a great quantity of watery matter run out at the abdomen, of a chylous consistence; almost all the glandules of the mesentery, through which pass the *venæ lacteæ*, were very great, and hardened beyond the hardness of a scirrhus. The breast being opened, the lungs were found grown to it round about, almost inseparable, full of purulent ulcers, but more especially the left side, obstructed and filled with much gravel and small stones; yea, whole pieces of the lungs, especially the extremities, about the thickness of a finger and more, were hardened into a stony matter.

An Account of four Books. N° 71, p. 2159.

I. Francisci de le Boe Sylvii Praxis Medicæ Idea nova. Lugduni Batav. 1671.

This work has been already noticed at p. 289 of this volume of our Abridgement. This is an improved edition.

II. Relatione dello Stato presente dell' Egypto, scritta dal Sig. Gio. Michaele Vanslebio. In Parigi, 1670, in 12mo.

The author observes that the winter of Egypt is as mild as the March air of Rome; and that the usual time of rain is in the months of December, January, and February, and that principally about the sea-coast: That there are tempests from Easter to Whitsuntide, when the wind is for the most part easterly: That the most agreeable weather is in November and December, when the country is dried again from the Nile waters, and every place in verdure, the winds gentle, and the sun tolerable: That the violent heats are in April, May, June, &c. till the inundation of the Nile cools the air; which begins in July and ends in September or October; and proves the great and general manure of that country, when it rises above 16 braccia or Italian ells; which when it stops short of that, the inhabitants are not obliged to pay any tribute to the G. Signior. The cause of this inundation is principally the plenty of rain falling in Abyssinia. The waters of the Nile, being generally esteemed very good, are cleared from their turbidness by bitter almonds beaten and thrown in.

He speaks of the great variety and abundance of birds there; also of the vast number of vegetables; he says the stones of dates are given to camels in long journeys; also, that horses as long as they feed upon trefoil, have no drink given them. Treating of the fossils of Egypt he observes, that their nitre is most abounding in the desert of St. Macare; and that about Thebes there is dug up plenty of marble, porphyry, alabaster, granite, &c.

As to the œconomy of the Egyptians, they do not cut, but pull up their corn, and their corn harvest is from the middle of April to the middle of May; and sometimes even before the middle of April new bread is eaten in Cairo. In villages, for want of ovens, they bake their bread under the hot ashes; and, in making their bread, some put nitre into the dough, to raise and colour it: which must be eaten new, or else it is not good. Among their drinks they have mead, which, though it inebriates, yet they are permitted to drink, though wine be forbidden them: also a very refreshing liquor made of liquorice by the Moors.

III. Theod. Kerckringii, M. D. Commentarius in Currum Triumphalem Antimonii Basil. Valentini,* a se Latinitate donatum. Amstelodami, 1671, in 12mo.

* It is supposed that the person who wrote under the name of Basil Valentine, and who lived at the end of the 15th or beginning of the 16th century, was a Benedictine monk at Erfurt. He was author of several chemical and mineralogical tracts, among which the most celebrated is that which he entitled Currus Triumphalis Antimonii, written originally in German, but afterwards translated into Latin and other languages. A collection of his works was published at Hamburg, in two vols. 12mo. 1740, by which time they had become very scarce. Basil Valentine appears to have been the first who extracted a regulus from the ore of antimony, the process for which he has described, as well as various other preparations of this semi-metal, making chemists acquainted with many of its properties and combinations, before unknown; and, although he was extravagant in his commendations of the medical virtues of antimony, and employed the language of empiricism; yet ought he to be ranked in the number of those who have increased our stock of useful and efficacious remedies. Against chemical preparations, and particularly against preparations of metallic substances, (if we except iron) the strongest prejudices existed at the time when Valentine wrote, and for many years afterwards. Hence at the solicitation of the Parisian physicians (among whom Guy Patin was the most conspicuous) an edict was passed by the parliament of Paris, prohibiting the employment of antimony in medicine. Some years afterwards, this injudicious legal restriction was removed; but it was a long time, indeed, before the physicians of France could get the better of their prejudices, or rather of

This learned physician having carefully perused the antimonial treatise of Valentine, tried all the particulars ordered therein to be done; but says that in the performance he frequently erred, and was at great expences without success, yet not by any fault of the author, but always his own.

Of the many things that are said concerning the usefulness of antimonial preparations, our commentator extols in a very especial manner the red oil of the glass of antimony, of which he teaches the way of preparing it but enigmatically, though he considers that he has done it more clearly than any body declared it to him. This he affirms to be the truly universal medicine, being seasonably and rightly used; alleging (p. 164, 165,) a considerable experiment of his own, made with it, importing, that by the means of this diaphoretic oil alone he cured a young woman of a dropsy in twenty days, making her on the fourth and the following days so to swim in water from sweat, that it dropped at length through the bed upon the floor.

Besides this, he much praises for surgical uses, the balsam or tincture of the sulphur of antimony, affirming upon his credit, that Basil Valentine has not given the full due to its worth; and relating withal (p. 157, 158,) the history of a cure he performed with it upon a cancered breast, that had been under the hands of some of the most expert surgeons, who judged it not curable but by cutting it off; which our author prevented by the use and application of this balsam, whereby within two days the matter was brought to due maturity, and, upon the joint use of some proper internal medicines, the person in the space of two months restored to perfect health.

There is another preparation of antimony here described, and praised above all the rest, called by Basil, his balsam of life, by which he affirms to have cured many that were altogether despaired of, appealing herein to the testimony of his brethren. In which he is seconded by this commentator, who is equally lavish in his praises of this medicine.

IV. Cogitationes Physico-Mechanicæ de Natura Visionis. Auth. Johanne Ott Schaphusa Helvetio. Heidelbergæ, 1670, in 4to.

This author shows himself to be a great admirer of algebra, declaring that whatever he has performed, he has obtained by means of this analysis.

Concerning his dioptrical studies he says, that, remote from other masters and books, by the conduct of the Cartesian analysis he has begun to wind himself, by a long calculus, out of the labyrinth of vision, and by means of equa-

their timidity, in regard to the employment of those active remedies which are derived from the chemical preparations of this and other metallic substances; and which give to the practice of physic, of the present days, a vigour and efficiency which it formerly wanted.

tions discovered divers truths; among which he delivers one, commended both for its newness and usefulness, which is, to contract the longest tubes, without at all prejudicing their perfection, by magnifying so much the angle of vision, that the longest tube shall not perform the like, the lateral rays being so accurately secluded, that more of them shall trouble any of the longest tubes, than those short ones of his contrivance.

An Observation concerning certain Insect Husks of the Kermes Kind. Communicated by Mr. Lister, *May 22,* 1671, *which came to Hand since the Printing of the former Sheets. N° 71, p.* 2165.

I gave you a short account formerly* of certain matrices or insect husks, of the kermes kind, which I had some years since observed on plum trees. This instant May has afforded me the same observation, and some little improvement of it. I have observed the same patellæ or husks indifferently on vine branches, cherry laurel, plum trees, and the cherry tree. The figure of the husks is round, except where it cleaves to the branch, in size somewhat larger than half a grey pea. These cleave to their branches, as patellæ do to rocks; they are of a very dark chesnut colour, extremely smooth, and shining membrane like. They adhere commonly to the under side of a branch or twig, and thus are best secured against the injuries of the weather, both of too much sun and rain. They are well fastened to the branches single, and sometimes many in company. They are seldom found without vermin, as ants, &c. which, I guess, pierce them and prey upon them. If you open one of them, by cutting off dextrously the top of the husk with a rasor, you will find sometimes five or more small white maggots of the wasp or bee kind, sharp at both ends. When these are carefully taken out, you will observe the remainder of their provision of meat, and a partition between them and the branch, where what they secrete is reserved. Lastly, if, when you have cleared the husk of maggots, bee meat and excrements, you then rub the empty membrane on white paper, it will copiously tinge the paper with a beautiful purple or murrey. At the date of this, none of the maggots were yet in nympha, so that you cannot expect from me a description of the bee or wasp when they come to perfection. Few cherry trees, I suppose, in any place, but will yield them some of these berries.

* March 17, 1670-1. I find in my notes (says he) that some years ago I gathered off our English oak round worm husks very like kermes berries, but I then made no trial of them. Again, I have often observed on plum trees and cherry trees, also on the vine and cherry laurel, certain patellæ or flat husks containing worms, which (or at least the husks, for them only I had the opportunity of making the experiment on) will strike a carnation with lye, and stand.

Of a Substance found in great Quantities in some Mines of Italy; out of which is made a Kind of incombustible Skin, Paper, and Candle-wick. N° 72, *p.* 2167.

This account is taken from the third Venetian Journal de Letterati, of March 15, 1671, as follows:

Signor Marco Antonio Castagna, superintendent of some mines in Italy, has found in one of them a great quantity of that lanuginous stone called amianthus, which he knows how to render so tractable and soft, that it resembles a very fine lamb's-skin dressed white. He thickens and thins it to what degree he pleases, and thereby makes it like either a very white skin or paper; each of which resists violent fire. To try this, the skin was covered with live coals, whence it took flame; but being afterwards taken out, the fiery colour presently disappeared, and it became cold and white again as before; the fire only passing through, without waste or alteration; whereas some of the hardest metals reduced to very thin plates, and kept as long in the fire, would cast scales. This skin being made as thin as paper, not only yields that ancient and so much admired amianthus, but also in a more perfect state than that which comes from Cyprus, and not inferior to what sometimes comes from China. Of the same matter this artist has wrought a wick, never to be consumed as long as it is fed, nor altering its quality after the aliment is wasted away.

Some Experiments of Signor Carolo Rinaldini, *of the University of Padua; showing the Difference of Ice made without Air from that which is produced with Air. From the Venetian Journal.* N° 72, *p.* 2169.

A glass-cane was taken, about $1\frac{2}{3}$ Florentine braccia or ell, open at one end, of which above one ell and a quarter was filled with quicksilver, the rest with common water. This open end was shut with a finger, and inverted into a vessel with stagnant mercury; then removing the finger, the mercury began to fall out, so that the aggregate of the quicksilver and water falling, the water remained in the upper part of the inverted cane, now free from air. This being done, the cane was thus exposed to the open air in the month of January, in frosty weather, and in one night the water in it was congealed into ice of a very good consistence. Afterwards Signor Rinaldini, having compared this ice with that which was produced in the open air, found, that the ice in the cane was in substance like hail, that is, an opaque and whitish body; whereas what was made in the air was transparent like crystal. He observed also that the ice made in the cane was heavier in specie than that in the air: which he discovered by putting it into a fluid, which was in specie lighter than water, but heavier than ice made in the open air.

Of a Kind of viviparous Fly; together with some Inquiries about Spiders, and a Table of 33 Sorts to be found in England. By Mr. MARTIN LISTER. *N° 72, p.* 2170.

SIR—I return you thanks for your obliging letter of the third of January, and have sent you the viviparous fly, and the set of inquiries you desire of me. The fly is one of the largest of the harmless tribe that I have met with in England. I call them harmless, because they are without that hard tongue or sting in the mouth, with which the œstrus kind, or gadflies, offend both man and beasts. This fly is striped upon the shoulders grey and black, and chequered on the tail with the same colours: the female may be known by a redness on the very point of the tail. The very latter end of May 1666, I opened several of them, and found two bags of live white worms of a long and round shape, with black heads; they moved both in my hand and in the unopened vesicles, backwards and forwards, as being all disposed in the cells along the body of the female like a sheaf.

This is the only fly I have observed with live and moving worms in the belly of it; yet I guess, we may venture to suspect all of this tribe to be in some measure viviparous.*

Then follow some general inquiries† concerning Spiders, which are omitted,

* The circumstance here described is observed to take place in more than one kind of fly: the eggs, in such species, hatching internally.

† The natural history of spiders was but obscurely known in the days of Lister: it is however so well detailed in the works of Clerk, Roesel, &c. that the queries here proposed may be considered as useless. It may be therefore more proper to give a few of the leading particulars in the œconomy of those insects, than to reprint the above-mentioned inquiries in the works of Dr. Lister.

1. Spiders are hatched from eggs deposited by the parent animals, and are excluded from the egg full formed, and without undergoing any farther change than a gradual increase of size, and greater or less varieties of colour after each period of casting their skins, which are deposited in such a manner as to appear perfectly complete, so as to represent the insect itself, with the difference only of a greater degree of transparency, and a more contracted or shrunk abdomen.

2. In feeding, the spider not only sucks out the juices of its prey, but also occasionally devours or comminutes with its jaws some of the more solid parts. (See Clerk's Aran. Suec.)

3. The glutinous fluid or substance of which the thread or web of spiders is drawn, is contained in a pair of undulated receptacles in the abdomen; and the animal possesses the power of forcibly evacuating or propelling this gluten occasionally to some considerable distance: this is termed the darting or shooting of the thread, and appears to have been known to Aristotle and Democritus.

as not of any importance in the present day. Retaining however the table of the 33 different kinds, as follows:

A Table of the different Species of Spiders in England.

Aranei vel fila mittunt ut sunt qui aut prædandi causa texunt—

Vel reticula orbiculata, N° IX.

1. Araneus subflavus, alvo paululum acuminata inflexaque.

2. Araneus rufus, cruciger, cui utrinque ad superiorem alvi partem velut singula tubercula eminent.

3. Araneus cinereus, pictura clunium in 5 fere partes divulsa, iisque plenis admodum.

4. Araneus flavus, quatuor albis, præter picturam foliaceam, in clune maculis insignitus.

5. Araneus nigricans, clunibus ad similitudinem querni folii pictis.

6. Araneus ex viridi inauratus, alvo prætenui proceráque.

7. Araneus cinereus, sylvarum incola, alvo in mucronem fastigiatâ, seu triquetrâ.

8. Araneus viridis, caudâ nigris punctis supernè notatâ, ipso ano croceo.

9. Araneus pullus, cruciger in alvo plenâ.

Plagas globatas, N° IV.

10. Araneus variegatus, alvo orbiculatâ.

11. Araneus rufus, clunium orbiculatorum fastigio in modum stellæ radiato.

12. Araneus pullus, domesticus.

13. Araneus cinereus macula nigra in summis clunibus insignitus, minimus.

Telas sive linteamina, N° VIII.

14. Araneus subflavus, pilosus, prælongis pedibus, domesticus.

15. Araneus nigricans, prægrandi macula in summis clunibus, cæterùm iisdem obliquè virgatis, domesticus.

16. Araneus fuligineus è Craven, insigni candore distinctus caudâ bifurcâ.

17. Araneus subflavus, nigricantium macularum quadratarum catenâ in clunibus insignitus, item cui utrinque ad clunium latera singulæ obliquæ virgulæ flavescentes.

18. Araneus cinereus, maximus, caudà bifurca.

19. Araneus niger aut castaneus, glaber, clunibus summo candore interstinctis.

By this method spiders are enabled to sail in the air during the autumnal season, and thus produce the appearance called the gossamer, (though that name is also applied to other floating substances, as thistle-down, &c.

4. The thread of spiders in general is so extremely fine, that that of the silkworm may be considered as coarse in comparison. In some exotic species, however, it is said to be of considerable strength.

20. Araneus cinereus, mollis, cui in alvo, obliquè virgatâ, macula latiuscula è nigro rubens.

21. Araneus plerumque lividus, sine ulla pictura, alvo acuminata.

Aut ideo nihil texunt (nisi filorum ejaculatio ac volatus illorsum spectet) cum tamen alias possint: nimirum Telas ad tutandum fætum aut ad hyberna, sed aperto Marte muscas venantur; atque ii sunt

Vel Lupi dicti, N° V. Hi verò cum superioribus singulis octo habent oculos.

22. Araneus subrufus, parvus, citissimo pede.

23. Araneus cancriformis, oculis è viola purpurascentibus, tardipes.

24. Araneus cinereus, alvo undulatim picta, insigniter procera, acuminata.

25. Araneus fuscus, alvo obliquè virgata.

26. Araneus niger, sylvicola.

Phalangia, sive assultim ingredientes, N° III. Hi verò sex tantum oculos habent.

27. Araneus cinereus, sive ex argento nigroque varius.

28. Araneus subflavus, oculis smaragdinis, item cui secundum clunes tres virgulæ croceæ.

29. Araneus subrufus è CRAVEN, sive Ericetorum sive rupium.

Vel omnino nulla fila mittunt, ut sunt qui plerique

Longissimis tenuissimisque pedibus donantur: atque hi duos tantùm oculos habent, telaque sive brachia digitata, N° IV.

*30. Araneus rufus, non cristatus, gregatim vivens.

*31. Araneus cinereus, cristatus.

*32. Araneus è candido nigroque varius, minima bestiola, sylvicola.

33. Araneus, ut puto, coccineus, vulgo dictus a Tant† Anglicè.‡

Extract of a Letter from the same hand, May 30, 1671; concerning an Insect feeding upon Henbane, the horrid Smell of which is in that Creature so qualified thereby, as to become, in some measure, aromatic; together with the Colour yielded by the Eggs of the same, &c. N° 72, p. 2176.

A cimex of the largest size, of a red colour with black spots, is to be found very frequently and plentifully, at least in its season, upon henbane: I have

* These 3 belong to the genus *phalangium.*

† This is not a spider but an *acarus.*

‡ In this table of English spiders, (which however contains some insects not, strictly speaking, of the spider kind,) Dr. Lister has shown a considerable degree of ingenuity. It is therefore retained as a good general outline of the insects of that tribe, so far as observed by himself.

therefore in my private notes entitled it, cimex ruber maculis nigris distinctus super folia hyoscyami frequens. It is observable, that that horrid and strong smell, with which the leaves of this plant affect our nostrils, is very much qualified in this insect, and in some measure aromatic and agreeable, and therefore we may expect, that that dreadful narcosis, so eminent in this plant, may likewise be usefully tempered in this insect; which we refer to trial. About the latter end of May, or sooner, you may find adhering to the upper side of the leaves of this plant, certain oblong orange-coloured eggs, which are the eggs of this insect.

Note 1. These eggs while in the belly of the females are white, and even sometime after they are laid; but as the young ones grow near the time of being hatched, they acquire a deeper colour, and are hatched cimices, and not in the shape of worms.

2. These riper eggs, if crushed on white paper, stain it with as lively a vermilion or *couleur de feu*, as any thing I know in nature; cochineal when assisted with oil of vitriol scarcely excepted.

Some Observations concerning Glow-worms. Communicated by Mr. JOHN TEMPLER, *in a Letter to a Friend of his in London. May* 31, 1671. *N°* 72, *p.* 2177.

The glow-worm being so common an insect, it is unnecessary to detail the remarks made by Mr. Templer in his own words: the chief particulars are, that the glow-worm's light is so very vivid as to be easily perceived through a common pill-box, even when lined with paper. Mr. T. also *persuaded himself* that he perceived a degree of heat from the insect, when shining in its fullest splendor.

An Account of some Books. N° 72, *p.* 2179.

I. Of the Usefulness of Experimental Natural Philosophy, the Second Volume, by the Honourable Robert Boyle, Esq. F.R.S. Oxford, 1671, in 4to.

This illustrious author proceeds in this 2d volume to deliver six very instructive and useful essays.—The first of which contains some general considerations on the means whereby experimental philosophy may become advantageous to human life; not only by bringing improvements both to the trades that minister to the necessities of mankind, and to those that serve for man's accommodation and delight; but also by introducing new ones.—

The second treats of the usefulness of mathematics to natural philosophy; showing that the empire of man may be considerably promoted by the naturalist's skill in those sciences, as well pure as mixed.—The third proves the usefulness of mechanical principles to natural philosophy, showing that the power of man may be much increased by the naturalist's skill in mechanics; forasmuch as nature plays the mechanician, not only in plants and animals and their parts, but in many other curiously contrived bodies.—The fourth manifests, that the good of mankind may be increased by the naturalist's insight into trades: for the proof of which, the author endeavours to show that an insight into trades may improve the naturalist's knowledge; and that the natural philosopher, as well by the skill thus obtained, as by the other parts of his knowledge, may be enabled to improve trades, by suggesting improvements in them.—The fifth shows, that that may be done by physical knowledge, which is wont to require manual skill.—The sixth and last represents man's great ignorance of the uses of natural things; or, that there is scarcely any one thing in nature, whereof the uses to human life are yet thoroughly understood.

II. Enchiridion Metaphysicum, sive de Rebus Incorporeis Dissertatio, per H. M. Cantabrigiensem. Londini, 1671, in 4to.

This book is intended to show the existence and agency of incorporeal beings or substances, and to explain their nature. According to this author, many of the more curious and occult phænomena of nature, which philosophers consider as effects of mechanical causes, are to be ascribed to the immediate agency of immaterial beings or spirits.

After all, he gives his definition of a spirit in general, with its explication; where he undertakes both to make it out, why an extended spirit is more capable of perception, than extended matter: And to show how a spirit, so subtle and penetrative, that it seems not capable of adhering to matter, may yet be conceived able to move and impel matter: And that the cohesion of a spirit with matter is as intelligible, as the union of one part of matter with another.

III. Diophanti* Alexandrini Arithmeticorum Libri sex, et de Numeris Multangulis Liber unus; cum Commentariis C. G. Bacheti, et Observationi-

* Diophantus was a celebrated mathematician of Alexandria; of what æra is not certainly known, some think a century or two before Christ, and others as much after. He lived to a great age, being 84 years old at his death. He is reputed the father of algebra, at least among the Europeans; and was so highly esteemed among the ancients, as to be ranked with Pythagoras and Euclid for mathematical learning. He wrote 13 books on numeral algebra; but only six of them are now extant; of these, the above article is the 3d or last edition. The 2d edition, by Bachet, in Greek and Latin, was in 1621; and the first, by Xylander, in Latin only, was in 1575.

bus D. P. de Fermat Senatoris Tholosani: Cui accessit Doctrinæ Analyticæ Inventum Novum. Tolosæ, 1670, in folio.

The works of Diophantus Alexandrinus concerning numeral algebra or analytics, and figurate numbers, which were formerly published in Greek and Latin by Gasper Bachet, with his commentaries thereon, and some treatises of his own, prefixed and subjoined thereto; are here printed anew with the annotations of that excellent senator of the parliament of Tholouse, M. Fermat; together with some new inventions of his in numeral algebra, and the solution of divers numeral problems, omitted by others: collected out of his private letters by R. P. Jacobus de Billy S. I. All published by M. Fermat, junior.

IV. Rosetum Geometricum, cum Censura brevi Doctrinæ Wallisianæ de Motu, Auth. Thoma Hobbes Malmesburiensi. Londini, 1671, in 4to.

Mr. Hobbes is very angry with Dr. Wallis, for exposing his errors and false reasoning concerning his pretended quadrature of the circle, &c. and the matter is only made worse by his obstinacy, and his ignorance in mathematics.

The book itself treats first of 21 propositions, said by the author to have been attempted hitherto in vain: adding a censure concerning Dr. Wallis's two first parts of motion and mechanics, which has some strictures accusing those treatises of pretended obscurity and vicious definitions.

V. The Prodromus of a Dissertation concerning a Solid contained in a Solid, by Nicolaus Steno. Englished out of Latin. London, 1671, in 8vo.

The author of this curious and learned Prodromus, apprehending that he might be diverted for a great while from finishing his intended main dissertation touching the frame and changes of the earth, and the manner of the productions made therein; thought fit to deliver in this tract both a scheme and a breviate of the same; not only delineating the method he has therein observed, but also sums up the most considerable particulars of his whole design.

He says then, that he has divided that dissertation into four parts. The first is to show, that the question about marine substances, found at a great distance from the sea, is ancient, pleasant and useful, and that though the solution thereof has been hitherto very uncertain, yet he hopes he shall be able to bring it to a certainty. The second resolves this general problem, viz. A natural body of a certain figure being given, to find arguments and marks in the body itself, whereby to detect the place and manner of its production: which problem he affirms to have so resolved, that no sect of philosophers shall find just cause to except against the principles and notions by him supposed for its explication. The third is designed to examine the particular solids included in a solid, according to the laws discovered in the resolution of the general problem. The fourth is to represent the different states or constitutions, (of Tuscany for instance) and proposes a way of explaining the phænomena of the general deluge, not contradicting the laws of natural motions.

The Compression of Air by Water. *N° 73, p.* 2192.

Some members of the Royal Society, with two different sorts of instruments, made experiments for finding the proportions of the compression of air, by or under water, in the month of July, at Sheerness, in the mouth of the river Medway, at the time of high water, where the depth was then about 19 fathoms, and the proportion of the weight of the salt water to that of the same quantity of fresh water, taken out of the river Thames, was as 42 to 41.

One of the instruments was a glass-bottle, that held a quart of water, having a brass ring fastened to its mouth, with a valve or flap, that opened inward, so well fitted, that the bottle being filled more or less with water, none dropped out, though forcibly shaken. This, let down 33 feet into the water with the mouth downwards, and after a little stay drawn up, was found to be so very near half full of water, at several trials, that it was thought fit to state the compression of air at that depth to that measure, which at other depths was found to hold the like proportions answerable to the depths.

The quantity of the compression was known by weighing the bottle with the water in it, after that a forcible depression of the flap had made way for the eruption of the compressed air (which kept it up even when the bottle was placed with the mouth upwards) and then filling the bottle full of the same water, and weighing it again; and lastly by weighing the bottle after the water was all let out; the weight of it being deducted, the first quantity of water weighed just half as much as the second, or so near it that the fraction was not considerable: Whence it was concluded, that the quantity of the air, that filled the bottle before it was immersed in the water, was at the depth of 33 feet, compressed into half the space it took up before, and so proportionably at other depths.

This was confirmed by repeated experiments made with the other instrument; which was a cylinder of glass, about two feet long, close at one end, and having the other end drawn out small by a lamp, and turned down a little way, after the manner expressed in fig. 1. pl. 14. This cylinder was immersed perpendicularly with the crooked end uppermost; by which, as it sunk in the water, the pressure gradually forced in so much water as thrust out the air proportionable to every depth, till the cylinder was so far immersed, that the hole of the crooked part of it was just 33 feet under water; and then it being drawn up, by measuring from the bottom of the cylinder to the height of the hole in the crooked part by a pair of compasses, the water was found to fill the cylinder so near the half, that the difference could not be perceived; and the same held proportionably at other depths.

The proportion of the weight of salt water to that of fresh, was found by weighing some ounces of both in a bottle whereof the weight was exactly known, and which was made with so small a neck, that the addition or diminution of one single drop in it was discernible.

And that these trials may not be thought to have been made out of mere curiosity, they will be found useful for those who have occasion to dive for recovering things lost in water; since, by those experiments, they may beforehand know, when they sink in the diving bell, or other fit instruments, to what depth they can endure the compression of the air for respiration, and how they may furnish themselves with air in a fit vessel for supply.

Extracts of two Letters from Mr. Martin Lister to the Editor, dated June 14, 1671, and July 5, 1671, concerning the Kind of Insect hatched from the English Kermes, formerly taken notice of and described by the same in N° 71, p. 598. N° 73, p. 2196.

June 10, I found several of the patellæ kermiformes hatched in a box, where I had purposely put them. They prove a sort of bees, as I guessed by the figure of the worm, but certainly the least that I ever yet saw of that tribe, as not much exceeding in their whole bulk the half of a pismire. They are very compact and thick, of a coal-black colour. They seem to want neither stings, nor the three balls in a triangle in their forehead, which yet are things to be referred to the testimony of a microscope. What is very remarkable to the naked eye, is a white or straw-coloured large and round spot on the back; of their four wings the upper pair are shaded or dark-spotted, the undermost pair are clear. We may entitle them, according to our custom, Apiculæ nigræ, maculâ super humeros sub-flavescente insignitæ, è patellis sive favis membranaceis, veri kermes similibus, suâque itidem purpurâ tingentibus, cerasi aut rosæ aliarumve arborum virgis adtextis, exclusæ.

Further Observations of the New Star near the Beak of Cygnus, in a Letter from M. Hevelius, at Dantzick. Translated from the Latin. N° 73, p. 2197.

On April 29, 1671, I again observed the new star below the head of Cygnus, and found it in the same place, near the milky way, where I saw it the former year, from June and July to the 14th of October. It then however appeared larger, exceeding the star in the bill of that constellation, as also that in the bend of the lower wing, being indeed nearly equal to the star in the breast, only that its light was duller and more ruddy. It was not seen in December, January, and February; for after the 14th of October, when it ceased to appear, it was sought for in vain, nor could it be seen again before the beginning of March, or perhaps later. I lately measured its distance from some fixed stars, and found it to be 20° 55′ 20″ from the star in the tail of Cygnus, and 17° 47′ 50″ from that in the bend of the upper wing, also 34° 19′ 40″ from that in the head of Serpentarius. So that it still appeared in the same place as formerly.

Another Account of the same Subject, translated from the French Journal des Sçavans, 22 of June, 1671. *N° 73, p.* 2198.

The new star, which Don Anthelme, a Carthusian of Dijon, lately discovered* is one of the rarest appearances observed for some time. As this person contemplated the heavens at night, June 20 of the last year, desirous to discover that admirable star, which has appeared and disappeared twice since the beginning of this century in the constellation of the Swan, viz. in its breast, he perceived near the same constellation a star of the third magnitude, which he had never before observed. He presently announced it to the company which meets in the king's library; and many of them observing it, agreed that there was indeed about the beak of the Swan a new star of the third magnitude, not to be met with in any catalogue of astronomers, although many other neighbouring stars, that are much smaller, be exactly marked by them. It was situate as appears in the fig. 2, pl. 14.

Supposing the obliquity of the ecliptic to be $23\frac{1}{2}$ degrees, the longitude of this star, according to the observation of Mr. Picard, was 1° 55′ of Aquarius; the right ascension 293° 33′; the north latitude 47° 28′; and the declination 26° 33′; it came to the meridian after the star in the beak of the Swan $16^m\ 44^s$; and before the bright star of the Eagle $0^m\ 27^s$. It was distant from the great star of the constellation Lyra 18° 39′ 40″; from the beak of the Swan 3° 47′ 30″; and from the tail of the Swan 20° 54′ 30″.

It is further remarkable, that in the beginning of July† this star was observed to decrease. In the night of July 3, it appeared yet of the third magnitude, but its light was sensibly fainter. In the night of the 11th of the same month

* See No. 65, p. 530, where the time mentioned of the first discovery of this star, differs from that of the relation of this Journal, and is doubtless hence to be corrected.

† Compare No. 66, p. 543.

it scarce appeared of the fourth magnitude. In the night of August 10, it was only of the fifth. And it has ever since decreased still more, so that at last it became so small that it could no more be seen.

And so it has remained for six months without showing itself, till March 17 last, when Dom. Anthelme spied it in the very same place where it was the year before, and found that it was of the fourth magnitude. The assembly that meets in the king's library having notice thereof, several of them observed this star the 2d of April last, and found it in the same place where they had seen it the last year. The 3d of the same month M. Cassini found it greater than the two stars of the third magnitude that are below in the constellation of Lyra, but a little smaller than that in the beak of Cygnus. The 4th of the same month, it appeared to him almost as large and much more radiant than that of the beak of the Swan. On the 9th he found it a little diminished, and almost equal to the greater of the two stars that are below in Lyra. On the 12th it was equal to the least of these two stars. On the 15th he perceived that it increased, and was equal the second time to the greater of these two stars. From the 16th to the 27th it appeared of different magnitudes, being sometimes equal to the larger of these two stars, sometimes equal to the smaller, and now and then between both. But the 27th and 28th it was become as large as the star in the Swan's beak. The 30th it appeared a little clearer. And the first six days in May it was greater. The 15th of May it was seen smaller than the same star. The 16th, it was in bigness between the two stars that are below in Lyra; and ever since it has diminished.

Thus this star has been twice in its greatest splendour: first on the 4th of April, and the second time in the beginning of May: which we read not to have ever happened to any other star. As far as can be judged, from the few observations made of this star, it is likely it is returning about ten months to the same appearance; whereas that in the Whale's neck makes its revolution in eleven months. As for the star in the Swan's breast, we have as yet no certain knowledge of the period of its revolution; yet we may be assured that it takes not less than 14 years to finish it.

The discoveries that have been made in the heavens this last age show, that changes are not so rare there as formerly was believed. If that was true what Pliny says, that Hipparchus, on the occasion of a new star he perceived, made an enumeration of all those which appeared at that time, there would not be any one constellation in which some change were not found since that time, since there are few wherein there be not found more stars now than that astronomer has noted in them. But as the little assurance we have of the exactness of Hipparchus's catalogue, gives reason to believe, that many stars which were not in that catalogue were yet in the heavens; so we may well grant that some of those that have been observed since have not appeared always. For besides the stars that have been seen in the constellation of Cassiopea, in the neck of the Whale, in the breast of the Swan, and in Serpentarius; M. Cassini has discovered many other small ones,* which may very well be presumed to be new. For example, he has observed one of the fourth magnitude and two of the fifth in Cassiopea, where it is certain they were not seen before, many astronomers having exactly reckoned up the very smallest stars of that constellation, and yet not one of them mentioned those three. He has discovered two others, towards the beginning of the constellation of Eridanus, where we were sure they were not about the end of the year 1664, considering that this place of the heavens, where passed the then appearing comet, was diligently beheld by many, who perceived divers other small stars, without observing those two. He has also observed, towards the arctic pole, four of the fifth or sixth magnitude, which astronomers, that always have their eyes upon that place, would not have failed to note, if they had there appeared before.

Nor are we to wonder at it, that we see now more stars in the heavens than there appeared formerly, seeing there appeared some formerly which are seen no more now. For M. Cassini has observed, that the star which Bayer puts near that which he marked in the figure of Ursa Minor, appears no more; that that which is marked A in the figure of Andromeda has also disappeared; that in lieu of that which is marked v at the knee of the same figure, there are two others more northward; and that that which is noted ξ is very much diminished. The star which Tycho places at the extremity of Andromeda's chain, and calls it of the fourth magnitude, is now so small that one can scarce see it; and that which is in his catalogue the 20th of the constellation of Pisces, is now no more seen; unless you will say, that it is gone down lower by four degrees, to the place marked o in Bayer's figure.†

But we are not therefore presently to say, that the stars that have been lately discovered were not in the heavens before, although they were not seen there.

* Compare those, discovered by M. Hevelius, in No. 65, p. 528.

† We may here notice an extract of a letter of April 30, 1670, to the Royal Society from Signor Montanari, professor of mathematics in Bononia. "I might announce to you many new things I have observed in the heavens of late years; but one, which is very remarkable, is, that there have disappeared two stars of the second magnitude, in the stern of the ship, marked β and γ by Bayer, near Canis Major, observed by myself and others, particularly on occasion of the comet in 1664. I know not in what year these disappeared, but this is certain, that from April 10, 1668, I have found no appearance of them; and that others near them of the fourth and fifth magnitude still remain. Many other changes of the stars, even more than a hundred, but less important, I have at times observed, &c."

For as we now know that there are stars which appear and disappear from time to time, so we have cause to suspect, that most of the stars that were not seen formerly, or that are seen no more now, or are found diminished, are of the same nature with the star in the Whale's neck, and do not cease to be in the heavens, though they there appear not.

It is also possible, that these new stars not only were in the heavens, but even appeared there before they were taken notice of as new ones: And it is very probable that it is also with most stars, as with that in the neck of the Whale, which was not observed at first, but when it was already of the third magnitude; although it has been since found, that it is not really so great when it begins to appear, but that being very small in the beginning, it increases insensibly until it come to that magnitude.*

An Answer of Dr. Wallis to Mr. Hobbes's Rosetum Geometricum, in a Letter to a Friend in London, dated July 16, 1671. N° 73, p. 2202.

Further refutations of Mr. Hobbes's pretended quadrature of the circle, and other geometrical errors; now of no use, and not even to be read without the books themselves, from whence the quotations are made.

An Account of some Books. N° 73, p. 2210.

I. De Motionibus a Gravitate Dependentibus Liber Joh. Alphonsi Borelli, in Academ. Pisana Matheseos Professoris: Regio Julio, 1670, in 4to.

The learned author of this book maintains, that all sublunary bodies have gravity: that they exercise this in endeavours to approach towards the centre of the earth: that the superior body, or the superior parts of the same solid or fluid, gravitate on the inferior, when at rest: that there is no positive levity in nature: that lighter bodies ascend, because thrust out of their place by heavier: that the air is heavy, elastic or springy, and thereby performs those things that were wont to be ascribed to fuga vacui; that the same is capable of very great expansion and contraction: that there is not in nature any proper attraction or suction; but things seeming so to be performed, are done by the pulsion or trusion of other bodies: that there is a necessity and a great use of vacuities in nature, notwithstanding the subtle and all-pervading matter of Descartes; with many other things consonant hereto. For these assertions he brings arguments; answers objections and difficulties, and particularly those that are

* In the future volumes of this work, we shall find many other notices of changes in the stars, and even a history of the new ones by Dr. Halley, in volume 29 of the Philosophical Transactions.

alleged to assert a vacuum: And from thence solves a great many phænomena in nature; as, about the Torricellian experiment, and others thereunto appertaining; about siphons, pumps, syringes, cupping-glasses, &c. about the nature of fluidity; (where he examines and animadverts upon the Cartesian doctrine concerning the same:) the ascent of water above its level (in small pipes and otherwise;) and its contracting itself into globular drops: of its expansion in freezing, and its strength thereupon: of the degrees of velocity in lighter bodies ascending in water; and of water running out of tubes or other vessels perforated at the bottom: of fermentations; and dissolving salts, metals, &c. in liquid menstruums: with many more, too numerous to repeat here.

II. Dissertationes duæ Medicæ de Veneno Pestilenti: Studio Caroli de la Font, M.D. et in Acad. Avenion. Prof. primar. Amstelodami, in 12mo.

These dissertations are filled with absurd conjectures and reasonings concerning the nature of pestilential contagion, which the author derives from arsenical exhalations infecting and corrupting the air, &c. &c.

III. Tractatus de Corde, item de Motu et Colore Sanguinis, et Chyli in eum transitu: Cui accessit Dissertatio de Origine Catarrhi. Auth. Richardo Lower, M. D. Editio tertia et novissima. Amstelodami, 1671, in 8vo.

Having already given an account of the principal part of this treatise in N° 45 of these Trans. when it was printed the first time; we shall here only add something about the newly annexed dissertation of the origin and cure of rheums [catarrhs]. As to their origin, the learned author, having declared, with the generality of physicians, that rheums are bred from the serous part of the blood, severed from it by an impeded transpiration, he undertakes to evince the erroneousness of the vulgar opinion, deriving all sorts of defluxions from the brain, by showing, that, whereas the authors and teachers of that tenet do acknowledge, that the water collected in the ventricles of the brain distils only through the os cribriforme into the nose, and through the glandula pituitaria into the palate, the structure of those parts is such, that that can be done neither of these ways; which assertion of his is accompanied with divers considerable observations and experiments; as also with an answer to those that apprehend great danger to the brain from the excrementitious matter gathered therein, if it should not be purged out from thence by the eyes, nostrils, ears, and the palate. Which being dispatched, he proceeds to suggest the ways of stopping and curing defluxions, by observing, that, since the matter for rheums is furnished by the serum of the blood, whatever is able to withdraw that pabulum, or to precipitate the serosity through the kidneys, or to convey it away by siege, or to dispel it through the pores of the body, is sufficient to perform the cure.

IV. Francisci de le Boe Sylvii, M.D. et Prof. Oratio de Affectus Epidemii, An. 1669. Leidam depopulantis, Causis Naturalibus. Lugduni Batavorum, 1670, in 12mo.

In this discourse the author endeavours to prove, that the late wasting disease in the city of Leyden is to be imputed to the excessive heat, long continued calms, want of rain, and the vapours of standing and muddy waters abounding in that place, together with certain saline and noxious exhalations of the earth, by the force of the sun propelled into the air, and there mingled with the former. From which he considers, that all the various, and even the most different and grievous symptoms, that were observed in the sick people at Leyden may be rationally derived; adding his opinion of the cause why rich and delicate persons were first of all and sooner attacked and destroyed by that disease, than those of the poorer and hardier sort of people; though these latter fell in greater numbers about the end of this sickness, than the former.

In the discourse about the ill effects of a tainted air upon human bodies, he takes occasion to insinuate, that as it is difficult to prove, so it is hard to deny, that some part of the inspired air is also commixed with the saliva, and being (together with other humours, falling from the brain and its glandules and the glandulous tunicles,) derived to the mouth and throat, and so swallowed together with the same, causes in the stomach and small guts some alterations in the humours there found or meeting together. But as he thinks this not improbable, so he judges that there are yet required many accurate observations to clear up and establish so obscure a doctrine.

V. Hypothesis Physica nova, sive Theoria Motus Concreti, una cum Theoria Motus Abstracti. Auth. Gothofredo Guilielmo Leibnitio,* J. V. D. et Consiliario Moguntino. Londini, 1671, in 12mo.

* Godfrey William Leibnitz, a philosopher and mathematician of the first rank, was born 1646 at Leipsic in Germany, his father being professor of moral philosophy in that university. He had an early predilection for mathematics and philosophy, and began the study of the sciences at 15 years of age. Indeed his reading and course of study were extensive and universal, but that of the law was pursued as a professional one, in which faculty he was admitted bachelor in 1665 at Jena, and doctor the year following at Altorf. In 1673 he visited Paris, where he cultivated the acquaintance of the literati, and improved himself by the study of the mathematical writings of Pascal, St. Vincent, Huygens, &c. In 1673 he visited England, and became acquainted with the learned men of the Royal Society, particularly Mr. John Collins, from whom it seems Leibnitz received some hints of the method of fluxions, which had been invented by Newton in 1664 or 1665. The same year he returned to France, where he resided till 1676, when he again passed through England and Holland, in his journey to Hanover, where he proposed to settle. In 1700 he was admitted member of the Royal Academy of Sciences at Paris. The same year, by his advice, was founded an academy at Berlin, of which he was appointed perpetual president. He also projected an academy of the same

The ingenious author of this small tract, though by profession a Civilian, and one of the Privy Council of his Electoral Highness of Mentz, and on that account much taken up with public affairs, is yet so much pleased with the study and search of nature, that whatever hours he can redeem from his state employment, he spends in that which he judges incumbent on him as man, viz. the contemplation of the works of God and improvement of natural philosophy. What he has therein performed, he imparts in this hypothesis, and dedicates it to the Royal Society of England, and the Royal Academy of France, desirous in his letters to have their thoughts concerning the same; wherein he shows, that by the help of it the causes of most of the phænomena of nature may be rendered from one single and universal motion, supposed in our globe, neither thwarting the Copernican nor Tychonian hypothesis; the author having so managed the whole, as that all sects may bear and admit what he here produces without prejudice to their own opinions.

VI. Philosophus Autodidactus, exhibitus in Epistola, ex Arabica in Latinam Linguam versa ab Edvardo Pocokio,* Oxonii, 1671, in 4to.

kind at Dresden, a design which was prevented by the troubles in Poland. Indeed his projects were very general and extensive: he had one for a universal language; another for the general government of all Europe, under one power, the Emperor of Germany, for civil affairs, and under one head, the Pope, for ecclesiastical concerns, though he himself was a protestant. Leibnitz received many favours from different princes: he was counsellor to the Electors of Mentz and Hanover, aulic counsellor to the Emperor, and privy counsellor to the Czar, with several pensions from them. His patron the Elector of Hanover having been raised to the throne of Great Britain, Leibnitz again visited England in 1714, where he was treated with great distinction. Soon after this he had a long and interesting contest with Dr. Samuel Clarke, on the subjects of freewill, space, &c. which terminated only with his life in 1716, at 70 years of age.

Leibnitz was certainly a man of first-rank genius and learning, in all branches; but he has not been more remarkable for any thing, than on account of his competition with Newton for the discovery of fluxions, the first specimen of which was published by Leibnitz in the Leipsic Acts, 1684. As to his person and character, he was of a middle stature, and a thin habit of body: he had a studious air, and a sweet aspect, though near sighted: of indefatigable industry: he eat and drank little; hunger alone marking the time of his meals; and then his diet was plain and strong. He was never married; and was used to say, that "marriage is a good thing, but a wise man ought to consider of it all his life." His writings, which were very numerous and various, have been published in several different forms and places, but were all collected in 6 large 4to volumes, by M. Dutens, and published at Geneva, n 1768.

* Edward Pococke, a learned English divine, was born at Oxford in 1604, where he was educated. Having acquired considerable knowledge in the oriental languages, he was sent out as chaplain to the English factory at Aleppo in 1630, where he had an opportunity of perfecting himself in the Arabic, Ethiopic, and Syriac languages, of which last he wrote a grammar, and was appointed reader of the Arabic lectures, 1636, just founded by archbishop Laud. He afterwards made a second voyage to the east, to collect MSS. for that prelate. After a residence of four years at Constantinople, he re-

This book being translated from a fair Arabic manuscript, in the Pocockian library, into Latin by the care of the learned Dr. Pococke, and printed in both the languages together, is a very ingenious and elegant piece. The design is to show, how from the contemplation of things here below, man by the right use of his reason may raise himself unto the knowledge of higher things; which is here performed by a feigned history of an infant exposed, he knows not how, on an island not inhabited; where he was nursed up by a gazel, or kind of wild deer, and coming afterwards to years of knowledge, did by his single use of reason and experience, without any human converse, attain the understanding, first of common things, as, the necessaries of human life; how to shift among the beasts for his food, &c.; the use of cloaths, of weapons, to keep the beasts in order, who were before too hard for him; then to the knowledge of natural things, of moral, of divine, &c. And afterwards by an accident coming to know that there were other men in the world beside himself, and being removed out of his island to them, and having learned the language, was found to excel their studied philosophers.

The whole design handsomely laid, and ingeniously prosecuted. The epistle written by Abi Jaafar, contemporary of Averroes, who lived about 500 years ago; at which time, it seems, it was already known, that the countries in the Torrid Zone were habitable, as appears by the preface of Dr. Pococke to the reader.

An Intimation of divers Philosophical Particulars, now undertaken and considered by several ingenious and learned Men. N° 74, p. 2216.

First, we learn that at Paris the excellent Signior Cassini has lately again detected spots in the sun, of which none have been seen these many years, that we know of. The last observation in England of any solar spots, was made by Mr. Boyle, as follows, viz. Friday, April 27, 1660, there appeared a spot in the lower limb of the sun, which was entered about $\frac{1}{40}$ of the diameter of the sun, itself being about $\frac{1}{160}$, in its shortest diameter, of that of the sun; its longest about $\frac{1}{40}$ of the same. It disappeared on Wednesday, May 9, though we saw it the day before, about 10 in the morning, to be near about the same distance from the westward limb, that it first appeared to be from the eastward limb. It seemed to move faster in the middle of the sun than to-

turned to England, and was presented to the living of Childrey in Berkshire. In 1648 he was appointed Hebrew professor at Oxford. After a long and useful life, of 87 years, having published a number of valuable works, he died at Oxford, the 10th of September, 1691.

wards the limb. It was a very dark spot, almost of a quadrangular form, and was inclosed round with a kind of duskish cloud.

We first observed this same spot both for figure, colour, and bulk, to be re-entered the sun May 25, when it seemed to be in a part of the same line it had formerly traced; and was entered about $\frac{4}{33}$ of its diameter. At the same time there appeared another spot, which was just entered, and appeared to be entered not above $\frac{1}{132}$ part of the sun's diameter. It appeared to be longest towards the north and south, and shortest towards the east and west. There seemed to be dispersed about it divers small clouds here and there. Discoursing of the thoughts he had entertained touching the effects of such spots, he suggested this inquiry, whether they might not cause a considerable alteration both in the body of the sun itself, and in our air, and the bodies in it, upon their dissipation.

Secondly, At Paris the royal observatory, now building for making celestial discoveries, is very far advanced, and will shortly be in a condition to be employed for the use intended; whence we may expect a considerable advancement of the astronomical science. Beneath the edifice there is a very deep cave, having 170 steps of descent; wherein many sorts of experiments are intended to be made, of such a nature as require to be remote from the sun beams and the open air; such as are thermometrical ones, and such as concern refrigerations, coagulations, indurations, and conservations of bodies, &c.

Thirdly, M. Mariotte is publishing two very useful discourses; the one on vision, on which subject he has discovered something new and considerable: the other of the art of levelling, containing many remarkable particulars about refraction, and the errors they occasion; with several new instruments for levelling exactly.

Fourthly, By letters from Germany we find that the learned physician Dr. Kornmann is printing a book, concerning the tinctures or essences of the excretions of insects, which having fed upon several herbs and flowers, yield such dungs, wherein the tincture, colour and virtue of these vegetables are to be found: thus for example, he can extract a curiously red tincture out of excrements of worms that have fed upon roses, &c.

Fifthly, That a very ingenious person in Italy offers it to the consideration of naturalists, whether it be likely to find a place in vegetables, whence the sap may part, and whether it may return, such as is the heart in animals; adding, that whereas vegetables are always to put forth new branches, leaves, &c. it seems to be sufficient for them, that there be a continual and plentiful course and supply of juice, to thrust out every way, without any necessity of such a circulation.

Sixthly, As it has been more than once mentioned in these papers,* Mulierum testes esse ovario analogos; nec tantum in nuptis et fœcundis mulieribus, sed etiam in virginibus esse ova vera, &c. we cannot but signify here, for further inquiry, that there has been very lately made by two physicians at Paris a dissection of a cow, in cujus testiculis ova reperta fuerint, uti Kerkringius observasse se scripserat in anthropogeniæ suæ Ichnographia.

Seventhly, from Germany we are informed, that in the university of Jena in Upper Saxony, one Mr. Weighelius, professor of the mathematics there, has invented several ingenious instruments and engines; as first, an astronomical one, which he calls astrodicticum, by the means of which many persons may, at one and the same time, behold one and the same star. Secondly, An exceeding large globe of the world, capable of ten persons sitting in it at once, and to behold the motions of the celestial bodies, &c. Thirdly, An odd bridge, or a kind of stairs, by which a man shall descend, and yet really be raised upward, and going as it were upon a plain, shall, from a lower, by gently subsiding, arrive to an upper story, &c.

The Extract of a Letter written by Mr. John Ray, to the Editor, from Middleton, July 3, 1671, concerning Spontaneous Generation; as also some Insects smelling of Musk. N° 74, p. 2219.

Whether there be any spontaneous or anomalous generation of animals, as has been the constant opinion of naturalists heretofore, I think there is good reason to question. It seems to me at present most probable, that there is no such thing; but that even all insects are the natural issue of parents of the same species with themselves. F. Redi has gone a good way in proving this, having cleared the point concerning generation ex materia putrida. But still there remain two great difficulties. The first is, to give an account of the production of insects bred in the by-fruits and excrescencies† of vegetables, which the said Redi doubts not to ascribe to the vegetative soul of the plant that yields those excrescencies. But for this I refer you to Mr. Lister. The second, to render an account of insects bred in the bodies of other animals. I hope shortly to be able to give you an account of the generation of some of those insects which have been thought to be spontaneous, and which seem as unlikely as any to be after the ordinary and usual way.

Of such an insect as you mention, feeding upon ranunculus, which when

* See Number 34, p. 242, and Number 70, p. 586.

† The natural history of insects of the genera of *Cynips* et *Ichneumon* being at that time but very obscurely understood may well account for the opinion of Redi.

dried yields a musky scent, I have no knowledge. I can at present call to mind but two sorts of insects that I have seen, which smell of musk. The one is like the common capricornus * or goat-chafer, which is mentioned by all naturalists that write of insects, and which smells so strong of that perfume, that you may scent it at a good distance as it flies by, or sits near you. The other is a small sort of bee, which in the south and east parts of England is frequently to be met withal in gardens among flowers in spring time. I remember, they were very plentiful in Sir Edw. Duke's tulip garden, when the tulips flowered. Sir Edward is now dead; his house was not far from Saxmundham in Suffolk; the name of the parish I have forgotten.

Another Extract of a Letter written from Middleton in Warwickshire, to the Publisher July 10th, by FRANCIS WILLOUGHBY, *Esquire; about the Hatching of a Kind of Bee, lodged in old Willows. N° 74, p. 2221.*

This bee has been sufficiently described in the note on Willoughby's preceding paper on the same subject. See p. 533.

A further Account of the Stellar Fish, formerly described in N° 57, p. 422, of this Abridgement; with the Addition of some other Curiosities. N° 74, p. 2221.

Of no importance.

An Extract from the 3d and 7th Venetian Giornale de Letterati, concerning the Formation of Fœtuses. N° 74, p. 2224.

There is nothing in this extract worthy of preservation.

Dr. WALLIS'S *Opinion concerning the Hypothesis Physica Nova of Dr.* LEIBNITZ, *announced in N° 73. N° 74, p. 2227.*

In examining this work, Dr. Wallis finds many things to commend, as agreeing with his own opinions in philosophical matters, explained in his writings. Such as, that all physical matters ought to be adapted and accommodated as much as possible to the mechanical ways of reasoning: no body can of itself return again in the same line in which it has moved, unless by means of a new force added: that all bodies, at least hard ones, are sensibly elastic; and hence results reflection: also that a heavy body is not raised by the dread

* The Cerambyx moschatus. Lin.

of a vacuum, but because of the equilibrium of the atmosphere: that levity is only an accident attending gravity, the less weight being forced up by the greater: that the rushing of air, or other fluids, into an exhausted vessel, is caused by its gravity or elasticity: also that exhaustion and distension being effected, hence result fermentations, deflagrations, and all kinds of explosions: that the gravity in the lower arises from the motion (or rather pressure) in the superior æther.

Objections however are brought against his theory or hypothesis. Dr. Wallis observes, that the basis of the new hypothesis is derived from his abstract theory of motion, namely that no cohesion arises from quiescence or rest, but that all consistence or cohesion comes from motion (which accords with the opinion of Wm. Neil.) But Mr. Boyle, with others, is of a contrary opinion, who holds that consistence arises from the quiescence of the particles, and fluidity from their continual motion. Others refer it to the various figures and hooked entanglings of the atoms. The doctor however declines to pronounce positively on this head, but leaves the matter to be cleared up hereafter by further examinations; acknowledging that many things in the theory of abstract motion meet his approbation, being treated with subtlety and solidity, and meriting the consideration of the learned.

An Account of some Books. N° 74, p. 2231.

I. A discourse touching the Original of Human Literature, both Philology and Philosophy; in two parts: by Theoph. Gale, M. A. Oxford, 1669 and 1671, in 4to.

The business of this book is, to derive human arts and sciences from the Jewish Church; for the doing of which the author professes he has been encouraged by considerable hints and assistances of the Scaligers, and of Grotius, Vossius, Bochart, Selden, Usher, Preston, and others, besides the concurrent testimonies of many of the ancients.

In the first part he endeavours to prove, that all languages have their origin and rise from the Hebrew; instancing particularly in the oriental tongues, as the Phenician, Coptic, Chaldean, Syriac, Arabic, Persian, Samaritan and Ethiopic; and then in the European, especially the Greek, Latin, the old Gallic and Britannic. In the second part he endeavours to evince, that philosophy also had its origin from the Jewish Church; beginning to show this of the Barbaric philosophy, under which he comprehends the Egyptian, Phenician, Chaldean, Persian, Indian, Ethiopic, Scythian, and Britannic; and thence proceeding to the Grecian, and chiefly to the Ionic, and Italic or Pythagorean; where he shows great reading and learning.

II. Joh. Joachimi Becheri * Spirensis Med. Doct. Experimentum Chymicum Novum, quo Artificialis et Instantanea Metallorum Generatio et Transmutatio ad oculum demonstratur. Francofurti, 1671, in 8vo.

This tract was written by the author as a supplement to his Physica Subterranea, the first part of which was printed at Frankfort about two years since, and which undertakes to explain both the abstruse generation of subterraneous things, and the admirable fabric of the superterraneous and subterraneous complex globe of earth, air and water: promising to deliver hereafter, in the second book, the particular nature of under ground bodies, and to teach the resolution of them into parts, and the proprieties of those parts; together with an appendix that shall contain a great number of chemical mixtures, never seen before, and grounded upon numerous experiments. We cannot forbear giving the reader one very considerable experiment, said to have been actually made by the author himself, viz.

Having a mind to melt a jasper, he put it into a crucible, and actually melted it by an intense fire, and some other requisites necessary to the operation. But to the end that no coals might fall into the paste, he covered and luted the crucible, which was about half filled with jasper stone; which being now melted, he opened the crucible when cool, and, to his great wonder, found at the bottom the jasper melted together into one mass,

* Many of the circumstances in the life of John Joachim Becher were remarkable. He was a self-taught man, endued with a quick understanding and capable of great application; but he was of a haughty and violent temper, which proved a source of frequent vexation and disappointment. He was born at Spire in 1635. His parents having lost most of their property in the religious and political troubles with which Germany was at that time afflicted, he found himself under the necessity, when he was only 13 years of age, of providing a subsistence not only for himself, but for his mother and two brothers also. This, by his talents and industry, he was able to effect. In 1660 he obtained a medical professorship in the university of Mentz, and was moreover appointed physician to the elector of that bishoprick. As his studies and writings had been directed to other subjects besides medicine, he was recommended to the Emperor as councellor to the chamber of commerce at Vienna, whither he removed in 1666, in consequence of his appointment to that office; but not long afterwards disagreeing with some of his colleagues, they intrigued against him at the Imperial court, and he found himself under the necessity of resigning this situation. He then removed to other parts of Germany, and from thence to Holland; from which country he proceeded in 1680 to England. Here he terminated a life of restlessness and disappointment in 1682, when only 47 years of age. The catalogue of Becher's works on medical, chemical, mechanical, commercial and philosophical subjects, is too extended for insertion here. He is said to have invented an universal character, to have suggested an improved and easy method of teaching languages, and to have thrown out many useful hints relative to metallurgy, mechanics and other arts. One of his tracts is characteristic in many respects of the writer's eccentricity; it is entitled Foolish Wisdom and Wise Folly, and is a curious and entertaining miscellaneous performance, at that time much admired in Germany; but the works by which this author acquired most of his celebrity, are his Institutiones Chemicæ and his Physica Subterranea.

as hard as before, but milk white and half opaque, resembling a natural white agate; but the cover, and the upper parts of the crucible, that were unfilled, and could not be touched by the jasper in the melting, were tinged with the natural colour of the jasper; insomuch that if there had been the hardness of a jasper, and the colour not superficial only, the fragments of the crucible might have been sold for the best and most polished jasper, having here and there greenish streaks and specks, the rest being red and yellowish; all so beautiful, that a good painter would scarcely have been able to imitate those various colours. The author says, he keeps still the pieces in his laboratory at Munchen in Bavaria, as a very extraordinary treasure; esteeming that those upper parts were tinged by the anima of the jasper, driven up by the force of the fire from its inferior part, and adhering to the body of the crucible.

We shall farther notice that chemical experiment which gives the title to the book, and is called new, alleged to prove the real and sudden generation and transmutation of metals. He took common brick earth, dried it in the air that it might be sifted; then poured so much linseed oil upon it as that he might roll it into little balls, of the size of the retort's neck, which they were to be put into, to the end, that the distillation being made, he needed not to break his retort for the taking out of the caput mortuum, but might reserve it for other use. That the fire might the more forcibly penetrate those globules, than if the matter were in one mass, he filled the retort with them, and by degrees distilled them with an open fire, during an hour or two. This distillation being finished, he found in the recipient an oil almost like that which he says is improperly called oleum philosophorum; then the retort being cooled, he took the little balls out of it, which not being found red by so strong a fire, but very black, he suspected that blackness proceeded from the oil, some terrestrial parts of which, being fixed and severed by virtue of the brick earth, might there have assumed a body; which of what kind it was, was now further to be examined by trial. Having therefore beaten small these black globules, and sifted them, he put them into a dish, and having poured some common water upon it, he stirred it; then being grown turbid, he gently poured it off, and poured on fresh clear water, still stirring the matter; which he so often repeated and continued, till the water came clear away, and there remained at the bottom of the dish a ponderous black sediment, which from its weight and sudden subsidence, as also from its dark colour, he suspected to be of a metallic, and indeed of an iron nature; which being dried on paper, upon the application of a loadstone, was thereby attracted in several grains, which by all proofs he found to be very good iron.

Esteeming hence, that sulphureous spirits may be fixed in earth as their matrix, he says, that he employed the same method with all minerals, sulphurs, and mercury itself, and accordingly melted various minerals with various earths and clays; whereby he discovered many truths and transmutations.

III. De Absynthio Analecta, per Joh. Michael Fehr, M. D. Lipsiæ, 1668, in 8vo.

A vast deal too much about wormwood.

Crocologia. Auth. Joh. Ferdinando Hertodt, M. D. Jenæ, 1671, in 8vo.

In like manner a vast deal too much about saffron.

IV. De Laudano Opiato, Auth. Matth. Tillingio, M. D. Francofurti, 1671, in 8vo.

A treatise on opium, superseded by the writings of modern pharmacologists.

A Supplement to what was published N° 73, on the Compression of Air under Water. N° 75, p. 2239.

At the desire of a particular friend, the scheme in fig. 3, pl. 14, was drawn, and is now permitted to be made public at the request of another, by way of supplement to what was said in the Philosophical Transactions, N° 73, of the compression of air under water, in which figure, ED is made to represent the tube $=x$; AB the distance of the upper part of the tube from the surface of the water, above or under it, $=b$; FC the depth of the water from its surface to the bottom of the air within the tube, $=a$; BC that part of it which remains filled with air within the water; CD the rest thereof which is full of water.

Then, any two of the first three x, b, and a, being given, the other is known, and consequently the rest also. For, if by the incumbent weight of 33 feet depth in water, the air in the tube is compressed into half the space it filled before; then the said 33 feet depth of water equals the weight or pressure of the incumbent air on the surface of the water.

Now as the weight or pressure of the air on the surface of the water, is to the depth of the water from the surface thereof to the bottom of the air within the tube, so is the length of the tube filled with air, to the length thereof filled with water; that is, according to the said experiments, putting z for 33, or whatever at other times or places shall be found to be the weight or pressure of the incumbent air on the surface of the water, for it is not always the same exactly;

Then $z . a :: a \pm b . \frac{a^2 + ab}{z} = CD$.

And therefore $\frac{a^2 + ab + za \pm zb}{z} = x$.

Wherefore $\frac{zx}{z + a} - a = b$.

And $\frac{\sqrt{: b^2 \mp 2zb + z^2 + 4zx :} \mp b - z}{2} = a$.

And therefore a and b being given, x is known by the first equation; and a and x being given, b is known by the second; and b and x being given, a is known by the third.

The horizontal line BFBAF is substituted for GABEFb, when the close end of the tube is not even with the surface of the water, to avoid the breach $cC = bB = \frac{b^2}{4z}$ in the length of the tube.

Note. That the perpendicular immersion of the tube or cylinder, spoken of in N° 73, is not to be understood of the depth of the bottom or open end in the water, but of the depth of the air within the tube or cylinder from the surface of the water, viz. FC, not FD.

*An Answer to four Papers of Mr. Hobbes, lately published in the Months of August and this present September, 1671. N° 75, p. 2241.**

In the former Part of his first Paper.

By reason of a proposition of Dr. Wallis (prop. 1, cap. 5, De Motu) to this purpose, for he does not repeat it verbatim; if there be supposed a row of quantities infinitely many, increasing according to the natural order of numbers, 1, 2, 3, &c. or their squares, 1, 4, 9, &c. or their cubes, 1, 8, 27, &c. whereof the last is given; it will be to a row of as many, equal to the last, in the first case, as 1 to 2; in the second case, as 1 to 3; in the third, as 1 to 4, &c.; where all that is affirmed is but, If we suppose that, this will follow. Which consequence Mr. Hobbes does not deny; and therefore all that he says to it is but cavilling.

Mr. Hobbes moves these questions, and proposes them to the Royal Society to pass a judgment on them. 1. Whether there can be understood (he should rather have said, supposed) an infinite row of quantities, whereof the last can be given? 2. Whether a finite quantity can be divided into an infinite number of lesser quantities, or a finite quantity consist of an infinite number of parts? 3. Whether there be any quantity greater than infinite? 4. Whether there be

* We think it necessary to give this paper (evidently composed by Dr. Wallis) at full length, as it contains a good deal of pleasing information on a very dry subject; containing the sum and substance, and indeed closing the long and tedious dispute with Mr. Hobbes, relating to his pretended quadratures of the circle, which he so obstinately and perversely defended to the last.

any finite magnitude of which there is no centre of gravity? 5. Whether there be any number infinite? 6. Whether the arithmetic of infinites be of any use for the confirming or confuting any doctrine?

For answer, in general, I say, 1. Whether those things be or be not; yea, whether they can or cannot be; the proposition is not at all concerned, which affirms nothing either way; but whether they can be supposed, or made the supposition, in a conditional proposition. As when I say, if Mr. Hobbes were a mathematician, he would argue otherwise; I do not affirm that either he is, or ever was, or will be such. I only say, upon supposition, if he were what he is not, he would not do as he does. 2. Many of these quæries have nothing to do with the proposition; for it has not one word concerning gravity or centre of gravity, or greater than infinite. 3. That usually in Euclid, and all after him, by infinite is meant only more than any assignable finite, though not absolutely infinite, or the greatest possible. 4. Nor do they mean, when infinites are proposed, that they should actually be, or be possible to be performed; but only, that they be supposed; it being usual with them, upon supposition of things impossible, to infer useful truths. And Euclid in his second postulate, requiring the producing a straight line infinitely, either way, did not mean that it should be actually performed; for it is not possible for any man to produce a straight line infinitely, but only that it be supposed. And if AB, pl. 14, fig. 4, be supposed so produced, though but one way, its length must be supposed to become infinite, or more than any finite length assignable; for, if but finite, a finite production would serve. But, if so produced both ways, it will be yet greater, that is, greater than that infinite, or greater than was necessary to make it more than any finite length assignable. And whoever does thus suppose infinites, must consequently suppose one infinite greater than another. Again, when by Euclid's 10th proposition, the same AB, fig. 5, may be bisected in M, and each of the halves in *m*, and so onwards, infinitely; it is not his meaning, when such continual section is proposed, that it should be actually done, (for who can do it?) but that it be supposed. And upon such supposed section infinitely continued, the parts must be supposed infinitely many; for no finite number of parts would suffice for infinite sections. And if further, the same AB so divided, be supposed the side of a triangle ABC, fig. 6, and from each point of division supposed lines, as mc, Mc, &c. parallel to BC; these parallels, reckoning downward from A to BC, must consequently be supposed infinitely many; and those in arithmetical progression, as 1, 2, 3, &c. each exceeding its antecedent as much as that exceeds the next before it; and whereof the last (BC) is given; and their squares, as 1, 4, 9, &c. their cubes, as 1, 8, 27, &c. And this I say, to show that the supposition of infinite, with these attendants, is not so new, or so peculiar to Cavallerius or Dr. Wallis, but that

Euclid admits it, and all mathematicians with him, as at least supposable, whether possible or not.

In particular, therefore, to his queries I answer, 1. There may be supposed a row of quantities infinitely many, and continually increasing, (as the supposed parallels in the triangle ABC, reckoning downwards from A to BC,) whereof the last (BC) is given.—2. A finite quantity, as AB, may be supposed by such continual bisections, divisible into a number of parts infinitely many, or more than any finite number assignable: For there is no stint beyond which such division may not be supposed to be continued; for still the last, how small soever, will have two halves; and all those parts were in the undivided whole; else where should they be had?—3. Of supposed infinites, one may be supposed greater than another: As a supposed infinite number of men, may be supposed to have a greater number of eyes.—4. A surface, or solid, may be supposed so constituted, as to be infinitely long, but finitely great, the breadth continually decreasing in greater proportion than the length increases, and so as to have no centre of gravity. Such is Toricellio's Solidum Hyperbolicum acutum; and others innumerable, discovered by Dr. Wallis, M. Fermat, and others. But to determine this, requires more of geometry and logic than Mr. Hobbes is master of.—5. There may be supposed a number infinite; that is greater than any assignable finite: As the supposed number of parts, arising from a supposed section infinitely continued.—6. There is therefore no reason on this account, why the doctrine of Euclid, Cavallerius, or Dr. Wallis, should be rejected as of no use.

But having solved these quæries, I have some for Mr. Hobbes to answer, which will not so easily be dispatched by him. For though supposed infinites will serve the mathematicians well enough; yet howsoever he please to prevaricate, which he says is for his exercise, Mr. Hobbes himself is more concerned than they, to solve such quæries. Let him ask himself, therefore, if he be still of opinion, that there is no argument in nature to prove the world had a beginning; 1. Whether in case it had not, there must not have passed an infinite number of years before Mr. Hobbes was born. For if but finite, how many soever, it must have begun so many years before.—2. Whether now there have not passed more, that is, more than that infinite number.—3. Whether in that infinite, or more than infinite, number of years, there have not been a greater number of days and hours: and, of which hitherto the last is given.—4. Whether, if this be an absurdity, we have not then, contrary to what Mr. Hobbes would persuade us, an argument in nature to prove the world had a beginning. Nor are we beholden to Mr. Hobbes for this argument; for it was an argument in use before Mr. Hobbes was born. Nor can he serve himself, as the mathematicians do, with supposed infinites; for his infinites, and more than infinites, of years, days, and hours, already past, must be real infinities, and which have actually existed, and whereof the last is given; and yet there are more to follow. Mr. Hobbes shall do well for his exercise, to solve these, before he propose more quæries of infinites. And this I say, to show that Mr. Hobbes is, as much as any, concerned to solve the quæries by himself proposed.

In the latter Part of his first Paper,

He gives us, out of his Roset, prop. 5, this attempt of squaring the circle. Suppose DT be $\frac{2}{5}$ DC, and DR a mean proportional between DC and DT: the semidiameter DC will be equal to the quadrantal arc RS, and DR to TV. Pl. 14, fig. 7.

That the thing is false, is already shown on another occasion. As he has it in his present publication, his demonstration is peccant in these words, "Therefore—the arc on TV, the arc on RS, the arc on CA, cannot be in continual proportion;" with all that follows; there being no ground for such consequence.

And the thing is manifest; for since that, by his construction,

DC, CA, arc on CA extended ∺ }
DR, RS, arc on RS extended ∺ } are in the same continual proportion, of the semidiameter to
DT, TV, arc on TV extended ∺ } the quadrantal arc;

Let that proportion be what you will; suppose as 1 to 2; and consequently DC to CA being as 1 to 2, it will be to the arc on CA, as 1 to 4: And by the same reason DR to the arc on RS, and DT to the arc on TV, must also be as 1 to 4: And therefore the arcs on TV, on RS, on CA; that is 4 DT, 4 DR, 4 DC, will be in the same proportion to one another, as their singles, DT, DR, DC: But these, by construction, are in continual proportion; therefore those arcs also, as they ought to be. Indeed if, by changing some one of the terms, you destroy, contrary to the hypothesis, the continual proportion of DT, DR, DC, you will destroy that of the arcs also, which are still proportional to these: but so long as DT, DR, DC, be in any continual proportion, whether that by him assigned or any other, those will be in the same continual proportion with them. As if for DT, DR, DC, be taken Dt, Dr, DC, in any continual proportion, either greater, less, or equal to his, the arcs on tv, on rs, on CA, extended, will be in the same continual proportion.

But, which is the common fault of Mr. Hobbes's demonstration, if this demonstration were good, it would serve as well for any proposition as that for which he brings it. For if, instead of $\frac{2}{5}$, he had said, $\frac{4}{9}$, $\frac{1}{2}$, $\frac{1}{100}$, or what else he pleased, the demonstration had been just as good as now it is, without changing one syllable: That is, it will equally prove the proportion of the semi-diameter to the quadrantal arc, to be what you please: As any may presently see, who does but read over his paper.

In his second Paper,

He pretends to confute a theorem, which has a long time passed for truth; and therefore no more concerns Dr. Wallis, than other men. And it is this, the four sides of a square being divided into any number of equal parts, for example, into 10; and straight lines drawn through the opposite points, which will divide the square into 100 lesser squares: The received opinion (says he), and which Dr. Wallis commonly uses, is, that the root of those 100, namely 10, is the side of the whole square. Which to confute, he tells us, the root 10 is a number of squares, whereof the whole contains 100; and therefore the root of 100 squares is 10 of those squares, and not the side of any square; because the side of a square is not a superficies, but a line.

For answer, I say that it is neither the opinion of Dr. Wallis, nor of any other that I know, so far is it from being a received opinion, which Mr. Hobbes insinuates as such, that 10 is the root of 100 squares: For surely a bare number cannot be the side of a square figure: Nor yet, as Mr. Hobbes would have it, that 10 squares is the root of 100 squares; But that 10 lengths is the root of 100 squares. It is true, that the number 10 is the root of the number 100, but not of 100 squares: and that 10 squares is the root, not of 100 squares, but of 100 squared squares: Like as 10 dozen is the root, not of 100 dozen, but of 100 dozen dozen, or squares of a dozen. And as there you must multiply, not only 10 into 10, but dozen into dozen, to have the square of 10 dozen; so here 10 into 10, which makes 100, and length into length, which makes a square, to obtain the square of 10 lengths, which is therefore 100 squares, and 10 lengths the root or side of it. But, says he, the root of 100 soldiers, is 10 soldiers. Ans. No such matter: For 100 soldiers is not the product of 10 soldiers into 10 soldiers, but of 10 soldiers into the number 10: And therefore neither 10, nor 10 soldiers the root of it. So 10 lengths into the number 10, makes no square, but 100 lengths; but 10 lengths into 10 lengths makes, not 100 lengths, but 100 squares.

So in all other proportions: As if the number of lengths in the square side be 2; the number of squares in the plain will be twice two, because there will be two rows of two in a row: If the number of lengths in the side be 3; the number of squares in the plain, will be 3 times 3, or the square of 3: If that be 4, this will be 4 times 4: And so in all other proportions. Of which, if any one doubt, he may believe his own eyes. See figs. 8, 9, 10, 11.

And this Mr. Hobbes might have been taught by the next carpenter (that knows but how to measure a foot of board) who could have told him, that because the side of a square foot, is 12 inches in length, the plain of it will be 12 times 12 inches in squares: Because there will be 12 rows of 12 in a row.

His third Paper,

Which came out just as the answer to the two former was going to the press, contains for substance, the same with his second, and the latter part of the first: And so needs no farther answer.—Only I cannot but take notice of his usual trade of contradicting himself. His second paper says, the side of a square is not a superficies, but a line: His third says the quite contrary, (prop. 1.) A square root, (speaking of quantity) is not a line, but a rectangle. Other faults, falsities, and contradictions, there are a great many.—As for instance: He tells us first, in the natural row of numbers, as 1, 2, 3, 4, 5, 6, &c. every one is the square of some number in the same row; that is, of some integer number; which is notoriously false. This he contradicts in the very next words, but square numbers, beginning at 1, intermit first two numbers, then four, then six, &c.; so that none of the intermitted numbers is a square number, nor has any square root. If these intermitted numbers, between 1, 4, 9, 16, &c., be not squares, how is it that every one in the whole row is a square, and that of some integer number? But this again is contradicted prop. 2, where 200, one of such intermitted numbers, is made a square, and $14\frac{4}{14}$ the root of it.

Again, in his definition he tells us, that a square root multiplied into itself produces a square: But (prop. 2) he multiplies the root $14\frac{4}{14}$, not into itself, but into 14 (a part thereof,) to make 200, which he will have to be the square of that root. Nor is it a mere slip of negligence in the computation, but his rule directs to it; any number given is produced by the greatest root multiplied into itself, and into the remaining fraction. Whereof he gives this instance: Let the number given be 200 squares, the greatest root is $14\frac{4}{14}$ squares (he should rather have said lengths; but that is a small fault with him;) I say that 200 is equal to the product of 14 into itself, which is 196, together with 14 multiplied into $\frac{4}{14}$, which is equal to 4: that is $14\frac{4}{14}$ multiplied into 14. But this calculation is again contradicted in his third proposition, where he calculates the same square otherwise, as we shall see by and by. In the mean time let us consider this alone, and see the contradictions within itself. His rule bids us multiply the greatest root into itself, &c. This greatest root he says is $14\frac{4}{14}$; yet does he not multiply this, but 14 (a part thereof) into itself, and into the fraction $\frac{4}{14}$. Again, if 14$\frac{4}{14}$ be the greatest root, what shall be the remaining fraction? Doth he take the root of 200 to be more than 14$\frac{4}{14}$ by some further remaining fraction? If so, he should have told us what that fraction is; for $\frac{4}{14}$ it is not, this being part of his greatest root 14$\frac{4}{14}$. But if we should allow, as I think we must, that by the greatest root he means sometimes 14$\frac{4}{14}$, sometimes 14, that is, if we allow him to contradict himself, yet how comes he by the fraction $\frac{4}{14}$? For $\frac{2}{14}$ is too much, the square of 14$\frac{2}{14}$ being more then 200, as by multiplying 14$\frac{2}{14}$ into itself will appear; which destroys his whole design; for 14, multiplied into 14$\frac{2}{14}$, will not make 200, but 198; contrary to his rule. But further, it is so gross a mistake to make 200 the square of 14$\frac{2}{14}$, that every apprentice boy that can but multiply whole numbers and fractions, could have informed him better, who would first have reduced the fraction to smaller terms, putting 14$\frac{2}{7}$ for 14$\frac{4}{14}$, and then multiplying 14$\frac{2}{7}$ into itself, would have showed him, that the square of 14$\frac{4}{14}$, that is 14$\frac{4}{14}$ multiplied into itself, is not 200, but 204$\frac{4}{49}$.

But the root of 200, is the said number 10$\sqrt{2}$, which is less than 14$\frac{2}{14}$, and greater than 14$\frac{2}{15}$: the square of that being somewhat more than 200; and of this somewhat less; but either of them within an unite of it.

14$\frac{2}{7}$
14$\frac{2}{7}$
——
56
14
4
4
$\frac{4}{49}$
——
204$\frac{4}{49}$

But this second proposition is, as I said, contradicted by his third, which makes the square of 14$\frac{4}{14}$ to be 200$\frac{1}{49}$, by what computation, we shall see by and by; and then finds fault, that this and the former do not agree. But it is no wonder they should disagree, when both are false. The same square (says he) calculated geometrically, consists (by Euclid 2, 4) of the same numeral great square 196, and of two rectangles under the greatest side 14 and the remainder of the side, and further of the square of the less segment; which altogether make 200$\frac{1}{49}$. He might have learned to reckon better; but let us see how he makes it out. As by the operation itself (says he) appeared thus: The side of the greater segment is 14$\frac{4}{14}$ (this was but now the side of the whole square, how comes it now to be but the side of the greater segment?) which multiplied into itself (says he) makes 200: (no, but 204$\frac{4}{49}$:) The product of 14 the greatest segment into the two fractions $\frac{4}{14}$ is 4, and that added to 196 makes 200: (if by two fractions $\frac{4}{14}$, he mean, as he ought by his rule, the fraction 4 twice taken, or the double of it, it will be not 4, but 8, and this added to 196 makes 204; but all this he puts in his pocket, for it comes not into account at all.) Lastly, the product of $\frac{2}{14}$ into $\frac{2}{14}$, or $\frac{1}{7}$ into $\frac{1}{7}$ is $\frac{1}{49}$; which with the first 200 makes 200$\frac{1}{49}$: But he forgets himself, for his lesser segment was not $\frac{2}{14}$, but $\frac{4}{14}$; he should therefore have said $\frac{4}{14}$ into $\frac{4}{14}$, or $\frac{2}{7}$

into $\frac{2}{7}$ is $\frac{4}{49}$. His calculation therefore should have been this: The greater segment is, not $14\frac{1}{14}$, but 14; which multiplied into itself makes, not 200, but 196: The rectangle of the greater segment 14, into the lesser $\frac{4}{14}$, is 4: And this taken a second time is another 4: The lesser segment, not $\frac{2}{14}$, but $\frac{4}{14}$, or $\frac{2}{7}$, multiplied into itself, is not $\frac{1}{49}$, but $\frac{4}{49}$: All which added together make not $200\frac{1}{49}$, but $196 + 4 + 4 + \frac{4}{49} = 204\frac{4}{19}$, which is just the same with $14\frac{4}{14}$ multiplied into itself. So that had he known how to multiply a number into a number, especially when incumbered with fractions, which it is manifest he does not, he would have found no disagreement between the arithmetical calculation, and what he calls the geometrical. But I am ashamed (for him) that so great a pretender to such high things in geometry, should be so miserably ignorant of the common operations of practical arithmetic.

His repeated quadrature he now expresses thus; the radius of a circle is a mean proportional between the arc of a quadrant and two fifths of the same. But instead of two fifths, he might as well have said the half, or tenth, or hundredth part, &c.; or (taking T in DC produced beyond C,) the double, decuple, centuple, &c. or what you please: For his demonstration would have proved it, which is this. Describe a square ABCD, and in it a quadrant DCA. In the side DC (continued if need be,) take TD two fifths of DC, (or its half, double, hundredth part, or what you please;) and between DC and DT a mean proportional DR; and describe the quadrantal arcs RS, TV. I say the arc RS is equal to the straight line DC. For seeing the proportion of DC to DT is duplicate of the proportion of DC to DR, it will be also duplicate of the proportion of the arc CA to the arc RS, and likewise duplicate of the proportion of the arc RS to the arc TV. Suppose some other arc, less or greater than the arc RS, to be equal to DC, as for example rs; then the proportion of the arc rs to the straight line DT will be duplicate of the proportion of RS to TV, or DR to DT, which is absurd; because Dr is by construction greater or less than DR. Therefore the arc RS is equal to the side DC; which was to be demonstrated. Which demonstration therefore proving indifferently every proportion, does not indeed prove any. In brief: The force of his demonstration is but this; DT being to DC as 2 to 5, or in any other proportion, and DR a mean proportional between them; RS will be so between TV and CA; and therefore rs (greater or less than RS,) will not be a mean proportional between TV and CA: which is true; but why it may not be equal to DC, we have nothing but his word for it; there being nothing to show, that DC is equal to such a mean proportional. Again, though rs be not a mean proportional between TV and CA, yet it may be between tv and CA, which serves his demonstration as well; which is indifferent to any three continual proportionals, as was shown before.

So that now we have had three demonstrations of this quadrature, in his Rosetum, in his first paper, and in his third, and this common fault in all of them, that they equally prove the proportion by him proposed, or any other what you please. But such his demonstrations use to be.

And this is what I thought fit to say to Mr. Hobbes's four papers, rather to satisfy the importunity of others, than because I thought them worth answering: And submit the whole, with all respects, to the Royal Society, to whom Mr. Hobbes makes his appeal.

His Fourth Part,

Which came out since the three former were answered, containing some faint endeavours to re-assert some things in them, is but mere trifling, or worse.

What he would therein insinuate concerning God, that we may as well prove Him to have had a beginning, as that the world had, smells too rank of Mr. Hobbes. We are not to measure God's permanent duration of eternity, by our successive duration of time: nor His entire ubiquity, by corporeal extension.

What in it concerns mathematics, whether his own or others, is so weak and trivial, and said only, that he may seem to say something, though nothing to the purpose, that I shall trust it with those to whom he makes his appeal, without thinking it to need any reply; the view of what he writes against, being a sufficient answer to all he says.

New Observations of Spots in the Sun; made at the Royal Academy of Paris, the 11th, 12th and 13th of August 1671; and translated from the French, as follows. N° 75, p. 2250.

It is now about twenty* years since astronomers have seen any considerable spots in the sun, though before that time, since the invention of telescopes, they have from time to time observed them. The sun appeared all that while with an entire brightness, and Signor Cassini saw him so the ninth of this month of August.

But the 11th of the same, about six o'clock at night, being furnished only with a three feet glass, he remarked in the sun's disk two spots very dark, distant from his apparent centre about the third part of his semidiameter. And that he might the more exactly note their situation, in respect of the several parts of the world, he made use of two very fine threads, cutting one another

* See number 74, p. 615, whence it will appear, that some such spots were seen here in London, A. 1660. And M. Picard affirmed to Dr. Fogelius at Hamburgh, that he had seen some in October 1661; witness the said Doctor's own letter, written to the editor, August 11th last.

at right angles in the common focus of the two glasses, and in the axis of the telescope: and having directed it toward the sun, he so turned it, that letting it afterwards rest, one might see the centre of the sun, according to one of these threads, advance westward, this same thread marking in the sun a circle parallel to the equator; and the other thread marked the circle of declination; or the horary circle of the said sun. Thus he noted the situation and motion of the spots.

On the twelfth of August he observed them from the time of sun-rising, and perceived them now nearer his centre. He continued exactly to observe them with a large telescope, from six o'clock in the morning to seven. The first was composed of two others almost round, and conjoined. The second represented the shape of a scorpion. The third was round. And they were all three environed with a coronet, which was composed of abundance of little obscure points. At 48 minutes after eight o'clock, the figure of the scorpion was seen divided into several pieces, as if his tail and arms had been cut off. The northern point appeared no more, there remaining none but those on the south side; and the length of the enclosure of all the spots, comprehended between the extremities, was of one minute and 15 seconds, and the breadth of 30 seconds. The same 12th day, at six in the evening, he found no great change in the first spot. The other two were severed into five distinct ones, compassed about with a coronet.

The 13th of August they were observed again.

In one day and a half these spots have run through very near the third part of the sun's apparent semidiameter, which gives an arc of $19\frac{1}{2}$ degrees of the circumference of the sun's body; and consequently their diurnal motion about the sun's axis has been of 13 degrees; and the time of their periodical revolution, as far as we could conjecture in so little time, must be about 27 days and a half: which yet will be more exactly determined by observations of a longer time. And in 6 or 7 days hence they may probably arrive at the sun's edge.

So far the French academists. To which we now add; 1. That since their observations were made public, we had notice sent us from the above-mentioned Dr. Fogelius at Hamburgh, that Mr. Picard had observed at sea a spot in the sun from the third of August n. st. to the 19th of the same inclusively. 2. That several observers at London have seen one of those spots arrive at the sun's eastern limb, on the same day that Signor Cassini predicted in the relation above delivered they should return, if they continued.

An Account of Vegetable Excrescencies, by Mr. Martin Lister, *in a Letter of July* 17, 1671. *N° 75, p.* 2254.

An ingenious paper, but not containing any thing that deserves particular notice at this day.

An Account of some Books. N° 75, p. 2258.

I. Lectiones 18, Cantabrigiæ in Scholis publicis habitæ, in quibus Opticorum Phenomenωn Genuinæ rationes investigantur et exponuntur, ab Isaaco Barrow,* Coll. S. Trin. Socio, Mathes. Profess. Lucasiano, et Soc. Regiæ Sodali. Londini, 1669, in 4to.

* Dr. Isaac Barrow, an eminent divine and mathematician, was born in London, 1630. He was first placed at the Charter-house, but afterwards removed to a school at Felsted in Essex, from whence he was sent to Cambridge, where he was entered of Trinity college, in 1645; where he studied very diligently, and made great progress in various branches of literature; as, natural philosophy, botany, anatomy, chemistry, divinity, mathematics, astronomy, &c. In 1649 he was chosen fellow of his college. But, on some disgust, he quitted the college, and travelled abroad for several years, through France, Italy, Turkey, &c. improving his knowledge; and at Constantinople he read over the works of Chrysostom, whom he ever after preferred to all the other fathers. On his return home he was episcopally ordained, and in 1660 was chosen Greek professor at Cambridge. In 1662 he was appointed professor of geometry in Gresham college, London; and the year following he was elected fellow of the Royal Society, being the very first choice made by the council after their charter. This same year also he was named the first Lucasian professor of mathematics, and soon after resigned that at Gresham college. But he held the former only till 1669, when he resigned the mathematical chair to his illustrious friend and pupil Mr. Isaac Newton. In 1670 he was created D. D.; and two years after he was appointed master of Trinity college; on which occasion the king was pleased to say, " he had given the office to the best scholar in England." Indeed his majesty was always and justly partial to him; being then his chaplain, he often conversed with him, and, in his humorous way, would call him " an unfair preacher," because he exhausted every subject, and left no room for others to come after him. In 1675 he served the office of vice-chancellor of the university. But his useful labours were suddenly terminated by a fever in 1677, when he died, being only in the 47th year of his age; and was interred in Westminster Abbey, where a monument, adorned with his bust, was soon after erected, by the contribution of his friends.

The name of Dr. Barrow will ever be illustrious for the strength of his genius and the compass of his knowledge. In mathematical learning he was unrivalled, especially in the higher geometry; in which he has since been excelled only by his successor Newton. The same lofty genius would sometimes amuse itself in the flowery paths of poetry; and we find he composed verses both in Greek and Latin. He at length gave himself up entirely to divinity; particularly to the more useful part of it, that which tends to make men wiser and better.

Dr. Barrow's works are very numerous and various; mathematical, theological, poetical, &c. All the theological pieces, being in English, were collected and published 1683, by Dr. Tillotson, in 3 vols. folio. His mathematical works are, Euclidis Elementi; Euclidis Data; Lectiones Opticæ; Lectiones Geometricæ; Archimedis Opera; Apollonii Conicorum lib. 4; Theodosii Sphericorum lib. 3, nova methodo illustrata, et succincte demonstrata. Those were all published by him-

A brief summary of these optical lectures the author himself delivers in an epistle to a friend, when he says, that in them he endeavours to promote that part of optics which he undertakes to treat upon, first by explaining and establishing its principles; then by deriving from them some useful consectaries, serving to the explication of the phænomena: in the mean while attempting to correct some common errors, and to supply some principal defects therein.

Towards these ends, he first examines the received hypotheses of this science, showing how they should be understood, and how far admitted. Having settled the hypotheses, he first draws from them some general corollaries, partly before acknowledged by others, and partly observed by himself; all confirmed by his own demonstrations. Then proceeding to more special matters, he prosecutes distinctly catoptrics and dioptrics, both plain and spherical. And slightly passing over plain catoptrics, he more largely insists on spherical catoptrics, propounding such theorems, whereby the intersections and the limits of reflected rays are declared, and also the appearing places, or images of points radiating both from a great (and as it were immense) distance, and from a distance sensibly near, are determined, both in respect of an eye placed in the axis of the radiation, and in respect to one placed without that axis any where; which particulars he had not observed to be truly and diligently handled in any book extant.

Then he raises a theory concerning both plain and spherical dioptrics, assuming for a ground that rule concerning the measure of refractions mentioned by M. Descàrtes, and which now most writers do admit, and which he judges agreeable to truth, which yet he knows not that any writers have applied to this purpose, so as to have raised any competent superstructure thereon. And here orderly, first in respect to plain, then to spherical surfaces, considering points radiating from a distance so great that their rays may be supposed to fall parallel to one another on the refracting surface, he propounds some theorems, from which the chief symptoms of refracted rays do result, their interjections and limits are easily discernible; the appearing places or images of such points are defined, both in regard to an eye situated in the axis of the radiation, and in regard to one placed elsewhere. Then he prosecutes in like manner the same things in respect to points radiating from a distance sensibly finite or near hand. And that the use of these things may be more ready, and serviceable to practise, he subjoins distinctly and particularly the

self. After his death appeared the three following, Mathematicæ Lectiones habitæ in scholis academiæ Cantab. and Lectio in qua theoremata Archimedis de sphæra et cylindro, &c. also Isaaci Barrow opuscula, viz. Determinationes, Conciones ad clerum, Orationes, Poemata, &c.

determinations of the places in the axis of the images of points however radiating through each kind of lenses.

Having dispatched these matters, he touches generally concerning the making a judgment about the appearances of magnitudes, as to their situation and figure, which follow those sorts of reflection and refraction: afterward he shows more fully what kind of images plain objects yield from such reflections, and how they may be delineated.

Among these things there are interspersed some considerations about divers incident matters, as about the nature of light, and the causes of different colours about the rainbow, or colours appearing in pellucid globes; about some appearances in the glass prism; concerning the linea refractaria; concerning the resolution of problems by appropriate lines; concerning the properties of the conical sections in the reflection of lucid rays, &c.

II. Lectiones 13 Geometricæ, in quibus (præsertim) Generalia Linearum Curvarum Symptomata declarantur ab eodem Isaaco Barrow, &c. 4to.

Concerning the geometrical lectures, arguing great depth in the mathematical learning.

In the first, in order to what follows concerning the generation of magnitudes, the author treats about the nature of time, as it may be considered in mathematical suppositions about such generations.

In the second are declared the mathematical hypotheses about simple motions, progressive and circular, which serve to the production of magnitudes, with some general remarks on the natures, dimensions, and properties (consequent on such productions) of magnitudes. There is also a touch about the method of indivisibles, explaining how in some cases it is to be understood and applied.

The third treats about the generation of magnitudes by the composition and concourse of motions.

In the fourth and fifth, from one generation propounded of curve lines, supposing them produced by two motions, one uniform, the other accelerated, divers theorems are inferred, implying so many general properties of curve lines.

The next five lectures contain many theorems, and problems about readily determining the tangents of curve lines, immediately by them, without other computation; particularly, there are divers single theorems, whereby the tangents of all curves commonly known or considered in geometry, the conical sections, conchoids, cissoids, spirals, quadratrices, &c. are determined in ways so general, as to comprehend also the like determination of tangents in regard to innumerable other curves, generated in a common manner with them. Of those five lectures, the two first are lemmatical, or preparatory to the demonstration of the propositions delivered in the other three, containing some curious theorems. In the tenth is delivered a general analytical method of determining tangents, extending to all sorts of curve lines, both geometrical and mechanical, as M. Descartes distinguishes them.

The 11th lecture contains several general theorems, about the dimension of magnitudes, or the comparison of them with one another. To which is subjoined an appendix concerning the dimension of circular and hyperbolical segments, with divers theorems and rules serving to that purpose.

The 12th lecture contains also several theorems concerning the dimension of magnitudes, but chiefly respecting the dimension of surfaces produced by the rotation of curve lines, and the dimension of curve lines themselves. To this lecture there are also three additaments; the first containing some theorems about the dimensions of spaces constituted by the tangents and secants of a circle. The second shows how the foregoing theorems may be demonstrated by the apagogic way, or by reduction *ad absurdum*; together with a way of finding the dimension of the surfaces of conical bodies. In the third divers problems and theorems are added, akin to those of the 11th and 12th lecture.

The 13th lecture propounds an explication of the nature and constitution of equations, with the variety of roots, their limits, &c. by construction and consideration of certain curve lines appropriate to each equation: with some notes respecting each particularly, and all in general.

So much of these two excellent treatises: since the publication of which, their worthy author has been pleased to communicate to a friend of his some corollaries, belonging to the second problem of his third appendix to the 12th lecture; which because we conceive they will be very acceptable to the mathematical reader, we shall here subjoin.*

III. A Continuation of the Memoirs of M. Bernier, concerning the Empire of the Great Mogul. From the French. London, 1671, in 8vo.

The first volume of these memoirs, containing little but political affairs, was left unmentioned in these books; but in this second, besides an accurate description of the two famous cities of Indostan, Delhi and Agra, and many things showing the genius of the Moguls and Indians, also those which belong to their militia, &c. is an account given,

First, of the extravagant opinions of the Gentiles of Indostan; of their odd

* These may be seen introduced into Mr. Stone's translation of the work, towards the conclusion.

belief concerning eclipses; of the books of sciences received amongst them; of their doctrine of the transmigration of souls, and of the creation, preservation, and destruction of the world; as also a relation of the different sects of philosophers among them; of their method of physic, very different from ours, and how successful; of their ignorance in, and aversion from anatomy; of their pleasant tenets in astronomy, geography, and chronology; of their opinion concerning plants and animals, importing that the seeds of both of these kinds are not formed anew, but were contrived in the first production of the world; as also, that they are actually the very entire plant or animal.

Secondly, a good description of the kingdom of Kachemire, the reputed paradise of the East Indies; its ancient state; its present condition and excellencies for soil, vegetables, waters, cattle, wild deer; the wit and industry of the inhabitants in making fine stuffs, good varnish, &c. the condition of its mountains, one side of them being intolerably hot, and yielding Indian plants, the other very tolerable, and affording none but European plants; some remarkables about the generation and corruption of trees there; strange cascades of water; periodical fountain, &c. The situation of the kingdoms of Tibet, and the commodities they afford. A considerable account of voyages made by caravans from Kachemire through the mountains of the Great Tibet into Tartary and Cataja, &c. &c.

IV. Historia et Meteorologia Incendii Ætnæi Anni 1669. Joh. Alph. Borelli. Regio Julio 1670, in 4to.

The learned author having given a short topography of this mountain, delivers first a succinct relation both of the old and later eruptions, as also of this last fire of Ætna, and assigns the perpendicular height of the same, showing it not to exceed three Italian miles, and here taking notice of Kepler's assigning two such miles for the height of the atmosphere, and thence concluding the top of Ætna to be considerably raised above that region of the common air; confirming the same by a known experiment, whereby those that are on the said top at a clear break of day, may plainly see the whole island of Sicily, and all the towns thereof, as if it were elevated and hanging in the air, near the eye, just as, by refraction, stones lying at the bottom of a pond appear nigh the surface of the water.

After this, in the history itself of this eruption he describes particularly:

First, the beginning of it, which happened on the 8th of March, 1669, accompanied with earthquakes, and a rent of the ground of 12 miles long, and five or six feet broad, as also with a terrible noise, roaring and cracking, vast globes of smoke first rising into the air, and abundance of fiery melted stones being ejected soon after, which first ran like a flood of fire, and overwhelmed in a short space of time 13 towns, besides a part of the city of Catania itself, and afterwards were by the air hardened into vast heaps of black and pumice-like stones, there called Sciarra; wasting and spoiling abundance of vines, olives, and other plants.

The casting out of the ashes and sand continued for three whole months without ceasing, and filled all the neighbouring country, and covered all the trees thereof for 15 miles about; the smallest dust flying even over sea into Calabria by a south wind, and into the most southern parts of Sicily by a north wind.

But then on the 25th of March, by a new earthquake the top or turret of Mount Ætna itself fell in, whereby was made an opening or cauldron of three miles in compass, and vast quantities of new matter cast out, and amongst it abundance of fiery sand, falling down with a yet burning heat at eight miles distance from the cauldron; whereupon the same by particular view and observation was found widened to the circumference of six miles. Mean while all considering men there were amazed at the force throwing out to so great a height such huge stones, whereof one was measured to be 60 palmes, or about 40 feet, long, which was fallen down a mile from the cauldron with that violence, that it was struck 30 palmes into the ground.

When this fiery torrent assaulted Catania itself, and had already by its impetuosity forced from its place a whole hillock, planted with vines, belonging to the Jesuits, and carried them floating, together with the soil bearing them, till it so swelled as to cover and sink them all; there appeared some gallant persons, who by their ingenuity and extraordinary diligence, with fit instruments, and raising vast strong walls, diverted the course of the fiery stream from that city, but chiefly by boring through the stony heaps, and thereby making passage for that current another way, and turning part of it into the sea, wherein it made a promontory of a mile's compass before the town.

It ceased by the 11th of July of the same year it began; and in May of 1670, our author himself could handle without hurt the inner parts of the cauldron and the former torrent, and saw not so much as any smoke remaining in the highest part of that opening; where yet he observes, that notwithstanding this entire ceasing in the said places, there were yet found in several parts of this newly ejected sciarra hot and strong-smelling fumes arising on high, especially near the walls of the south side of Catania, where wells had been dug for watering their garden fields.

This being the breviate of the history of this eruption, which the author describes with many other considerable circumstances, after this description he expatiates into divers important speculations and remarks thereon.

An Advertisement necessary to be given to the Readers of the Latin Version, made by Mr. STERPIN *at Copenhagen, of the Philosophical Transactions of A.* 1669; *printed at Frankfort on the Maine by* DAN. PAULI, *A.* 1671. *N° 75, p.* 2269.

Of no use now.

Observations of the Eclipse of the Moon, on Sept. 8, 1671, *made by several Astronomers in different Countries. N° 76, p.* 2272.

1. *By Mr. Street, in London.*

The emersion: altitude of the ☾'s upper edge	10°	30′	7^h	21^m
End of the eclipse: altitude of Arcturus	16	20	8	16½

2. *By Mr. Palmer, at Ecton in Northamptonshire.*

About six in the evening the moon rose totally eclipsed at Ecton. The moon began to emerge out of the shadow at 7h. 18m. her centre being 9° 35′ high. At the end, Arcturus was 16° 30′ high, or the time was 8h. 16m. 20s. Whence the middle of the eclipse is computed to be at 6h. 28m. 16s.

3. *By Bullìald, at Paris.*

End of eclipse: altitude of Arcturus to the west was	13°	41′	0″
Subtracting the refraction, his altitude was	13	37	45
Hence is given the time of observation	8^h	29^m	16^s
End of the penumbra: altitude of Arcturus	13°	0′	0″
The same corrected	12	56	0
Hence the time was	8^h	33^m	40^s
Mœotis observed wholly out of the shadow at	8	24	16
The corrected altitude of Arcturus being	14°	23′	55″

The Moon quitted the shadow opposite Petra Sogdiana in Hevelius's map.

Account of the Dissection of a Porpoise. By Mr. JOHN RAY. *N° 76, p.* 2274.

SIR,

About the latter end of April, 1669, being at Winchester with my Lord Bishop of that diocese, in the company of Fr. Willoughby, Esq. I had the good fortune to meet with a young porpoise of a convenient size for dissection, brought thither by some fishermen who caught him upon the sands, where the tide had left him; in the anatomy whereof I observed some things omitted by Rondeletius* in his description of the dolphin.

The length of this was by measure three feet seven inches; a string of two feet two inches girded him in the thickest place; the shape of his body was not much unlike that of a tunny fish, only his snout longer and sharper; his skin was thin, smooth, and without scales. In an old and well grown fish the skin may probably be thick and tough, as Rondeletius represents it.

His fins are cartilaginous and flexible, not sharp or prickly, as the ancients report them. On his back he has only one, which was distant from the tip of his snout one foot nine inches, and the basis of it in length five inches and a half, so that measuring from the tip of his snout to the end of the tail, it was situate somewhat below the middle of the fish's length. On the belly it had only one pair of fins, nine inches and a half distant from the tip of the lower mandible, much about the place where the foremost pair of fins in other fishes usually grow. The tail is forked, of the figure of a crescent; the breadth thereof from angle to angle 11 inches. The situs or position of it contrary to that of all other fishes, except those of this kind. For, whereas the plain of the tail of other fishes, when they swim, stands erected perpendicularly to the plain of the horizon, in this fish (and I suppose in all others of the cetaceous kind) it lies parallel thereto. The reason whereof I conceive to be partly to supply the use of the hindmost pair of fins in other fishes, which serve to balance the body, and keep it up in the water, answering in proportion to the hinder legs of a quadruped; hence we see, that those fishes, which have long bodies and but one pair of fins, as eels, and the like, cannot keep themselves up in the water, but lie always grovelling on the bottom; partly to facilitate the

* A celebrated teacher of anatomy and physic at Montpellier in the 16th century. It is disputed whether he or Ingrassias was the discoverer of the vesiculæ seminales. At his instance an anatomical theatre was built at Montpellier; wherein he is said to have dissected the dead body of one of his own children, and to have incurred much obloquy for allowing his feelings as a parent to be overcome by his ardour for anatomical inquiries. He died in 1566 of a bowel complaint, occasioned by eating immoderately of fresh figs. [A physician should have been more cautious in his diet; but physicians, like the rest of mankind, will sometimes indulge in appetites and inclinations which they know to be wrong. The writer of this note was acquainted with a medical practitioner of considerable eminence, who could not refrain from eating toasted cheese, though he was subject to an alarming pulmonary complaint, which was uniformly aggravated by it, and which terminated fatally at an age by no means advanced; and there is now living a physician of his acquaintance, who, at an autumnal desert, never ceases eating all the filberts he can lay his hands upon, although he very candidly acknowledges that they are extremely indigestible and hurtful things.] He wrote several Latin treatises on medical subjects, which were reprinted collectively at Geneva, 1628, under the title Gulielmi Rondeletii Opera omnia medica; but the work for which he is most celebrated is that De Piscibus, printed at Lyons, part I. 1554, and part II. 1555, in folio.

fish's ascent to the top of the water for the use of respiration, which is as necessary for him as for quadrupeds.

Immediately under the skin lay the fat, which, as I remember, our seamen call the blubber; it was firm, full of fibres, and in this small fish of an inch thickness, encompassing and enclosing the whole body, back, belly, and sides. The use whereof I conceive to be, 1. To keep the cold water at a distance from the blood, which is, I believe, actually and to the touch hot, in a degree not much inferior to that of quadrupeds, and therefore by immediate contact of the water would be apt to be chilled. 2. To keep in the hot steams of the blood from evaporating; by that means also preserving and maintaining its natural heat; as we see water, and any other liquor, in a close vessel will retain its heat much longer than in an open; and nothing is more proper to detain the finest and subtillest evaporations and spirits, than oil or fat. 3. Perhaps also, to lighten or counterpoise the body of the fish, which would otherwise be too heavy to move and swim in the water. Under the blubber lay the muscular flesh, like to that of quadrupeds, but of a darker colour.

The body was divided into three regions or ventries like a quadruped's, viz. head, breast, and belly; the vessels and viscera in each venter, for the main, the same as in quadrupeds; 1. The abdomen was compassed about with a strong peritonæum. The guts joined to the mesentery, and of a very great length, in measure 48 feet, without any difference or distinction of great and small; neither was there any blindgut or appendix, that I could find. The stomach was of a strange make, being divided into two large bags, beside other smaller ones. I found nothing in it but a good number of those little long fishes, which our fishermen dig out of the sands at low water, and therefore call in some places sand eels, by some they are called launces, and by Gesner, ammodytæ.

The liver was of a moderate size, situate in the right side, and divided into two lobes, having no cystis fellea or receptacle of gall annexed. The pancreas large, sticking close to the third bag of the stomach, into which also its duct enters and empties itself. The spleen small and roundish. The kidneys large, sticking close to the back, and lying contiguous one to the other, made up of many little kernels, like, but less than those of an ox, of a flat figure, having no pelvis in the middle, but the ureters going out at the lower end.

The urine bladder oblong, and little for the bulk of the animal, having on each side a round ligament, made of the umbilical arteries degenerating. The penis long, slender, having a small sharp glans, it appears not outwardly, but lies hid in its sheath within the body, doubled up or rather reflected in the form of the letter S, as is that of a bull. The testicles lie within the cavity of the abdomen on each side, as they do in a hedgehog, and some other quadrupeds, of an oblong figure; for their internal substance, seminal vessels both præparantia et deferentia, epididymides, vas pyramidale, corpus varicosum, et glandulæ prostatæ, exactly like to those of quadrupeds. The seminal vessels perforate the urethra with many little holes, whereof four are most conspicuous somewhat above the neck of the bladder.

The diaphragm was muscular, as in quadrupeds. The heart large, included in a pericardium, had its two ventricles; its valvulæ sigmoides semilunares, tricuspides et mitrales; its coronary arteries and veins: in a word, the whole structure and substance of the heart and lungs agreed exactly with that of quadrupeds. The windpipe was very short, as it must needs be, the fish having no neck; the larynx at top was of a singular figure, running out with a long neck, and a nob at the end like an old fashioned ewer.

The pipe in the head, through which this kind of fish draw their breath, and spout out water, lies before the brain, and ends outwardly in one common hole, but inwardly it is divided by a bony septum, as it were into two nostrils; but below again it opens into the mouth in one hole. This lower orifice is furnished with a strong sphincter, whereby it may be shut and opened at pleasure, and above this sphincter, the sides of the pipe are lined with a glandulous flesh, which if you press, you shall see start out of many little holes or papillæ into the cavity of the pipe a certain glutinous liquor. Above the nostrils is a strong valve or membrane like an epiglottis, which serves to stop the pipe that no water get in there against the fish's will. Within the fistula are six blind holes having no outlet; four tending toward the snout; two above the valve that stops the nostrils; and two beneath it; two tending towards the brain, having a long but narrow cavity for the use of smelling, as I conjecture, though opening the brain I could find neither olfactory nerves nor processus mammillares. The eyes are small, considering the bigness of the fish, and at a good distance from the basis of the brain. The snout is long, and furnished with very large muscles, for rooting or turning up the sand at the bottom of the sea to find fishes, as of that we found nothing was in its stomach but sand eels. The brain and cerebellum are, for the substance and anfractus of them, the same with those of quadrupeds, only differing in the figure as being shorter; but what they want in length they make up in breadth. They have also the like teguments called dura and pia-mater. Six or seven pair of nerves, besides the optic, the same ventricles; only in the medulla oblongata we observed not those protuberances called nates and testes. The skull, (cranium) is not so strong and thick as in quadrupeds, but articulated after the same manner to the first vertebra of the backbone. This largeness of the brain, and correspondence of it to that of man, argue this creature to be of more than ordinary wit and capacity, and make those ancient stories appear less fabulous and improbable which are related by Herodotus concerning Arion, by Pliny the Elder,* concerning a dolphin enamoured of a boy, whom he was wont to carry across a bay of the sea, from Baiæ to Puteoli, to school, &c. By Pliny the Younger, of another enamoured of a boy at Hippo in Africa, whom he was wont to carry upon his back in like manner. The story is worth the noting, Epist. 33, l. 9.

But to proceed, this fish had in each jaw 48 teeth, standing in a row like little blunt pegs. The tongue was flat above, of an equal breadth to the very tip, which was toothed or pectinated about the edges, tied firmly down to the bottom of the mouth all along the middle, as Aristotle truly describes; whence I cannot but wonder, that Rondeletius should herein contradict Aristotle, and affirm quòd delphinis lingua est mobilis, quæ modò exeri modò condi potest: unless perchance in this particular the dolphin differs from the porpoise. For the porpoise is, as I take it, the phocæna of the ancients, which is a lesser sort of dolphin, and not the delphinus; at least if the fish we are describing were a porpoise; for the teeth of this fish were less than, and of a different figure from, those in the jaw of the dolphin we got beyond seas; yet is the difference not great between the dolphin and phocæna. As for that fish, which our seamen now call the dolphin, and which, as it is described by Mr. Terry and Ligon, has teeth on its tongue, small scales, is finned like a rock, of a pleasant smell and taste; what it is I know not, but I am sure it is *toto genere* different from the dolphin of the ancients.

We observed not in this fish any nostrils besides those in the fistula, nor any ear-holes or meatus auditorii at all; wherein also Aristotle agrees with us, which yet Rondeletius found out near the eyes; it being manifest, says he, that a dolphin does hear, and seeing no creature can hear without a passage for that purpose to convey sounds to the brain; hac ratione impulsus, cùm Delphini cranium diligentissimè contemplatus essem, manifestissimum audiendi meatum, qui ad cerebrum usque patet, inveni statim post oculum, tam exiguum, ut ferè oculorum aciem fugiat. And we observed in the skull a bone answering to the os petresum, which most certainly was for the use of hearing. It had six short ribs with no cartilages, and seven that had cartilages (on each side I mean.) The breast bone was very small. As for the name porpoise, I agree with Gesner, that it was so called, quasi porcus piscis, most nations calling this fish porcus marinus, or the sea swine. Indeed it resembles a swine in many particulars, as the fat, the strength of the snout, &c.

* L. ix. Hist. Nat. c. 8.

Observations about that Kind of Wasps called Vespæ Ichneumones. By FRANCIS WILLOUGHBY, *Esq.* N° 76, *p.* 2279.

Mr. Lister's opinion is, that the muscæ ichneumones lay their eggs in the bodies of caterpillars; which I look upon as true and very ingenious, and must subscribe to it, though I cannot yet absolutely demonstrate it, as I hoped I should have done before this. These ichneumones have all four wings and antennæ, like bees; their body hanging to their breasts by a slender ligament, as in wasps; most, if not all, have stings, and are made of a maggot that spins herself a theca before she turns into a nympha. There is great variety of them; some breed as bees do, laying an egg, which produces a maggot, which they feed till it comes to its full growth: Others, as we guess, thrust in their eggs into plants, the bodies of living caterpillars, maggots, &c. For it is very surprising to observe, that a great caterpillar, instead of being changed into a butterfly, according to the usual course of nature, should produce sometimes one, sometimes two or three, and sometimes a whole swarm of ichneumones. I have observed this anomalous production in a great many sorts of caterpillars, both hairy and smooth; in several sorts of maggots, and which is most strange, in one water insect. When there come many of these ichneumon maggots out of the body of the same caterpillar, they weave all their thecas together into one bunch, which is sometimes round, with web about it, just like a bag of spider's eggs; but I dare venture to answer Mr. Lister negatively, that none of them feed upon spiders' eggs; but it is the similitude of those thecas, conglobated together, to the eggs of spiders, that has occasioned the conjecture.

One of the green caterpillars, common in the heaths in the north, went so far on to her natural change, that she made herself up into a large brown theca, almost of the shape of a bottle, which was filled with a swarm of ichneumones. And I have observed in one or two other sorts, that from the very aurelia itself has come an ichneumon; which is very odd, that the caterpillar stung and impregnated by the ichneumons, should be yet so far unhurt, and unconcerned, as to make herself a theca, and to be turned into an aurelia.

I have often seen a large ichneumon dragging a caterpillar in the highway. This year Mr. Ray, in company with a neighbour, observed one hauling a large green caterpillar much larger than herself, which after drawing the length of a perch, she lays down, and then takes out a little pellet of earth, with which she had stopped the mouth of a small hole like a worm-hole; then she goes down into it, and staying a very little, comes up again, and draws the eruca down with her into the hole, and there leaves her; and afterwards not only stops but fills up the hole, sometimes carrying in little clods, and sometimes scraping dust with her feet, and throwing it backwards into the hole, going down afterwards herself, to ram it close. Once or twice she flew up into a pine-tree, which grew just over her hole, perhaps to fetch cement; when the hole was full and even with the superficies of the ground about it, she draws two pine-tree leaves, and lays them near the mouth of the hole, and flies away. Not taking notice that she came any more in three or four days, we digged for the caterpillar, and found it pretty deep. I put it into a box, expecting it would have produced an ichneumon, but it dried away and nothing came of it. We lately observed a sort of ichneumons, or rather vespæ, which prey upon several sorts of flies; when they fly with them, they hold them by the heads, and carry them under their bellies. These make holes a great depth in the ground, in which they lay their young, and feed them with the flies they catch, creeping backwards into the ground, and drawing the flies after them. I suspect they may at first lay their eggs in the very body of a fly, but one fly being not enough to bring the young one to its full growth, they feed it with more: Their thecas are at last all covered over with the wings, legs, and other fragments of flies.

A Letter of Mr. Martin Lister, confirming the Observation in N° 74, about Musk-scented Insects; adding some Notes upon Dr. Swammerdam's Book of Insects, and on that of Mr. Steno, concerning Petrified Shells. N° 76, *p.* 2281.

The former part of this paper is now unnecessary: the author's ideas relative to fossil shells are singular, and therefore reprinted in his own words.

Concerning petrified shells, I mean such shells as I have observed in our English stone quarries. But, Sir, let me premise thus much, that I am confident that you at least will acquit me, and not believe me one of a litigious nature. This I say in reference to what I have lately read in Steno's Prodromus, that, if my sentiments on this particular are somewhat different from his, it proceeds not from a spirit of contradiction, but from a different view of nature. First then, we will easily believe, that in some countries, and particularly along the shores of the Mediterranean Sea, there may be found all manner of sea shells promiscuously included in rocks or earth, and at good distances too from the sea. But, for our English inland quarries, which also abound with infinite number and great varieties of shells, I am apt to think there is no such matter as petrifying of shells in the business (or, as Steno explains himself, p. 84, in the English version, et alibi, that the substance of those shells, formerly belonging to animals, has been dissolved or wasted by the penetrating force of juices, and that a stony substance is come in the place thereof,) but that these cockle-like stones ever were, as they are at present, lapides sui generis, and never any part of an animal. That they are so at present, is in effect confessed by Steno in the above cited page; and it is most certain, that our English quarry-shells have no parts of a different texture from the rock or quarry where they are taken, that is, that there is no such thing as shell in these resemblances of shells, but that iron-stone cockles are all iron-stone; lime or marble all lime-stone and marble; spar or crystalline-shells all spar, &c. and that they never were any part of an animal. My reason is, that quarries of different stone yield us quite different sorts or species of shells, not only one from another (as those cockle-stones of the iron-stone quarries of Adderton in Yorkshire differ from those found in the lead mines of the neighbouring mountains, and both these from that cockle-quarry of Wansford bridge in Northamptonshire, and all three from those to be found in the quarries about Gunthrope and Belvoir Castle, &c.;) but, I dare boldly say, from any thing in nature besides, that either the land, salt, or fresh water does yield us. It is true, that I have picked out of that one quarry of Wansford very resemblances of murices, telinæ, turbines, cochleæ, &c. and yet I am not convinced, when I particularly examined some of our English shores for shells, also the fresh waters and the fields, that I did ever meet with (N. B.) any one of those species of shells any where else, but in their respective quarries, whence I conclude them lapides sui generis, and that they were not cast in any animal mould, whose species or race is yet to be found in being at this day.

Further Communications about Vegetable Excrescences, and Ichneumon Worms. By Mr. Lister, N° 76, p. 2284.

See the preceding observations on similar subjects, of which this is a kind of continuation.

An Account of some Books. N° 76, p. 2286.

I. Johannis Wallisii, S. Th. D. Geometriæ Professoris in Academia Oxoniensi, Tractatûs de Motu Pars III. An. 1671, in 4to.

In this third and last part, the excellent author, continuing his doctrine of motion, treats, amongst other things, of the five mechanical powers, or noted engines for the facilitation of motion. There are also described six several forms of flat roofs, for large rooms, framed of short timbers, much shorter than the breadth of the room, mutually supporting one another: with methods of computing the weight sustained by every joint. Of several sorts of winchers, winders, capstans, rollers, wheels of engines, with or without teeth, boarers, cranes, &c. with the reason and measure of their force; as likewise the wheels of coaches, carts, or waggons, with the reason of divers circumstances in the fabric and use of them. Of pulleys; of the measure and reason of their strength. Of screws; with the reason of their great force, and the measure of it; as also, the measure of the length of the spiral line about a cylinder, whence the screw takes its rise, and of the solid content of the screw. Of the composition, acceleration, and retardation of motions; and the motion of projectiles, with the nature of the lines described by such motions. Of percussion or striking; with the measure and effects of blows, or strokes, according as the bodies striking do differently occur one to the other, and the centre of force in such percussion. Of the wedge, with the reason and measure of its force. Of repercussion, resilition, or reflection of bodies, which he derives from the spring or elastic virtue in them: Of hydrostatics, with the reason and measure of sinking or swimming of heavy bodies in a fluid: Of the weight and spring of the air, and the counterpoise of the atmosphere; from whence he derives those effects which were wont to be ascribed to the fuga vacui, or nature's abhorring a vacuity; with the explication and reason of the several phænomena of the Torricellian experiment, and others of like nature, and of the very great contraction and expansion of the air: Of raising a great weight with a man's breath, blowing a bladder. And the solution of divers other mechanical questions.

II. Danielis Ludovici, Medici Ducal. Saxo Gothani, de Pharmacia Moderno Seculo applicanda, Dissertationes III. Gothæ, 1671, in 12mo.

A pharmaceutical treatise, an account of which cannot be interesting to the medical reader of the present day.

III. Joh. Bapt. Sylvatici Institutio Medica de iis qui Morbum simulant deprehendendis. Francofurti ad Mænum. An. 1671, in 12mo.

A dissertation on feigned diseases, and on the methods of detecting such impostures.

IV. Quadripartitum Botanicum Simonis Pauli Medici Regii in Dania Argentorati, in 4to.

The learned author undertakes, in this work, to describe chiefly the uses and virtues of those simples or plants, which growing in Europe, are by long experience approved of; treating more particularly of those that grow in Denmark and Norway.

This description he performs in such a method, that he reduces the said plants to four classes alphabetically, according to the four seasons of the year wherein they grow or are in flower; that so it may be known when such and such plants are to be had and gathered for use; beginning from the winter plants, and thence proceeding to the vernal and æstival, and concluding with the autumnal.

Observations, made by Mr. Hooke, of some Spots in the Sun, which returned after they had passed over the upper Hemisphere of the Sun which is hid from us; according as was predicted. See N° 75, p. 631 of this Abridgement. N° 77, p. 2295.

August 30, 1671, about noon, I saw a large spot in the centre of the sun's face, but had not then time to make any more exact observation of it.

Sept. 1, at three o'clock, I saw the same spot moved about a quarter of the diameter of the sun westward, and it appeared to consist of one greater and two lesser black spots with a dusky cloud encompassing them: the diameter of the whole phenomenon was about $\frac{1}{72}$ of the diameter of the sun, and it was distant from the next adjoining limb $\frac{18}{72}$, that is exactly one quarter of the diameter of the sun. This I examined and measured several times, and found very exact.

Observations, made by the same, of the late Eclipse of the Moon, the 8th of September, 1671, Old St. N° 77, p. 2296.

September 8, 1671, at 7 h. 27½m. I first observed the moon eclipsed, when it began to be enlightened, the total darkness being already past. The shadow passed through the middle of the spot called by Hevelius M. Porphyrius, half of the said spot appearing without the shadow and the other half being darkened thereby.—At 7 h. 49 m. the shadow passed through the middle of M. Sinai, through the middle of the eastermost of the three lakes called Mare Adriaticum, and just touched the ridge of the Appennine mountains.—At 7 h. 54 m. it passed the middle of the J. Besbicus in the Propontis.—At 8 h. 0½ m. it passed through the straits of the Pontus Euxinus at the promontories Acherusia and Aristes.—At 8 h. 6½ m. it touched the Palus Mæotis, which Palus Mæotis was then distant from the limb of the moon next adjacent one third part of its shorter diameter or breadth.—At 8 h. 17 m. the shadow went off the body of the moon upon the innermost limb line of Hevelius's large chart of the moon at the 29th division, just without the J. Major of the Caspian Sea. The duskish penumbra left not the limb of the moon quite without some kind of darkness, till 8 h. 29 m. at which time I found that side of the moon which the shadow last left, was full as light and clear as the other.

About four or five minutes after the shadow was gone off, I perceived a faint representation of colours upon that part of the body of the moon which was most affected with the penumbra somewhat resembling the colours of a faint halo about the moon; this grew fainter and fainter, and after a few minutes was no more visible. It did not seem to be caused by any clouds or exhalations in the air, the sky near the moon being very clear, and the said colours not appearing any where, but upon the dusky parts of its phasis. Possibly it might be caused by the refraction of the light from the sun through the atmosphere about the earth.

Lunæ ad Fixas Appulsus Visibiles, nec non arctiores juxta eas Transitus, observabiles An. 1672: *Prædicti, et ad Meridianum Latitudinemque Londini, è Tabulis Carolinis accuratè supputati à* JOH. FLAMSTEDIO, *Derbiensi Anglo. N°* 77, *p.* 2297.

These prognostications are not now of any use.

Some Additions by Mr. LISTER *to his former Communications, about Vegetable Excrescences, and Ichneumon Wasps; with an Inquiry concerning Tarantulas, and a Discovery of another Musk-sented Insect. N°* 77, *p.* 3002.

You may take an occasion to put this quære to your correspondents of Italy, viz. Whether the tarantula be not a phalangium, that is, a six-eyed skipping spider, as Matthiolus and others seem to tell us? if so, whether some later authors impose not on us by giving us a cut or figure of a net or reticulum orbiculatum, which our English phalangia are never observed to weave, or make use of in hunting? and whether the person bit by a tarantula, be not ever, when on his feet, disposed to and actually dancing, after the nature of a phalangium, which never moves but by skippings; even as it happens with such as are bitten by a mad dog, who have been sometimes observed to bark like a dog, &c.? And if so, what we are to think and credit concerning such and such musical tunes, said to be most agreeable and tending to the cure of persons bit by a tarantula?

Among other things I had the good fortune to present Mr. Willoughby with a musk ant,* an insect observed by me not many days before his first visit: the note concerning which take as follows.

Sept. 2, I found in a sandy ditch bank, about a mile and a half from York, a sort of very small pismires.

* See of two or three more musk insects Number 74, and Number 76.

Those without wings were of a light yellow or flaxen, and being broken at one's nostrils they emitted, like others, an acid or sour scent; but those of the same bank with wings were coal black, and these bruised and smelt to, emitted a smell like musk, that it was too strong for me to endure: yet having kept them some time by me, the more delicate sex were not displeased with the smell.

Mr. Willoughby informed me, that he had found the goatchafer, or sweet beetle,* out of season as to that smell; and thereupon asked me, what I had observed as to the time of their sweetest and strongest smelling? I answered, that I believed it to be at the time of the coit, forasmuch as at that time, when I took them highly perfumed, I had observed the female full of egg.

Quere, Upon what parts or juices can the ichneumon worms, supposed to be thrust into caterpillars and other maggots, be thought to feed? may there not be actually eggs in caterpillars and maggots, as there are to be observed in their respective chrysalises sufficient to serve them for food? Concerning the name ιχνεύμων, although I could willingly refer you to Mr. Ray, who is another Hesychius; yet for present satisfaction I shall transcribe what the excellent critic G. Vossius says, (c. 16. de Inimicitia;) Ichneumon (i. e. Mus Pharaonis sive Ægyptiacus) Crocodili et Aspidis ova indagat, unde illi Ichneumonis nomen, quasi dicas Indagatorem (ἀπὸ τȣ̃ ἰχνεύειν:) Reperta utriusque ova conterit; ut est apud Oppianum in 30 de Venatione: Nicander tamen ait, eum aspidis ova humi mandare.

Now a like observation of certain insects of the wasp kind, made no doubt by some of the ancients, occasioned the application of that name to wasps as well as to that Egyptian mouse. Yet cannot I remember to have met with, in any of the ancients, more than one text concerning those wasps; viz. Aristot. de Hist. Anim. l. 5. c. 20. which Pliny (vid. lib. 11. c. 21.) has rendered, in a manner verbatim, thus: Vespæ, quæ Ichneumones vocantur (sunt autem minores quàm aliæ) unum genus ex araneis perimunt, phalangium appellatum, et in nidos suos ferunt; deinde illinunt, et ex iis, incubando, suum genus procreant.

How far this relation is true, and agreeable to modern observations, we shall have perhaps occasion to discourse of elsewhere; our design here is only to tell you, that we have enough to make us believe, that those very insects, we have been treating of, are in species the Ichneumons of the ancients.

* Of which see Number 74, and Number 76.

An Account of some Books. N° 77, p. 3006.

I. Scheeps Bouw en Bestier, that is, Naval Architecture and Conduct; by N. Witsen. Printed at Amsterdam, 1671, in folio.

The ingenious and industrious author of this work treats largely, not only of the manner of the naval architecture used by the Greeks and Romans, with their naval exercises, battles, discipline, laws and customs; but also of the method and way used at this day both in his own country, England, France, and the Indies, with the difference between the manner of building ships, practised by others, from that of the Dutch, and particularly of the Indian way of equipping their ships, and the manner of building galleys: All enriched with an ample seaman's dictionary, and a great number of illustrating diagrams.

The whole work is divided into two main parts; the first contains 19 chapters; whereof, the 1st gives an account of the first builders of ships, and in general of the building of the ancients, both before and after the deluge; where the author particularly discourses of Noah's Ark; of divers ships found deep under ground; of the structure of the ship Argo; of the navigation of the Phenicians, Rhodians, Corinthians, Egyptians, Tyrians, Cretans, &c. —2. Delivers the method of naval architecture of the Greeks and Romans, both for war and commerce, together with the manner of equipping their ships rowed with oars, both of single and multiple ranks, and the sitting of the rowers.—3. Discourses of several sorts of the ancients' structure of ships, and chiefly of the great vessels built by Philopater and Hiero, the pompous make of both which is here represented; as also of the number and launching of their ships.—4. Enumerates divers uncommon observables in ships, both of ancient and later times, as in Noah's Ark, the ships of Argo, Theoris, Paralon, Salamine, Magellan, Drake, &c. To which he adds that noble frigate built in England An. 1637, called the Sovereign, of 1637 tuns, having a keel that was to be drawn by 28 oxen and 4 horses; as also a description of the Spanish Armada of 1588, called the Invincible; not forgetting the Bucentoro of the Venetians; nor the Mageleza of the Swedes, a man of war appearing at sea about 100 years since, and having sides of that thickness, that all cannon balls stuck within her boards. In this chapter is inserted a relation of a ship found in the time of Pius II. in the Numidian Sea, 12 fathoms under water, 30 feet long and of a proportionable breadth, built of cyprus and larix wood, and reduced to that hardness, that it would scarcely burn; and was difficult to cut: no signs in it of any rottenness; its deck covered with paper, linen and leaden plates, fastened with gilt nails, as also were the boards; the whole ship so close, that not a drop of water was found soaked through into any close room. The author concludes it to have lain there about 1400 years.—5. Relates what great fleets were anciently set out, and what distant voyages undertaken: where he takes particular notice of the expedition of the Argonauts, of Xerxes, of Alexander, of Rome, and Carthage, of the Saxons, Britons, &c.—6. Describes what the ancients observed in building their ships, and how they closed, rigged, and beautified them; where occur several relations of divers ways of cementing, caulking, pitching, and defending ships from rottenness and worms; of which I shall only mention, what occasionally he alleges of a certain cement now used by the Indians, made of finely beaten reeds, chalk and oil, with which their ships are overlaid to keep them from rotting.—7. Rehearses the state of naval architecture after the ruin of the Roman empire; especially amongst the Scythians and Saracens invading Italy, Spain, France, &c; with the endeavours of the Romans, under Justinian and others, to defend themselves against those barbarians: not omitting what was done by the Danes, Huns, English, Saxons, and particularly by that brave and vigilant King Edgar, who maintained a fleet of 3600 sail, which he divided into three squadrons, called the eastern, western, and northern, sailing in them himself every year round about England and Scotland. To this he annexes at what time shipping was at the lowest ebb, and how it began to be restored by some kings of Portugal, the Frieslanders, and his countrymen in general, about 200 years since. —8. Gives an ample and very particular account of the present way of building ships, both for war and trade in Holland.—9. Contains a particular description of the proportions of all the parts of a Dutch ship, and the measures of some peculiar sorts of vessels of that country.—10. Declares the make and weight of all sorts of anchors, and the bigness and weight of cables in general, and in particular of certain ships built there; as also the measures and proportions of masts, and sails, of divers vessels, and how sails may be best ordered to take in most wind, mathematically shown: Where occasion is taken to insert considerable remarks about the several sorts of hemp, and the best way of working cables, and the care to be had in the manner of tarring them, and in the degree of heating the tar for that purpose, &c.—11. Delivers the method of conjoining the parts of a ship one after another, used by Dutch shipwrights; with a representation of a ship upon the stocks, and their manner of launching ships: Adding their way of redressing a ship that lies on her side, as well as of laying her on her side for repairing or cleansing.—12. Speaks of the measures and proportions of several other sea vessels, that are of a structure and use different from that of the former; such as are flutes, Greenland vessels for whale-fishing, advice-yachts, boyars, galliots, fire-ships, pinks, busses, &c.—13. Treats of other sorts of vessels, as coasters, yachts, shallops, lighters, boats, skiffs, double-bottomed vessels, ships rising without being unladen, and such as move under water, or against the stream, and especially of a vessel used at Amsterdam, whereby in one day may be fetched up 50 or 60 boats of mud, performed by the means of a large wheel and large spoons. In the same chapter, instructions are given concerning the choice of ship timber.—14. Considers the structure of galleys and galleasses, &c.—15. Discourses of the proportions observed by the English and French in the building of their respective ships.—16. A narrative of the Indian way of framing ships: Where first of all occur the canoes and their structure out of a single tree, hollowed by burning. Next the Chinese yonks of Nankin, a sort of flat-bottomed boats, and other vessels of the same country; among which those are described that are as large as little islands, and hold many houses and families, floating upon the waters, and going up and down through all the parts of China that have the conveniency of navigable rivers. Then the ships of Malabar, Ternate, Sumatra, Japan, Terra del Fuego, of Borneo and Calecut. After this, the author returns to China, and relates that ships are found there, which upon rollers sail over land; and gives a large account of the vast number of ships, both warlike and mercantile, maintained in that empire. He concludes this chapter with describing the ships of Madagascar, Bengal, Macassar, Siam, Pegu, Maldives, Ormus, Congo, Russia, Lapland, Virginia, &c.—17. Demonstrates how much weight of water lies against a ship moving at sea. He examines also the centre of gravity of a ship; which being known, it may be certainly concluded, how a ship is to lie upon the water, and how heavy it is when it is floating, whether loaden or unloaden. Lastly, he imparts Hudde's method of calculating exactly what burden a ship can carry either in salt or sweet water. Also several ways of measuring a ship's burden, used by other nations, and particularly that of the English, French, Dutch and Danish.—18. Explains and gives reasons for the several sizes and shapes of the parts of a ship. As, why the masts ought just to be of such a bulk and height? Why some of them must incline backward, some stand upright? Why a small rudder can turn a great ship; and a little anchor stay it? What makes ships not feel the rudder? Why vessels too broad are weak and prove inconvenient in high winds? Why long and moderately narrow ships endure the sea better than short and broad ones? How the keel ought to be placed? Why galleons and the parts of them are framed as they are? Why a ship is to be broader before than abaft? That frigates built long, narrow and low, sail best? What hinders well sailing? Why Turkish vessels are excellent sailers? And many questions more, considered by this author.—19. Reckons up the particulars of the loose apparatus necessary in a moderately far voyage for a hundred men, in a ship 134 feet long, both for her conduct and defence, and the food of the mariners.

The second part comprehends the equipping and conduct of ships and navies, as well by the ancients as moderns, in several chapters.

II. Recherches et Observations sur les Viperes, faites par Monsieur Bourdelot.* A Paris, 1671, in 12mo.

This short discourse is an answer to a letter, which the learned author had received from Signor Redi, first physician to the Great Duke of Florence. In it M. Bourdelot declares, that though Signor Redi's letter does not finally decide the matter in question, yet it is very useful to the farther knowledge of the nature of vipers by the particularities by him recited. The controversy being, whether the yellow liquor about the long and crooked teeth of vipers is, even when they are not irritated, venomous; (which is affirmed by Signor Redi,) or, whether it be a simple innoxious saliva or spittle, as is maintained by M. Charas: this author observes, that that liquor controverted is not yellow in French vipers, as it is in those of Italy: which remark he makes use of to the advantage of the often mentioned Redi, who would reconcile these two opinions by suggesting, that the vipers of Italy and France are differently disposed; countenancing this observation with what he has taken notice of, that the venom of the lues venerea is much more malign in hotter than colder countries; and also with what is constantly related by voyagers, viz. that animals are more venomous in Africa than elsewhere. But that notwithstanding this, the objection made by M. Charas seems not cogent, when he speaks of a viper's teeth, whose bite proved mortal, although the teeth had been rubbed and perfectly dried with bread crumbs; whereby he would support that experiment, in which he caused to be bitten and killed 7 or 8 animals one after another, of which the last bitten died first; it seeming impossible to him, that there should be remaining any of that salival juice about his teeth after so many bitings; and that therefore, to give a cause of that death, recourse must be

* Peter Joseph Michon, better known by the name of the Abbé Bourdelot, was born in France according to some biographers, according to others at Geneva, in 1610. He was educated in the medical profession, and became physician to Queen Christina of Sweden, with whom he had influence enough to prevail upon her to embrace the catholic religion; for which act he was rewarded with an abbacy. Quitting the service of the Queen of Sweden, he repaired to Paris, and was appointed physician to Lewis XIV. He died at Paris in 1685, through a mistake of his servant, who gave him a strong dose of opium instead of a purgative medicine. Philosophical and medical meetings used to be held at his house, of which he left some MS. minutes that were published after his death under the title of Conversations de l'Academie de M. l'Abbé Bourdelot, recueillies par le S. Gallois, Par. 1675.

had to the fierceness of the spirits, transmitted to the crooked teeth, to be revenged of those against whom these beasts are provoked; which angry spirits, being thrust into the flesh and veins, infect the spirits and blood of those that are bitten. To which our author answers, that it is hard to maintain, that the vindicative spirits can pass through a body so solid, as teeth are, especially since the little teeth have been found by experience to cause as dangerous effects as the great ones, after these had been broken out; and that therefore it may be justly doubted, whether by the said bread crumbs all the salival liquor about the teeth of an animal alive could be taken away, as it may be truly affirmed, that the viper's teeth are incessantly plunged into their sheaths, and do there continually fill themselves with the said juice.

But he considers withal, that in hot countries this liquor may work alone, when conveyed into our flesh by the teeth of a dead viper, or even with an ear picker, into a wound; as it comes to pass in Italy and in hot countries; but in France and in colder parts, especially such vipers being used as are kept in tuns and brought from afar off, the said juice not being strong enough alone, needs to be made keen by the bilious breath of the angred viper.*

III. Admirandorum Fossilium, quæ in tractu Hildesheimensi reperiuntur, Descriptio, Iconibus illustrata, à D. Friderico Lachmund, Hildesheimi, 1669, in 4to.

This description contains several things that may increase the materials for a history of nature. This author delivers what he has met with in the country above-mentioned, in four sections. In the first he discourses of the earths there found: as marls, clays, fullers earth, tripoli, black chalk, a vermilion earth, melting like butter on the tongue, and oker. In the second, of concrete juices, as salt, nitre, alum, vitriol, sulphur, bitumen, &c. In the third, of stones, especially spars, touch-stones, marble, the lapis specularis, blood-stone, schistus, lapis samioides, crystal, brontia and ceraunia, (vulgarly called thunder-stones) several sorts of belemnites, some of which being rubbed smell like burnt horn; eagle-stones, cornu ammonis, &c. In the fourth, of some uncommon springs.

IV. De Catarrhis, A. Rich Lower, M.D. in 8vo.

We mention this book only to give notice, that it is now printed by itself in England; referring the reader for the account of the contents thereof to N° 73, where it was spoken of, when we saw it printed in Holland, together with the author's book De Motu Cordis et Sanguinis.

* It is amusing enough to see that this writer should not be able to divest himself of the idea of "the bilious breath of the angred vipers," while he admits the existence of the yellow liquor at the roots of the teeth.

V. Goth. Voigtii Deliciæ Physicæ. Rostochii, A. 1671, in 8vo.

This author entertains his readers with divers marvellous subjects, such as are the bleeding of persons killed, at the presence of the murderer; the tears of crocodiles; the licking of new whelped bears by their dams; the love between wolves and sheep; fossil fishes; the casting of horns by deer, &c.

The Observations of the Spots of the Sun, made at the Royal Academy at Paris, continued. N° 78, p. 3020.*

The observation of these solar spots having been noted in the first paper as far as the 13th of August, the present paper continued the account of them as far as to the 19th of the same month. From which it is inferred that the apparent velocity of the spots, when they approached to the sun's centre, gave room to determine their apparent periodical revolution about the sun's axis, about $27\frac{1}{2}$ days,† supposing them to be adherent to his surface, or at least very nigh to it; and consequently that from the morning of the 13th of August, when they were near his centre, they should take between six and seven days to arrive at the limb of his apparent disk, which has come to pass accordingly. For, since the morning of the 13th unto the evening of the 19th, when they were seen nigh the limb, there are $6\frac{1}{2}$ days; and then they were yet so far distant from it that it was easy to judge they would not come out that day. The clouds and night then hindered the observation, but in the morning of the 20th, which was not the full seventh from the day that they were arrived to the middle of the disk, they had disappeared. This likewise agrees well enough with what had been practised, viz. that these spots during the fourth part of the time of their motion about the sun's centre, calculated according to this hypothesis, and upon the first observations, would remain in the western quadrant.

The apparent velocity near the centre was such, that if it had continued the same, the spots would have arrived almost in four days to the limb of the disk; but in this hypothesis this apparent velocity was to lessen according as the spots should remove from the centre; as has come to pass in effect. The diminution of the length of the misty crown was in a manner proportionate to the diminution of the apparent velocity; since that, when this crown was in the middle, and in a situation wherein its true figure could be best seen, it appeared oblong, and of the form of the human ear, its greatest diameter respecting east and west; but being nigh the limb, this same diameter seemed to shorten; and having

* See the beginning of them in No. 75.

† By later, and more correct observations, the sun's rotation on his axis has been stated at 25 days 10 hours.

appeared greatest in its first situation, it appeared least in this, because it was almost in a circle that passed through the centre of the sun, whose equal arches are by so much the more oblique, by how much they approach more to the limb of his disk, and consequently appear less, according to the rules of optics; mean time the diameter, that was turned from south to north, apparently kept the same bigness it had near the centre, because it was in a circle almost parallel to the horizon of the sun, which formed the representation of its limb, and whose equal arches, by the same optical reasons, do not appear contracted..

Observations concerning Saturn, made in the same Place with the former. N° 78, *p.* 3024.

At the same time that the new spots of the sun began to appear, Signor Cassini observed in Saturn also something remarkable in regard of the unexpected change of his figure. Astronomers know, that this planet is for the most part seen with arms or anses, fastened to the two sides of his disk, when he is observed with some large telescope; and that he gains his round figure only every 15th year. This change was to come to pass this present year, and Saturn to appear in that round figure, without his anses or handles, according to the hypothesis and predictions of M. Huygens, published An. 1659; which indeed has so happened, but not just within the time he had appointed; for this spherical figure of Saturn should not have appeared, according to his suppositions, but in the month of July and August, and so continued for the rest of the time that Saturn was to be visible at this time, and even for a part of his appearance in the next year; but this roundness has been perceived sooner, and Saturn has appeared orbicular since the end of May, at a time when he was distant enough from the sun and the horizon to be well observed; and he has remained in this figure to the 11th of August. Signor Cassini then observed him thus; but three days after he saw him with arms, though very narrow ones, which still continue.* They are represented in pl. 14, fig. 12.

Extract of two Letters from M. Hevelius, of June 19 *and Oct.* 7, 1671, *concerning several Celestial Phænomena. Translated from the Latin.* N° 78, *p.* 3027.

These communications contain some of Hevelius's late celestial observations;

* Mr. Huygens ascribes their being able to perceive the anses, though very narrow, to the long and powerful telescopes used in these observations; for that, with the former short ones, the edge of the ring would not be visible. So that his prediction of the planet's appearance, according to his hypothesis, is not in the least discredited.

viz. Saturn occulted by the moon; a lunar eclipse; occultation of Jupiter's first satellite; and the phases of Saturn and his ring. Of each of these in their order.

1. *Saturn occulted by the moon.*—Beginning of the occultation at Dantzick, June 1, (new stile) 1671, was at 3h. 38m. 27s. in the morning, near the mountain Germanicianus. The line of transit, as far as could be collected from the ingress alone, was through mount Ætna, near the moon's centre; then by mount Herminium, mount Hercules, and the upper part of Mare Caspium. Excepting this year's observation, within 41 years, as far as I remember, I have seen Saturn only two times covered by the moon; first, in the year 1630, Jan. 29, at 11h. at night, when I was at the island Huanna in the Danish sea; and again in the year 1661, Aug. 3, at 7h. 58m. 20s. in the evening, here at Dantzick.

2. *A total eclipse of the moon, An.* 1671.—This lunar eclipse was observed by Hevelius at Dantzick, Sept. 18, new stile, in the evening. He was prevented from seeing the beginning and most part of the eclipse, by rain the whole day; till 8h. 30m. after noon, when, through an opening in the thick clouds, he perceived the moon rather obscurely, but so much enlightened, that the eclipse seemed to have been quite over, at least that the interval of total eclipse was past, and the moon recovering her light again; so that he concluded the eclipse must have happened above half an hour sooner than Kepler's calculation made it. At 8h. 34m. he observed the moon had got out of the shadow a whole digit at least, and again, at 9h. 41m. the moon's light had increased to $1\frac{1}{2}$ digit, as near as could be judged.

3. *Occultation of Jupiter's first satellite by his shadow.*—This was observed by Hevelius at Dantzick, 1671, Sept. 7, new stile, in the morning. It had been agreed on, by M. Cassini at Paris, and M. Picard at Uraniburg, to observe this occultation of Jupiter's first satellite; and therefore, says Hevelius, I also thought proper to attend to the same phenomenon. Hence, as soon as Jupiter appeared at 4h. 27m. I found that all his four satellites were visible also, three on the left hand and one on the right, as seen through my tube, which shows objects in an inverted order. The two which were nearest him on the left seemed to be not far from his limb, as also that on the right, which was the least of them all. At 5h. 12m. beyond my expectation, the nearest satellite on the left hand seemed entirely to vanish; the three others remaining, though that on the right hand also seemed to approach nearer and nearer to Jupiter. Whether, indeed, that was the very moment of the immersion of the former satellite I dare not certainly affirm, at least however that occultation did not

happen later, although I grant that it might perhaps happen about a minute sooner.

The Observations.		The Times corrected.		
Jupiter was first seen		4^h	32^m	7^s
The altitude of the star Procyon..........	34° 43′	5	2	7
The first of the satellites disappeared		5	12	0
The altitude of Procyon	36 39	5	23	27

4. *Transit of the moon above Jupiter.*—This he observed at Dantzick, An. 1671, Sept. 30, in the morning, new stile. In a very clear sky, with a tube of 20 feet, with great eagerness he observed the rising moon, and Jupiter a little after. He found by his large brass octant, of near 9 feet radius, that Jupiter was yet at 1° 23′ 40″ distance from the eastern limb of the moon, and that all his four satellites were seen towards the right hand, from whence the moon approached. An unfortunate accident prevented him from taking the very moment of conjunction. For when Jupiter now approached within 3′ of the moon's eastern limb, and was only 6′ from the line of conjunction drawn through both horns, some little clouds intervening prevented him from seeing both Jupiter and the moon. The Rudolphine tables indicated an occultation, and that much sooner than it could happen, yet there was none, but only a very near approach, the moon being a little above Jupiter, by about 2 digits, the time of which was 7h. 26m.

5. *The Appearance of Saturn and his ring.*—According to the hypothesis of M. Huygens, Saturn was to resume his round figure in the summer of 1671, the ring not appearing, being turned edgeways towards the earth. It was observed by Hevelius and others. How it appeared, says he, Sept. 11, new stile, I have delineated very truly and carefully, and have here sent you the scheme (see fig. 12, pl. 14.) But in the months of June, July, and August, that you should see it quite round, as the Parisians affirm, I can hardly imagine. For though the arms of Saturn might appear very close at the sides, even in a tube of 60 or 70 feet, yet I can hardly think they could quite vanish, so that no remains of them could be seen. Perhaps those gentlemen might view Saturn with short telescopes during the twilight when the moon was up.

Extract of a Letter from Dr. Fogelius, dated Hamburg, Nov. 1, 1671, concerning the Spots of the Sun returned, and the last Eclipse of the Moon. Observed by Henry Sifer. Translated from the Latin. N° 78, p. 3033.

We found here, as M. Hevelius had done at Dantzick, that the Rudolphine tables were not correct, as to the late eclipse of the moon, Sept. the 8th. We found here also, that the moon emerged out of the shadow of the earth before 9 o'clock.

The solar spots we have observed at Hamburg, from Aug. 26, old stile, being nearly the day they first began to appear, as far as to Sept. 5, when they approached very near to his limb. In which interval they traversed nearly over the whole solar disk, by the centre of it.

Extracts of two Letters from Mr. John Flamsteed, on some late Appearances of Saturn: dated Derby, Nov. 21 and Dec. 2, 1671, N° 78, p. 3034.

Oct. 12, at my first viewing Saturn with my smaller tube, I thought I saw something on each side of him, amidst the colours of my glass and the spurious rays of his body. Directing my longer tube, of 14 feet, to him, I could see his anses somewhat more distinctly, but very slender, and, to one that thought not of them, scarcely discernible.

Nov. 30, I observed Saturn with my 14 feet telescope, the aperture being $1\frac{1}{2}$ inch, and its eye-glass drawing two inches. He appeared perfectly round, free from rays and colours, and no ansæ to be seen. My worthy friend Mr. Townly, in his last to me, of Nov. 20, 1671, desires me to continue the observations of Saturn; telling me that he looked at him one night, and could hardly distinguish his line of the ansulæ, but plainly saw a dark line through him near his upper part.

A week or two after the observation of Saturn, which I made Oct. 12, and sent you in my former, I had frequently the same appearance, though in a wider aperture than I use at present, &c.

Some further Observations of Mr. John Templer, about the Shining of Glow-Worms. N° 78, p. 3035.*

Nothing in this paper worth reprinting.

An Account of some Books. N° 78, p. 3037.

I. The Anatomy of Vegetables begun; with a general account of Vegetation, founded thereon: by Nehemiah Grew,† M. D. Fellow of the Royal Society. 1671, in 12mo.

* See Number 72, of these Tracts.

† Nehemiah Grew, an ingenious and learned physician, was the son of Mr. Obadiah Grew, sometime minister of the parish of St. Michael in Coventry. Having been sent to a foreign univer-

The ingenious and learned author of this book considering with himself, that the anatomy of vegetables has hitherto been little cultivated, though very well deserving the labours of diligent naturalists, has here made a particular inquiry into the constitution and structure of plants, and thereon founded a rational discourse concerning the nature of vegetation. And he advises those that shall think fit to examine his observations, not only that they begin, and so proceed till the end again, with the seed; but also, that they confine not their inquiries to one time of the year, but to make them in several seasons, wherein the parts of a vegetable may be seen in their several states: and then, that they neglect not the comparative anatomy, confronting several vegetables and their several parts together.

The method he chuses in the prosecution of this subject, is the method of nature herself, in her continued series of vegetations, proceeding from the seed sown, to the formation of the root, trunk, branch, leaf, flower, fruit; and lastly, of the seed to be sown again, or in its state of generation.

Discoursing of the seed as vegetating, he dissects a garden-bean, and shows its two coats; the foramen in the outer coat; and what is generally observable of the covers of the seed. This done, he displays the proper seed itself, and therein finds three constituent and as it were organical parts of the bean, viz. the main body, always divided into two lobes, though in some few other seeds into more; and two other appendant to the basis of the bean; whereof the one is called by him the radicle, being that which, on the vegetation of the seed, becomes the root; the other the plume, which becomes the trunk of the

sity for some years, he returned, after taking the degree of doctor of physic, to London, where he became a candidate for an honorary fellowship in the college of physicians, and was admitted on the 30th of September, 1680. He obtained extensive practice; was elected a fellow of the Royal Society; and, on the death of Mr. Oldenburg, succeeded to the office of secretary; in consequence of which he carried on the publication of the Philosophical Transactions for a considerable time. He also drew up a catalogue of the articles in the museum of the society, which he finished in folio under the title of *Museum Regalis Societatis*. This publication (though by no means free from mistakes) is, upon the whole, a work of very considerable merit, and is remarkable for an ingenious scheme or disposition of shells.

To this is generally appended a work entitled The Comparative Anatomy of Stomachs and Guts, being several lectures read before the Royal Society in 1676. The work however by which Grew is most deservedly celebrated, is his *Anatomy of Plants*, in which he has shown a wonderful degree of ingenuity. This work is accompanied by very numerous and well executed engravings, and may be considered as one of the most curious performances of the seventeenth century.

Another very celebrated publication of this author, is the *Cosmologia Sacra*, or "*A Discourse of the Universe, as it is the Creature and Kingdom of God.*" This was chiefly composed to demonstrate the truth and excellence of the sacred writings.

Dr. Grew died, after a very short illness, on the 25th of March 1711.

plant; and being divided at its loose end into divers pieces, all very close set together, as feathers in a bunch, these pieces are so many true and already formed, though not displayed, leaves, intended for the said trunk, and folded up in the same plicature wherein they appear on the bean's sprouting. These organical parts he finds composed of these similar ones, viz. 1. The cuticle, extending itself over the whole bean, and herein distinguished from the coats, that whereas these, on setting the bean, only administer the sap, and then die; the cuticle is with the organical parts of the bean nourished, augmented, and co-extended. 2. The parenchyma itself, having some similitude to the pith, while sappy in the roots and trunks of plants; common to, and the same in the lobes, radicle and plume of the bean. 3. The inner body distributed throughout the parenchyma, but yet essentially different from it; called by the author the seminal root, and distinguished from the radicle, in that the former is the original root within its seed, the latter is the plant-root, which the radicle becomes in its growth; the parenchyma of the seed being as it were the same thing to the seminal root at first, which the mould is to the plant-root afterwards; and the seminal root being that to the plant-root, which the plant-root is to the trunk. Having viewed these parts, he inquires into their use, and in what manner they are the fountain of vegetation, and concurrent to the being of the future plant.

Proceeding to the root, which he finds substantially one with the radicle, as are the parts of an old man with those of a fœtus, he therein observes its skin, cortical body, and lignous part, with the original of each of these, and the pores of the two latter, and their proportions; as also the pith, and its original, sometimes from the seed, sometimes from the cortical body, with its pores and proportions: likewise the fibres of the lignous body dispersed through the pith, and the cavity and pith of those fibres. Where he explains how the root grows, and what is the use of its parts; how it grows in length and breadth; and how it descends; adding the use of the pith, viz. for the better advancement of the sap, and its quicker and higher fermentation, begun in the cortical body, inserted through the lignous part, by which insertions the sap, like the blood of the disseminations of the arteries, is conveyed to its intimate parts: our author conjecturing, that the design of all these parts is the circulation of the sap.

Having thus declared the degrees of vegetation in the root, he next shows the continuance thereof in the trunk; the observables and parts of which are, 1. The skin, derived from the cuticle of the seed: 2. The cortical body, originated from the parenchyma of the seed: 3. The lignous body, being the prolongation of the inner body, distributed in the lobes and plume of the seed:

4. The insertment and pith, proceeding also from the plume, as the same in the root from the radicle; so that, as to their substantial parts, the lobes of the seed, the radicle and plume, the root and trunk, are all one. Here notice is taken of the shooting of the lignous body in breadth; wherein are observable its fibres, production of rings, and especially pores; and these of three sorts, greater, lesser, and least of all; all continuous and prolonged by the length of the trunk: which he proves by an experiment made by Mr. Hook, by filling up (suppose in a piece of charcoal) all the said pores with mercury, which appears to pass quite through them, as is visible by a good glass. The result of all is, that the woody part of a vegetable is nothing else but a cluster of innumerable and extraordinary small vessels or concave fibres. He farther shows the insertions of the cortical body in the trunk, and the pores of those insertions; in none of which pores he could observe any thing that may have the true nature and use of valves; the non-existence of which he is asserting. He discourses also of the position and course of the pores: and concludes this chapter by declaring, how the trunk ascends; how its parts, in consequence of that ascent, are disposed; how that disposition is consequent to the different nature and energy of the sap; what the effects are of that difference; which way, and how the sap ascends, viz. by the joint subserviency of the lignous and the cortical body in some, but in most, and principally, of the lignous body and pith; the latter being here considered as a curious filtre of nature's own contrivance: where he examines, how the pores of the pith are permeable; and renders a reason why a piece of dry elder pith, set in some tinged liquor, the liquor does not then penetrate the pores, so as to ascend through the body of the pith? To this part is annexed, by way of appendix, some considerations of the trunk-roots and claspers, and the use of both.

After this, he proceeds to the germen, branch, and leaf, and finds in the two former the same parts with those of the trunk, viz. the same skin, and the same cortical and lignous bodies, as also the same insertment and pith, hereinto propagated, and distinctly observable in it. Further, he shows the manner of their growth and nutrition, and how the germen is secured; as also the use of the knots. Then he lays open the parts of a leaf, and explains the positions of the fibres in the stalks of leaves, and the cause of their different shape, and of their being flat. Then he discourses of the folds of leaves, their kinds and use, with the uses of the leaf itself. To this chapter also he makes an appendix, concerning thorns, hairs and globulets, explaining both their constitution and use.

Next he gives an account of the flower, and its three general parts, the empalement, the foliation, and the attire; explaining the formation, nature, and uses of all three, but most particularly of the attire, which he finds to be of two kinds, seminy and flory; the seminy, made up of two parts, chives and semets, the latter of which are hollow, yet not so but that they are filled up with minute particles, like a powder. The florid attire is commonly called thrumbs, which are several suits, of which this attire is made up: the outer part of every suit, is its floret, which is the epitome of a flower, and in many plants all the flower. The next part is from within its tube brought to sight, and is called the sheath, likewise concave. The third part, and the innermost of the suit, is the blade, which is solid, yet at its point always divided into two halves; on which division there appears a powder of globulets, of the same nature with those of a semet. The use of the attire he assigns to be, not only ornament and distinction to us, but also food to a vast number of little animals, who have their peculiar provisions stored up in these attires of flowers; each flower becoming their lodging and dining-room, both in one: though it cannot as yet be determined, wherein the particular parts of the attire may be more distinctly serviceable, this to one animal, that to another; or to the same animal, as a bee, whether this for the honey, another for their bread, a third for the wax; or whether all do only suck from hence some juice, or some may not also carry some of the parts, as the globulet, wholly away, &c.

In the following chapter he treats of the fruit, considering the number, constitution, and original of the parts of an apple, bean, plum, nut, and berry; and observing, that the general composition of all fruits is one, that is, their essential and vital parts, are in all the same, and but the continuation of those, which in the other parts of a vegetable he has already taken notice of. To which he subjoins the uses of fruits, both for man and beast, as also for the seed; to which latter it serves for supply of sap, and for protection and security, the whole fruit being, by comprehension, that to the seed, what the hen, by incubation, is to the egg or chick.

In the last chapter he considers the seed again, but in its state of generation; as he before examined it in its state apt for vegetation: where occurs, what in the other state was either not distinctly existent, or not so apparent, or not so intelligible. As first the case of the seed, and its outer coat, their figures, various surface and mucilages; with the nature of the outer coat and its original: then the original and nature of the inner coat, in which the lignous body or seed branch is described. On which he observes, that all the parts of a vegetable, the root, trunk, branch, leaf, flower, fruit, and seed, are still made up of two substantially different bodies; and that, as every part has two, so the whole vegetable, taken together, is a compound of two only, and no more; all properly woody parts, strings and fibres, being one body; all simple barks, piths, parenchymas and pulps, and, for substance, pills and skins also, all but one body: the several parts of a vegetable differing from each other only by the various proportions and mixtures, and variously sized pores of these two bodies.

But to return, besides these three covers, he finds a fourth, which is the innermost, called by him the secundine, the concave of which membrane is filled with a transparent liquor, out of which the seed is formed. Through this membrane, the ligneous body or seed branches, distributed in the inner coat, at last shoot downright two slender fibres, like two navels, one into each lobe of the bean: these fibres, from the superficies of each lobe, descend a little way directly down; and then presently each is divided into two branches, one distributed into the lobes, the other into the radicle and plume.

As for the generation of the seed, dependent on the history delivered, he says, that the sap, having in the root, trunk and leaves, passed divers concoctions and separations, in the manner by him described, it is at last, in some good maturity, advanced towards the seed. The more copious and cruder part hereof is again separated by a free reception into the fruit, or other part analogous to it. The more essential part is entertained in the seed branches, which being considerably long and very fine, the sap becomes therein, as in the spermatic vessels, still more mature. From hence it is next delivered up into the coats of the seed, as into a womb, and the meaner part hereof is again discharged to the outer coat, as proper aliment; the finer is transmitted to the inner, which being a parenchymous and more spacious body, the sap therefore is not herein a mere aliment, but in order to its being farther prepared by fermentation. The sap being thus prepared in the inner coat, as a liquor now apt to be the matter of the future seed embryo, by fresh supplies is thence discharged or filtred, or transpired through the secundine abovementioned; and the depositure thereof, answerable to the colliquamentum in an egg, or to the semen muliebre, is at last made into the concave of the same. The other part of the purest sap, imbosomed in the ramulets of the seed branch, runs a circle, and so becomes as the semen masculinum, yet more elaborate. With this purest sap the said ramulets being supplied from thence, at last the navel fibres shoot, as the artery into the colliquamentum, through the secundine into the aforesaid liquor, deposited therein. Into which liquor being now shot, and its own proper sap or tinctures mixed therewith, it strikes it thus into a coagulum, or into a body consistent and truly parenchymous. And in the interim of the coagulation, a gentle fermentation being also made, the said parenchyma or coagulum becomes such, not of any constitution indifferently, but is raised, as we see bread in baking, into a congeries of fixed bubbles; the parenchyma of the whole seed being such.

II. Dissertations sur la Nature du Froid et du Chaud; par le Sieur Petit, Conseiller du Roi, Intendant des Fortifications, &c. Avec un Discours sur la Construction et l'Usage d'un Cylindre Arithmetique, inventé par le même Auteur. A Paris, 1671.

The famous author of these two tracts, examines in the former, first, the nature, subject, cause, and effects of cold. As to its nature, he considers it a positive thing, and not a mere privation, the effects of it being as sensible to us as those of heat. For the subject of it, he places the supreme cold in the pure air, and makes the heat, that is sometimes in the air, merely adventitious, produced in it by the sun, but the cold natural to the same. Concerning the cause of this cold in the air, he will not acknowledge it to be nitre, all sorts of salt being by him esteemed hot, much less an universal spirit, or any stars; but esteems, that the air is the *primum frigidum* by nature; the same cause, that has made it air, having made it cold.

Secondly, he discourses of fire, after he has discarded it from being one of the common four elements, and dislodged it from its reputed place above the air under the concave of the moon: And affirms, that it is the heat of the sun, which moves, quickens, and coagulates the three families of mixts, animals, vegetables and minerals, yet taking in a subterraneous heat for the production of minerals.

In the latter part of this book, the author explains an invention of his, the arithmetical cylinder. This is a contrivance for making arithmetical calculations after the manner of Napier's rods. Instead of these, the author writes the numbers on narrow slips of paper, pasted lengthwise on a cylinder of wood or pasteboard, having some small knobs on the surface, by means of which the cylinder is turned round, and thus the arithmetical calculation is performed.

III. La Dioptrique Oculaire, par le Pere Cherubin* d'Orleans, Capucin. A Paris, 1671, in fol.

The author of this large and elegant volume, having proposed to himself to comprehend in it and to teach all that concerns the theory, use, and mechanism of the telescope (by him called the ocular dioptrique,) divides it into three principal parts.

The first contains the doctrine of optics and dioptrics, or of simple vision

* The father Cherubin was author also of another optical work, named, La Vision Parfaite, in 2 volumes folio, An. 1677 and 1681. He took great pains to recommend the use of the Binocular telescope, or one to look through with both eyes at once; which however has fallen into disuse.

direct, and that which is made by rays refracted.—Discoursing of refraction, he declares, that the refraction of a visual ray in glass, to 30 degrees of inclination, is proportional to the inclination of the ray, as far as sense is able to judge of it. And that the inclination not exceeding 30 degrees, the angle of the refraction of the ray which enters into glass, is about a third part of the angle of the inclination of the ray passing into the air: But that, the same inclination not exceeding 30 degrees, the angle of the refraction of the ray issuing out of the glass into the air, is about the half of the angle of its inclination in the glass.

The second part delivers the theory of the telescope in all its kinds: which is ushered in by a history of the invention and antiquity of telescopes; and by a discourse concerning the difference of the ancient glasses from the modern.—This done, he explains the matter of this second part in 11 sections.

The third part of this volume is subdivided into two, which the author calls the positive, and mechanical. The positive teaches the actual construction of telescopes, and their uses, and that in 12 sections. In these, among other things, he treats of the use of telescopes in the observations of celestial objects: Where the author enumerates the many excellent discoveries that by their means have been made by modern astronomers:—Such as are, 1. The conjunction of Mercury with the sun.—2. Venus having her phases like the moon.—3. The body of the moon appearing like another earth, full of mountains and valleys, seas, rocks, islands, lakes, forests and vast plains; as also the libration of the moon.—4. Spots in the sun.—5. The four satellites of Jupiter.—6. A satellite of Saturn, and rings about the same.—7. Several belts about Jupiter, and divers spots in Mars, Venus, &c.—8. The milky way nothing but an innumerable company of small stars, near to one another.—9. The finding an eclipse to begin and end sooner when observed with the naked eye, than when seen with a telescope; as also, that it appears always less by a digit, being observed by the bare eye, than in reality.—10. Pleiades consisting of many more stars than seven.—11. Orion having 80 other stars besides those three in his belt, and the six in his sword: And the same having in his head 21, instead of the one called the Nebuloso.—12. The observation of many new stars, as in Cassiopæa, in Cete, in Cygno, Andromeda, &c.

The other head is the mechanical, showing the several ways of forming and polishing all sorts of glasses, that serve for telescopes; which is done in six sections.

A Letter from Mr. MARTIN LISTER, *York, January* 10, 1671-2, *containing an ingenious Account of Veins, by him observed in Plants, analogous to Human Veins.* *N°* 79, *p.* 3052.

SIR—I am very much pleased, when you give me to understand, that something is published of the anatomy of vegetables, and that more is designed by that excellent person Signior Malpighi. And since the receipt of your last, I have perused the very ingenious book of Dr. Grew; and, as far as I have observed these matters, all things therein are faithfully delivered, and with great sagacity. In turning over my notes, made some years ago, I find among other things of this nature, some few observations concerning the veins of plants, or such ducts as seem to contain and carry in them the noblest juices of plants. Of these there is little or no mention made in this curious tract, unless under the notion of pores. And because I am of opinion, that they will prove vessels analogous to our human veins, and not mere pores, they shall, if you please, be the subject of your entertainment in this letter; and the rather that, if they prove veins, as I little doubt them, they are not to be passed over in silence, but are early to be accounted for in the anatomy of vegetables.

To avoid ambiguity; those parts of a plant, which Pliny (lib. 16, cap. 38,) calls by the names of venæ and pulpæ, are nothing else, in my opinion, but what our late author, Dr. Grew, calls fibres and insertments, or the ligneous body interwoven with that which he takes to be the cortical, that is, the several distinctions of the grain. Now that the vessels we are about to discourse of are not any of the pores of the ligneous body, to use the doctor's terms, is plain in a transverse cut of Angelica Sylvestris magna vulgatior J. B, for example; the veins there very clearly show themselves, to an attentive view, to be distinct from fibres, observable in the parenchyma of the same cortical body with themselves; the milky juice still rising beside and not in any fibre. Also in the like cut of a burdoc in June, the like juice springs on this and on that side of the radii of the woody circle, that is, in the cortical body and pith only. Again, where there is no pith, there is none of this juice to be observed, and consequently none of these veins, as in the roots of plants, and trunks of trees; but ever in the bark of either. I need not here enumerate the many plants, wherein these particulars are most plainly observable, as in spondylium, cicutaria, many of the thistle kind, &c.

Neither are they probably of the number of the pores, described by our author in the cortical body or pith. Not surely of those pores extended by the breadth, because the course of the juice in these vessels is by the length of the plant; as I have sometimes very plainly traced in the pith of a dried fennel-stalk, following them by dissection quite through the length of the pith. It remains that, if pores, they are of those pores of the cortical body, that are supposed to be extended by the length thereof; which yet seems, to me at least, not enough, but we think them vessels invested with their own proper membranes, analogous to the veins of our human body; for these reasons: 1. Because they are to be found in the pith, and sometimes in the cortical body of a plant, not included within the common tunicle of any fibres, as is above noted: (that fibres or the seminal root are clothed, is most plain in some plants, as in fern and geranium batrachoides, the fibres of the former are coated, at least in some parts of the plant, with a black skin, in the latter likewise with a red one:) And in these cases had they not, I say, their own proper membranes, we see no cause why the very porous and spongy body of the pith and cortex should not be in all places filled alike with the juice, and not rise, as most plainly it does, in a few determinate and set places only, that is, according to the position and order of these vessels.—2. Again the experiment I made, which you were pleased to publish,* concerning the effect of a ligature on cataputia minor Lobel. viz. the sudden springing of the milky juice out of infinite pores besides the incision; the cause of which phenomenon I take to be, the dissected veins impetuously discharging themselves of part of their juice within the porous parenchyma of the bark; whence it is probable, that if there was no coated vessel to hold this milky juice, we might well expect its springing upon the bare ligature, as when we squeeze a wet sponge; the external cuticle of the plant, as this experiment shows, being actually perforated.

In the next place, it is very probable that these vessels are in all plants whatsoever. For as it is probable of all the other substantial parts of plants, that they are actually in, and common to all plants, though specified by divers accidents in figure and texture; so of these veins, which, though they be discernible mostly in those plants where they hold discoloured juices, yet we may very probably think that they are wanting, where the eye finds not that assistance in the challenging of them. As in these very plants where they are least visible, there is yet a time when they are, if not in all, yet in some parts of these plants, plain enough to the naked eye: The tender shoots of the greater and lesser maple, in May, are full of a milky juice, viz. the known liquor of these veins. Again to this purpose, if you apply a clean knife blade to a transverse cut of the like shoots of elder, the gummy liquor of these veins will be drawn forth

* See Number 70.

into visible strings, as is the nature of bird-lime, of the bark of holly, or the milk of cataputia minor Lobel. Further, the leaf stalks of our garden rhubarb do sometimes shoot a transparent and very pure crystalline gum, though the veins, that held this gummy juice, are by no ordinary means visible in them, and yet by comparing the nature and properties of this gum, with that of the gums of other vegetables, we cannot doubt but this gum-rhubarb is the juice of these veins, as well as we are assured the gum of other vegetables to be of theirs, by the same comparative anatomy. Lastly, we think that even mushrooms, that seemingly inferior and imperfect order of vegetables, are not exempt and destitute of these veins, some of them yielding a milky juice, hot and fiery, not unlike some of the spurge kind, or euphorbium.

It might be expected that I should add something at least concerning the original and productions of these veins, if not an exact description of them, the course of the juices in them, and their more immediate and primary uses in the matter of vegetation: But I must acquaint you, that although I find indeed many other scattered particulars, concerning the position, order, number, capacity, distributions, differences, figure, &c. of these veins; you will be pleased to take in good part, if I reserve them till the opportunity of another summer's review.

To conclude with the primary use of these veins; which is, in my opinion, to carry the succus nutritius of plants, because, where they are not, there is no vegetation; as it is seen, if an engrafted branch or arm be bared and stripped of the clay, &c. in June, all the course of vegetation will appear to have been made only by the bark, and not by the wood, that is, in the place only where these veins are. A secondary use is the rich furniture of our shops; for, from these veins only it is that all our vegetable drugs are extracted, and an infinite more might be had by a diligent inquiry, and some easy means, which I have not unsuccessfully put in practice.

An Account of the Speaking Trumpet, as contrived and published by Sir Sam. Moreland,* *Knight and Baronet; with its Uses both at Sea and Land. London, An.* 1671. *N° 79, p.* 3056.

The author of this instrument relates first the several trials made with it; of which the most considerable was, that the largest of those that have been as

* Sir Samuel Moreland was master of mechanics to King Charles the 2d, and invented several useful machines; as, the speaking trumpet, and the engine for extinguishing fires, &c. He published a treatise on arithmetic in 1674. Besides the above article, Sir Samuel had two other papers inserted in the Philosophical Transactions, viz. in volumes 9 and 56, the former on an undertaking

yet employed, turned trumpet-wise, being 5 feet 6 inches long, and of 21 inches diameter at the great end, and 2 inches at the less; when by his Majesty's special command it was tried at Deal Castle by the governor thereof, the voice was plainly heard off at Sea, as far as the king's ships usually ride, which is between two and three miles at a time, when the wind blew from the shore.

Next, he discourses of the nature of sounds and the manner how he conceives them to be extended by this organ. Where he observes, 1. That a small tube does not at all magnify sounds.—2. That it is necessary the diameter of the least end of one of these instruments be equal to, if not greater than, the diameter of the orifice of the speaker's mouth, and that what it wants of that, so much the less does the instrument magnify the voice.—3. That the instrument must be enlarged by degrees, and not too suddenly.—4. That the mouth-piece of it must be so applied and adapted to the speaker's mouth, that no air or breath be lost, and yet the mouth have free liberty of opening and shutting, that so the articulation be entirely preserved, &c.

Lastly, he subjoins the uses of this instrument; as, 1. That in a storm, or in a dark night, when two ships dare not come so near one another as to be heard by any ordinary voice, by this tube they may very easily speak together at half a mile or a mile's distance, or more, if need be, especially if alternately they take the advantage of the wind: And if that be so strong, that but one of the ships can speak with the wind, the other may answer by signs, though directly against it.—2. In a storm it is of good use in a single ship, for hearing one man giving order to all in a ship.—3. By it an admiral may in a calm give immediate orders to his whole fleet; as also a governor may convey his orders from a sea-fort to ships riding at a pretty distance in the road.—4. In case of a close siege, by this instrument, at one, two, or three miles distance, the besieged may be told, by speaking in cypher, that there is relief coming, and how great, and when: And, on the other side, the besiegers may thereby threaten and discourage the besieged, in a spacious town.—5. By this means a general may speak himself to his whole army; a herald may make a proclamation to be distinctly heard by many thousands; an overseer of works give orders to many hundreds of workmen, without changing his station; &c.—The author doubts not but this invention may be much improved.

for raising water, and the latter on a successful operation for the hydrops pectoris. The time of his birth and death are not mentioned.

Our author was the son also of a Sir Samuel Moreland, a great statesman, and under secretary to the minister Thurlow. He was employed by Cromwell in several embassies, and had received the title of baronet for services rendered to Charles the 1st.

An Observation and Experiment concerning a Mineral Balsam, found in a Mine of Italy by Signior Marc Antonio Castagna; *inserted in the 7th Giornale Veneto de Letterati of June* 22, 1671, *and thence Englished as follows. N° 79, p.* 3059.

In the territory of Bergamo, Signior M. Ant. Castagna upon the confines of his jurisdiction, smelling accidentally an uncommon sweet balsamic scent, and following the same so as to find the spirits thereof to strike his nose more and more strongly, he first caused that rocky hill, where he then was, to be digged in the place that appeared to him most likely to be the seat of it, and found, that the stones thereof harboured that fragrancy he smelt, which was so strong, and by trials found so friendly to the uterus, that being applied they did in a very short time cure it of any evil it is subject to. Encouraged hereby to prosecute this work, he made his workmen dig into the very bowels of the hill, and so discovered holes in some stones, as if excavated by art, of a greenish colour, in which he found, as distilled by nature and kept in vessels, that liquor and balsam, which proved the source of that scent, which was limpid and of a white colour, like the white of an egg, but somewhat oleaginous, floating upon all sorts of liquors like oil. Besides, he met in the same cavities some small grains concreted of the said liquor, resembling that which they call white amber; which, being chemically distilled, had the same odour with the balsam.*

Two Observations, from the same Venetian Journal, by P. Francesco Lana, *concerning some of the Effects of the Burning Concave of Lyons; and also an odd Salt extracted out of a Metallic Substance. N° 79, p.* 3060.

The first is, that the said Francesco Lana having been informed, that the famous burning concave, not long since made by M. de Vilette, did much sooner melt iron than gold or silver; he esteems it worth considering, why a kitchen fire does the contrary, melting gold sooner than iron.

The other is, that the said P. Lana, having extracted out of a metallic substance a very white salt, the same was, upon the application of the gentlest heat, resolved into a golden-coloured liquor; which being removed from that warmth, as soon as it felt the cool air, and even by opening the glass wherein it was inclosed, did in a moment shoot afresh into the same salt; and that

* The bituminous fluid here described appears to have been that species of pellucid rock-oil termed naphtha.

whilst he was pouring it out of one glass into another, during its fluidity, it was dispersed all over the glass it was poured into, suddenly congealing into very fine threads, many of which were extended from one side of the glass to the other, and, hanging as it were in the air, formed just like the subtilest cobwebs, not at all rigid, but, by reason of their exquisite subtlety, pliable, and scarcely perceivable by the eye.

The Pleiades observed in 1671, *and predicted for* 1672. *By Mr.* JOHN FLAMSTEED. *Translated from the Latin.* N° 79, *p.* 3061.

Mr. Flamsteed here predicts certain appulses and occultations of the Pleiades by the moon, as they may be expected to happen in the course of the year 1672. Preceding the statement of the expected appulses, he gives a short tablet of the latitudes and longitudes of those stars, with their magnitudes and literal marks, a, b, c, d, e, f, g, h, i. The predicted appearances are now quite useless; but the tablets of places, marks, &c. are as follow:

The stars of the Pleiades, An. 1672.	Mark	Long. ♉	Lat. N.	Mag.
The western brighter star	b	24° 45′ 15″	4° 8′ 51″	5
Between this and the northern telescopic one ..	g	24 46 47	4 19 21	8
The western and more northerly..............	c	24 54 48	4 28 19	6
The highest in the quadrilaterum	e	25 1 24	4 20 39	5
The lowest southern opposite	d	25 2 18	3 53 59	6
The middlemost bright star..................	a	25 19 48	4 0 0	3
That in the eastern angle....................	f	25 41 29	3 52 19	5
The upper of the eastern telescopic	h	25 42 55	3 56 51	7
Another telescopic star	i	25 14 4	3 42 37	9

The observations in 1671 are then given as follow:

I have a tube of $13\frac{2}{3}$ feet, furnished with convex lenses, and a very exact Townley's micrometer, with which, in the clear nights of last October and November, I have often measured the minute distances of the Pleiades, and that with such success, that my repeated observations never differed 20″ from one another, and very seldom 10″. They are also confirmed by the former observations of the late Mr. Gascoigne and those of Mr. Townley, which were made in a similar manner. The correct distances are as follow:

Stars.		Distances.	
a	b	35′	40″
a	e	27	40
e	b	20	0
c	b	21	45
e	c	10	0
e	g	14	40
c	g	11	55
b	d	22	4
a	d	18	30
e	d	26	10
d	i	16	25
a	i	18	18
f	i	29	4
f	a	23	0
h	a	23	20
f	h	4	45

Vinc. Mutus, in a letter to Ricciolus, (printed in the Almages, Nov. tom. 1, p. 717) says, that the brighter western star passed over the meridian just at the same altitude as the bright star of the Pleiades. Relying on which notice, and the observed distances, I have to those stars assigned their places, as in the first table above; first, having given the same place and latitude to the middle bright star, as the Caroline author has done; and the others also being settled from thence. Notwithstanding which, however, if I might follow my own opinion in this matter, I should advance all those longitudes three minutes more, or two at the least, and I would also assign them a greater latitude from the ecliptic.

An Account of some Books. N° 79, p. 3064.

I. Elemens de Geometrie; par le P. Ignace Gaston Pardies, de la Comp. de J. A Paris, 1671, in 12mo.

A neat compendium of Geometry, both theoretical and practical.

II. Regnerus de Graaf de Succo Pancreatico. Lugduni Batavorum, An. 1671, in 12mo.

A new edition of De Graaf's Treatise on the Pancreatic Juice, containing, among other additions, a letter to Dr. Lucas Schacht, professor of physic at Leyden, de partibus genitalibus mulierum, the prodromus of a separate work on that subject. From this letter a Latin extract is inserted in the original Transactions, a translation of which is here deemed unnecessary, as the treatise here promised by the author, and afterwards noticed in N° 82, is in the library of most anatomists.

III. Physico Mathesis de Lumine, Coloribus et Iride, &c. Auth. Franc. Maria Grimaldo,* J. S. Bononiæ, 1665, in 4to.

The author finding that much obscurity was left in the doctrine of light, and esteeming it rather commendable than presumptuous to endeavour the clearing of it, especially if that be done by experiments (which he accounts an excellent way for the improvement of all natural knowledge) undertakes in two parts to deliver his trials and meditations on this subject.

In the first are contained the several experiments which may favour the doctrine of the substantiality of light, together with the ratiocinations thence arising. In the second is represented what may be answered to all those arguments, so as to save the peripatetic opinion of the accidentality of light.

But more particularly, in the former part, he explains how many ways light is propagated or diffused, viz. not only directly, and by refraction and reflection, but also by diffraction, which last, according to him, is done when the parts of light, separated by a manifold dissection, do in the same medium proceed in different ways. Next he considers the nature of light, as also diaphaneity and opacity, and takes notice that most bodies, whether solid or fluid, are porous; on which occasion he ventures to explain almost the whole philosophy of magnetics. Then he discusses the question, whether the diffusion of light be instantaneous, and concludes it in the negative, though the duration of it be imperceptible. This done, he examines the nature of reflection and refraction, and seems to acknowledge, that supposing light to be a substance very fluid and

* Father Grimaldi was one of those philosophers of the 17th century to whom the sciences of astronomy and optics lie under great obligations. Those sciences he cultivated in compact with his compatriot Riccioli, by diligent observations and experiments. He first noticed the spots in the sun, and gave to those in the moon names that are still in use, denominating them after the most eminent astronomers and philosophers. He made numerous experiments in optics, and some discoveries, which were afterwards confirmed and much farther extended by the brilliant discoveries of Newton. Grimaldi discovered the circumstance of the lengthening of the solar image, by a ray of light let in through a small hole, and refracted through a glass prism, but without, however, discovering the different refrangibility of the rays. He taught also that the rays are of different colours, and that opaque objects have no colour but what they receive from the rays of light. He discovered that property of the rays by which, when they pass near the edge of certain objects, though without touching, they are inflected or bent from their direct course, an effect which he termed the diffraction of light, and which Newton afterwards called the inflection of it. Indeed the above work of Grimaldi comprises a great quantity of curious experiments and remarks on light and colours. This indeed was his chief merit; for his philosophy is but too much after the manner of his country, which although it has given to the world a Galileo, a Torricelli, &c. was not the first to shake off the yoke of Aristotle. It must, however, be acknowledged to the honour of Grimaldi, that had he lived in another country, and under another authority than that of his society, he would have boldly opposed the dogmas of the ancient philosophy. Add to this, that he was cut off at a very early period, viz. in 1663, at only 44 years of age.

very subtle, an account may easily be given, why it is reflected and refracted, and why it observes such laws in its reflection and refraction as really it does.

Further, he discourses of colours, and considers how light is changed into colour, sometimes by reflection alone, sometimes by refraction alone, sometimes without either and without the change of the medium, viz. by diffraction. He explains also, how light, by the sole intrinsic modification of itself, passes sometimes into a colour that is commonly called apparent; where he declares, that the reason why light passes into an apparent colour, is not some determinate angle at which the rays amongst themselves are inclined, but that that colour is produced by the intention and density of light. He teaches also, that to the vision of things permanently coloured, there are not required any intentional species transmitted from them, and contradistinct from light; but that the light which is diffused or at least reflected from things coloured, is sufficient; yet with such a modification as is to be found in light apparently coloured, on which occasion many particulars are delivered concerning reflex vision, with an explication of that quære, how the place of the thing seen is perceived, &c.? To all which is added, that the modification of light, by which it is both permanently, and (as they speak) apparently coloured, or made sensible under the representation of colour, may not improbably be said to be a determinate and most finely furrowed undulation of the same, and a kind of tremulous diffusion, with a certain very subtle floating, whereby it does, in a peculiar way of application, affect the organ of vision; which is illustrated and confirmed by what is by philosophers taught of sound and hearing. Upon which it is inferred, that colours are not any thing permanent in visible things, not of themselves lucid, when they are not illuminated; but that they are the light itself, under some peculiar modification made sensible by the sight.

Lastly, This first part is ended with a large discourse of the rainbow, its colours and their order, its circular figure, the concentricness of rainbows, &c. Concluding on the whole, that a rainbow, both the primary and secondary, is generated from the solar rays, reflected and refracted by the drops of a rorid cloud, so that the primary is represented by the rays that are once reflected within those drops; but the secondary, by the rays twice reflected, and which after a double refraction in both cases pass to the eye, placed in the axis of the rainbow.

The second part is dispatched in six propositions; in which the author takes pains, notwithstanding all that he has delivered before, to abet Aristotle's opinion, importing, that light is an accident; though he dissembles not, that that philosopher seems to have somewhere favoured the contrary opinion; as he also acknowledges, that the experiments and the reasons thence deduced for the substantiality of light, approach very near to a physico-mathematical evidence, especially with such men as have, either skilfully and carefully made those experiments themselves, or attentively beheld them when made by others. However, he makes a shift to say something by way of answer to all the arguments, produced in the first place for the proof of light's being a substance: yet denying, that, though light were an accident, it would follow, that colours, called permanent, are something distinct from light, and residing in bodies when light is absent.

IV. Marci Meibomii de Fabrica Triremium Liber. Amstelodami, 1671, in 4to.

This discourse treats first of the occasion and original of shipping, and relates, that it began with oars, and then was improved by sails, and at last was practised with the use of both. In the beginning, for celerity and fight, they multiplied oars, and for some strength, they fortified their ships with strong beaks, as birds of prey have strong wings and a sharp beak. He ascribes to the Sidonians the first invention of building long ships for war, and the contrivance of filling them with oars in such a manner, that no void spaces might be left. As broader and shorter ships were built for burthen.

Galleys he distinguishes into monocrota, wherein one or more rows of men sit in the same level or plain; and polycrota, in which the rowers sit in divers heights, one above another, as in amphitheatres; whence the biremis, triremis, quadriremis, and so on to the tessaraconteris, the largest that we read of, and recorded to have been made by Philopater.

In the monocrota he considers the manner of the sitting of the rowers; and the interscalmium, or the space between the two oars of the same versus or row; referring the transtra to the polycrota galleys; where he has occasion to examine the measure of the great and Roman foot and cubit; as also to give the meaning of the words versus (Gr. ϛοιχος or ϛιχος) and ordo (Gr. τάξις.)

Next he endeavours to explain, in the galleys that are polycrota of 3, 4, 5, or more tiers of rowers, seated in different heights, how those men could be placed. And here he pretends to have been the first that has perfected the way of lessening the height of the ancient galleys, by devising these two expedients; by the first of which (said to have been published by him 22 years since) he affirms to have showed, so to place the lateral rowers, as that he that sits behind another, may move his hands and oar under the seat of the rower sitting next before him: by which means three lateral rowers, which, according to Scaliger's way, would require the height of 13½ feet, will occupy only the space of 7½ feet. By the other invention, which he now adds, he pretends to have found a new place in those ships for almost half the number of rowers.

After this he inquires, whether ever such great vessels of so many tiers of oars, sitting in so many different heights, were ever actually built? And, if they were, whether they ever came abroad to fight? Especially such a one as that of Philopater's is recorded to have been, of 40 tiers, requiring above 4000 rowers; and that of Ptolemæus Philadelphus, of 30 tiers, having more than 3000 rowers; and another of 20 rows, requiring 2000. Hereupon our author scruples not to affirm all to be true that is written of such vast ships; adding, that he has made it intelligible how it may be so, by finding places for the zygitæ, and a conveniency of moving their oars under the seats of those that sat next before them. And here he shows at large, of what determinate size those vessels were, according to his supposition and contrivance; beginning from a triremis, and showing how many oars and seamen it contained, namely 200, of which 180 were rowers, and the rest mariners. So that in the Athenian fleet, of which Cono was general, consisting of 180 triremes, there were 36000 men. Then proceeding to a quinqueremis, with 420 men each, of which there were rowers 300, and soldiers 120. So that three things were stupendous in that Roman fleet at Messina, and the Carthaginian at Lyliboum; one is, that the former consisted of 330, and the latter of 350 ships, most quinqueremes, that is, 150 feet long; the second, that the number of men, they contained was 130000, and 150000 men; the third, the apparatus and provision necessary: yet all this affirmed by one of the best of the ancient historians, Polybius; who himself wonders at such a vast equipage.

Here the author undertakes, out of Polybius, Plutarch and Livy, to refute Salmasius, affirming, that hardly any galleys were built or equipped larger than of nine tiers, called ἐννήρης. Hence he proceeds to the ships of eleven rows (ἑνδεκήρης,) and of fifteen rows (πεντεκαιδεκήρης;) and to one of sixteen (ἑκκαιδεκήρης.)

A Letter of Mr. Isaac Newton,* *Professor of Mathematics in the University of Cambridge; containing his New Theory of Light and Colours: sent by the Author to the Editor from Cambridge, Feb.* 6, *1671-2; to be communicated to the Royal Society.* *N° 80, p.* 3075.

Sir,—To perform my late promise to you, I shall without further ceremony

* This letter appears as the first public communication of the illustrious author, concerning one part of his brilliant discoveries, viz. the different colour and refrangibility of the rays of light; a discovery truly novel and philosophical, and is the more extraordinary as made when the author was only 23 years of age; as appears by comparing the date of the experiments with that of his birth, Dec. 25, 1642. We shall here only notice farther that he died March 20, 1727, in the 85th year of his age; reserving the more ample account of his life and writings to the intended miscellaneous volume before mentioned.

acquaint you, that in the beginning of the year 1666 (at which time I applied myself to the grinding of optic glasses of other figures than spherical,) I procured a triangular glass prism, to try therewith the celebrated phænomena of colours. And for that purpose having darkened my chamber, and made a small hole in my window shuts, to let in a convenient quantity of the sun's light, I placed my prism at his entrance, that it might be thereby refracted to the opposite wall. It was at first a very pleasing diversion to view the vivid and intense colours produced thereby; but after a while applying myself to consider them more circumspectly, I was surprised to see them in an oblong form; which according to the received laws of refraction, I expected would have been circular. They were terminated at the sides with straight lines, but at the ends, the decay of light was so gradual, that it was difficult to determine justly what was their figure; yet they seemed semicircular.

Comparing the length of this coloured spectrum with its breadth, I found it about five times greater; a disproportion so extravagant, that it excited me to a more than ordinary curiosity of examining from whence it might proceed. I could scarce think, that the various thickness of the glass, or the termination with shadow or darkness, could have any influence on light to produce such an effect; yet I thought it not amiss, first to examine those circumstances, and so tried what would happen by transmitting light through parts of the glass of divers thicknesses, or through holes in the window of divers sizes, or by setting the prism without, so that the light might pass through it, and be refracted before it was terminated by the hole: but I found none of those circumstances material. The fashion of the colours was in all these cases the same.

Then I suspected, whether by any unevenness in the glass, or other contingent irregularity, these colours might be thus dilated. And to try this, I took another prism like the former, and so placed it, that the light passing through them both, might be refracted contrary ways, and so by the latter returned into that course from which the former had diverted it. For, by this means, I thought the regular effects of the first prism would be destroyed by the second, but the irregular ones more augmented, by the multiplicity of refractions. The event was, that the light, which by the first prism was diffused into an oblong form, was by the second reduced into an orbicular one, with as much regularity as when it did not at all pass through them. So that, whatever was the cause of that length, it was not any contingent irregularity.

I then proceeded to examine more critically, what might be effected by the difference of the incidence of rays coming from divers parts of the sun; and to that end measured the several lines and angles, belonging to the image. Its distance from the hole or prism was 22 feet; its utmost length $13\frac{1}{4}$ inches; its breadth $2\frac{5}{8}$; the diameter of the hole $\frac{1}{4}$ of an inch; the angle, which the rays, tending towards the middle of the image, made with those lines in which they would have proceeded without refraction, was 44° 56′; and the vertical angle of the prism, 63° 12′. Also the refractions on both sides the prism, that is, of the incident and emergent rays, were as near as I could make them equal, and consequently about 54° 4′. And the rays fell perpendicularly upon the wall. Now subducting the diameter of the hole from the length and breadth of the image, there remains 13 inches the length, and $2\frac{3}{8}$ the breadth, comprehended by those rays, which passed through the centre of the said hole, and consequently the angle of the hole, which that breadth subtended, was about 31′, answerable to the sun's diameter; but the angle which its length subtended, was more then five such diameters, namely 2° 49′.

Having made these observations, I first computed from them the refractive power of that glass, and found it measured by the ratio of the sines, 20 to 31. And then, by that ratio, I computed the refractions of two rays flowing from opposite parts of the sun's discus, so as to differ 31′ in their obliquity of incidence, and found that the emergent rays should have comprehended an angle of about 31′, as they did, before they were incident. But because this computation was founded on the hypothesis of the proportionality of the sines of incidence and refraction, which though, by my own experience, I could not imagine to be so erroneous as to make that angle but 31′, which in reality was 2° 49′; yet my curiosity caused me again to take my prism. And having placed it at my window, as before, I observed, that by turning it a little about its axis to and fro, so as to vary its obliquity to the light, more than an angle of 4 or 5 degrees, the colours were not thereby sensibly translated from their place on the wall, and consequently by that variation of incidence, the quantity of refraction was not sensibly varied. By this experiment therefore, as well as by the former computation, it was evident, that the difference of the incidence of rays, flowing from divers parts of the sun, could not make them, after a decussation, diverge at a sensibly greater angle, than that at which they before converged; which being at most but about 31 or 32 minutes, there still remained some other cause to be found out, from whence it could be 2° 49′.

Then I began to suspect whether the rays, after their trajection through the prism, did not move in curve lines, and according to their more or less curvity tend to divers parts of the wall. And it increased my suspicion, when I remembered that I had often seen a tennis ball, struck with an oblique racket, describe such a curve line. For, a circular as well as a progressive motion being communicated to it by that stroke, its parts on that side, where the motions conspire, must press and beat the contiguous air more violently than on the other, and there excite a reluctancy and reaction of the air proportionably greater. And for the same reason, if the rays of light should possibly be globular bodies, and by their oblique passage out of one medium into another acquire a circulating motion, they ought to feel the greater resistance from the ambient æther, on that side where the motions conspire, and thence be continually bowed to the other. But notwithstanding this plausible ground of suspicion, when I came to examine it, I could observe no such curvity in them. And besides (which was enough for my purpose) I observed, that the difference between the length of the image and diameter of the hole, through which the light was transmitted, was proportionable to their distance.

The gradual removal of these suspicions, at length led me to the experimentum crucis, which was this: I took two boards, and placed one of them close behind the prism at the window, so that the light might pass through a small hole, made in it for the purpose, and fall on the other board, which I placed at about 12 feet distance, having first made a small hole in it also, for some of that incident light to pass through. Then I placed another prism behind this second board, so that the light trajected through both the boards, might pass through that also, and be again refracted before it arrived at the wall. This done, I took the first prism in my hand, and turned it to and fro slowly about its axis, so much as to make the several parts of the image, cast on the second board, successively pass through the hole in it, that I might observe to what places on the wall the second prism would refract them. And I saw, by the variation of those places, that the light tending to that end of the image, towards which the refraction of the first prism was made, did in the second prism suffer a refraction considerably greater then the light tending to the other end. And so the true cause of the length of that image was detected to be no other, than that light consists of rays differently refrangible, which, without any respect to a difference in their incidence, were according to their degrees of refrangibility, transmitted towards divers parts of the wall.

When I understood this, I left off my aforesaid glass works; for I saw, that the perfection of telescopes was hitherto limited, not so much for want of glasses truly figured according to the prescriptions of optic authors, (which all men have hitherto imagined,) as because that light itself is a heterogeneous mixture of differently refrangible rays. So that, were a glass so exactly figured, as to collect any one sort of rays into one point, it could not collect those also into the same point, which having the same incidence upon the same medium are apt to suffer a different refraction. Nay, I wondered, that seeing the difference of refrangibility was so great, as I found it, telescopes should arrive to that perfection they are now at. For measuring the refractions in one of my prisms, I found, that supposing the common sine of incidence upon one of its planes was 44 parts, the sine of refraction of the utmost rays on the red end of the colours, made out of the glass into the air, would be 68 parts, and the sine of refraction of the utmost rays on the other end 69 parts: so that the difference is about a 24th or 25th part of the whole refraction; and consequently, the object glass of any telescope cannot collect all the rays which come from one point of an object, so as to make them convene at its focus in less room than in a circular space, whose diameter is the 50th part of the diameter of its aperture; which is an irregularity, some hundreds of times greater than a circularly figured lens, of so small a section as the object glasses of long telescopes are, would cause by the unfitness of its figure, were light uniform.

This made me take reflections into consideration, and finding them regular, so that the angle of reflection of all sorts of rays was equal to their angle of incidence; I understood that by their mediation optic instruments might be brought to any degree of perfection imaginable, provided a reflecting substance could be found, which would polish as finely as glass, and reflect as much light as glass transmits, and the art of communicating to it a parabolic figure be also attained. But there seemed very great difficulties, and I have almost thought them insuperable, when I further considered, that every irregularity in a reflecting superficies makes the rays stray 5 or 6 times more out of their due course, than the like irregularities in a refracting one: so that a much greater curiosity would be here requisite, than in figuring glasses for refraction.

Amidst these thoughts I was forced from Cambridge by the intervening plague, and it was more then two years before I proceeded further. But then having thought on a tender way of polishing, proper for metal, whereby as I imagined, the figure also would be corrected to the last; I began to try what might be effected in this kind, and by degrees so far perfected an instrument (in the essential parts of it like that I sent to London,) by which I could discern Jupiter's 4 concomitants, and showed them divers times to two others of my acquaintance. I could also discern the moon-like phase of Venus, but not very distinctly, nor without some niceness in disposing the instrument.

From that time I was interrupted till this last autumn, when I made the other. And as that was sensibly better then the first (especially for day objects,) so I doubt not, but they will be still brought to a much greater perfection by their endeavours, who, as you inform me, are taking care about it at London.

I have sometimes thought to make a microscope, which in like manner should have, instead of an object glass, a reflecting piece of metal. And this I hope they will also take into consideration. For those instruments seem as capable of improvement as telescopes, and perhaps more, because but one reflective piece of metal is requisite in them, as you may perceive by the diagram, (fig. 13, pl. 14,) where AB represents the object metal, CD the eye glass, F their common focus, and O the other focus of the metal, in which the object is placed.

But to return from this digression, I told you, that light is not similar, or homogeneal, but consists of difform rays, some of which are more refrangible than others: so that of those, which are alike incident on the same medium, some shall be more refracted than others, and that not by any virtue of the glass, or other external cause, but from a predisposition, which every particular ray has to suffer a particular degree of refraction.

I shall now proceed to acquaint you with another more notable difformity in its rays, wherein the origin of colours is unfolded: concerning which I shall lay down the doctrine first, and then, for its examination, give you an instance or two of the experiments, as a specimen of the rest.—The doctrine you will find comprehended and illustrated in the following propositions:—

1. As the rays of light differ in degrees of refrangibility, so they also differ in their disposition to exhibit this or that particular colour. Colours are not qualifications of light, derived from refractions, or reflections of natural bodies (as it is generally believed,) but original and connate properties, which in divers rays are diverse. Some rays are disposed to exhibit a red colour, and no other; some a yellow, and no other; some a green, and no other, and so of the rest. Nor are there only rays proper and particular to the more eminent colours, but even to all their intermediate gradations.

2. To the same degree of refrangibility ever belongs the same colour, and to the same colour ever belongs the same degree of refrangibility. The least refrangible rays are all disposed to exhibit a red colour, and contrarily, those rays which are disposed to exhibit a red colour, are all the least refrangible: so the most refrangible rays are all disposed to exhibit a deep violet-colour, and contrarily, those which are apt to exhibit such a violet colour, are all the most refrangible. And so to all the intermediate colours, in a continued series, belong intermediate degrees of refrangibility. And this analogy betwixt colours, and refrangibility, is very precise and strict; the rays always either exactly agreeing in both, or proportionally disagreeing in both.

3. The species of colour, and degree of refrangibility proper to any particular sort of rays, is not mutable by refraction, nor by reflection from natural bodies,

nor by any other cause, that I could yet observe. When any one sort of rays has been well parted from those of other kinds, it has afterwards obstinately retained its colour, notwithstanding my utmost endeavours to change it. I have refracted it with prisms, and reflected it with bodies, which in day-light were of other colours; I have intercepted it with the coloured film of air interceding two compressed plates of glass; transmitted it through coloured mediums, and through mediums irradiated with other sorts of rays, and diversely terminated it; and yet could never produce any new colour out of it. It would, by contracting or dilating, become more brisk, or faint, and by the loss of many rays, in some cases very obscure and dark; but I could never see it change in specie.

4. Yet seeming transmutations of colours may be made, where there is any mixture of divers sorts of rays. For in such mixtures, the component colours appear not, but, by their mutual allaying each other, constitute a middling colour. And therefore, if by refraction, or any other of the aforesaid causes, the difform rays, latent in such a mixture, be separated, there shall emerge colours different from the colour of the composition. Which colours are not new generated, but only made apparent by being parted; for if they be again entirely mixed and blended together, they will again compose that colour, which they did before separation. And for the same reason, transmutations made by the convening of divers colours are not real; for when the difform rays are again severed, they will exhibit the very same colours, which they did before they entered the composition; as you see, blue and yellow powders, when finely mixed, appear to the naked eye green, and yet the colours of the component corpuscles are not thereby really transmuted, but only blended. For, when viewed with a good microscope, they still appear blue and yellow interspersedly.

5. There are therefore two sorts of colours. The one original and simple, the other compounded of these. The original or primary colours are, red, yellow, green, blue, and a violet-purple, together with orange, indigo, and an indefinite variety of intermediate gradations.

6. The same colours in specie with these primary ones may be also produced by composition: for a mixture of yellow and blue makes green; of red and yellow makes orange; of orange and yellowish green makes yellow. And in general, if any two colours be mixed, which in the series of those, generated by the prism, are not too far distant one from another, they by their mutual alloy compound that colour, which in the said series appears in the midway between them. But those which are situated at too great a distance, do not so. Orange and indigo produce not the intermediate green, nor scarlet and green the intermediate yellow.

7. But the most surprising and wonderful composition was that of whiteness. There is no one sort of rays which alone can exhibit this. It is ever

compounded, and to its composition are requisite all the aforesaid primary colours, mixed in a due proportion. I have often with admiration beheld, that all the colours of the prism being made to converge, and thereby to be again mixed as they were in the light before it was incident upon the prism, reproduced light, intirely and perfectly white, and not at all sensibly differing from a direct light of the sun, unless when the glasses, I used, were not sufficiently clear; for then they would a little incline it to their colour.

8. Hence therefore it comes to pass, that whiteness is the usual colour of light; for, light is a confused aggregate of rays indued with all sorts of colours, as they are promiscuously darted from the various parts of luminous bodies. And of such a confused aggregate, as I said, is generated whiteness, if there be a due proportion of the ingredients; but if any one predominate, the light must incline to that colour; as it happens in the blue flame of brimstone; the yellow flame of a candle; and the various colours of the fixed stars.

9. These things considered, the manner how colours are produced by the prism, is evident. For, of the rays constituting the incident light, since those which differ in colour, proportionally differ in refrangibility, they by their unequal refractions must be severed and dispersed into an oblong form in an orderly succession, from the least refracted scarlet, to the most refracted violet. And for the same reason it is that objects, when looked upon through a prism, appear coloured. For the difform rays, by their unequal refractions, are made to diverge towards several parts of the retina, and there express the images of things coloured, as in the former case they did the sun's image upon a wall. And by this inequality of refractions they become not only coloured, but also very confused and indistinct.

10. Why the colours of the rainbow appear in falling drops of rain, is also from hence evident. For, those drops which refract the rays disposed to appear purple, in greatest quantity to the spectator's eye, refract the rays of other sorts so much less, as to make them pass beside it; and such are the drops on the inside of the primary bow, and on the outside of the secondary or exterior one. So those drops, which refract in greatest plenty the rays apt to appear red, towards the spectator's eye, refract those of other sorts so much more, as to make them pass beside it; and such are the drops on the exterior part of the primary, and interior part of the secondary bow.

11. The odd phænomena of an infusion of lignum nephriticum, leaf gold, fragments of coloured glass, and some other transparently coloured bodies, appearing in one position of one colour, and of another in another, are on these grounds no longer riddles. For, those are substances apt to reflect one sort of light, and transmit another; as may be seen in a dark room, by illuminating

them with similar or uncompounded light. For, then they appear of that colour only, with which they are illuminated, but yet in one position more vivid and luminous than in another, accordingly as they are disposed more or less to reflect or transmit the incident colour.

12. From hence also is manifest the reason of an unexpected experiment, which Mr. Hook, somewhere in his micrography, relates to have made with two wedge-like transparent vessels, filled the one with red, the other with a blue liquor: namely, that though they were severally transparent enough, yet both together became opaque; for, if one transmitted only red, and the other only blue, no rays could pass through both.

13. I might add more instances of this nature; but I shall conclude with this general one, that the colours of all natural bodies have no other orgin than this, that they are variously qualified to reflect one sort of light in greater plenty than another. And this I have experimented in a dark room, by illuminating those bodies with uncompounded light of divers colours. For, by that means, any body may be made to appear of any colour. They have there no appropriate colour, but ever appear of the colour of the light cast upon them, but yet with this difference, that they are most brisk and vivid in the light of their own day-light colour. Minium appears there of any colour indifferently, with which it is illustrated, but yet most luminous in red; and so bise appears indifferently of any colour with which it is illustrated, but yet most luminous in blue. And therefore minium reflects rays of any colour, but most copiously those indued with red; and consequently when illustrated with daylight, that is, with all sorts of rays promiscuously blended, those qualified with red shall abound most in the reflected light, and by their prevalence cause it to appear of that colour. And for the same reason bise, reflecting blue most copiously, shall appear blue by the excess of those rays in its reflected light; and the like of other bodies. And that this is the entire and adequate cause of their colours, is manifest, because they have no power to change or alter the colours of any sort of rays, incident apart, but put on all colours indifferently, with which they are enlightened.

These things being so, it can be no longer disputed, whether there be colours in the dark, nor whether they be the qualities of the objects we see, no nor perhaps whether light be a body. For since colours are the qualities of light, having its rays for their entire and immediate subject, how can we think those rays qualities also, unless one quality may be the subject of and sustain another; which in effect is to call it substance. We should not know bodies for substances, were it not for their sensible qualities, and the principal of those being now found due to something else, we have as good reason to believe that to be a substance also.

Besides, whoever thought any quality to be a heterogeneous aggregate, such as light is discovered to be. But, to determine more absolutely what light is, after what manner refracted, and by what modes or actions it produces in our minds the phantasms of colours, is not so easy. And I shall not mingle conjectures with certainties.

Reviewing what I have written, I see the discourse itself will lead to divers experiments sufficient for its examination, and therefore I shall not trouble you further, than to describe one of those which I have already insinuated.

In a darkened room make a hole in the shut of a window, whose diameter may conveniently be about a third part of an inch, to admit a convenient quantity of the sun's light; and there place a clear and colourless prism, to refract the entering light towards the further part of the room, which, as I said, will thereby be diffused into an oblong coloured image. Then place a lens of about three feet radius (suppose a broad object glass of a three-foot telescope,) at the distance of about four or five feet from thence, through which all those colours may at once be transmitted, and made by its refraction to convene at a further distance of about ten or twelve feet. If at that distance you intercept this light with a sheet of white paper, you will see the colours converted into whiteness again by being mingled. But it is requisite, that the prism and lens be placed steady, and that the paper on which the colours are cast be moved to and fro; for by such motion, you will not only find at what distance the whiteness is most perfect, but also see how the colours gradually convene, and vanish into whiteness, and afterwards having crossed one another in that place where they compound whiteness, are again dissipated and severed, and in an inverted order retain the same colours which they had before they entered the composition. You may also see, that if any of the colours at the lens be intercepted, the whiteness will be changed into the other colours. And therefore that the composition of whiteness be perfect, care must be taken that none of the colours fall beside the lens.

In the annexed design of this experiment, ABC expresses the prism set endwise to sight, fig. 14, pl. 14, close by the hole F of the window EG. Its vertical angle ACB may conveniently be about 60 degrees: MN designs the lens. Its breadth $2\frac{1}{2}$ or 3 inches. SF one of the straight lines, in which difform rays may be conceived to flow successively from the sun. FP and FR two of those rays unequally refracted, which the lens makes to converge towards Q, and after decussation to diverge again. And HI the paper, at divers distances, on which the colours are projected; which in Q constitute whiteness, but are red and yellow in R, r, and ϱ, and blue and purple in P, p, and π.

If you proceed further to try the impossibility of changing any uncompounded colour, (which I have asserted in the 3d and 13th propositions) it is requisite that the room be made very dark, least any scattering light mixing with the colour disturb and allay it, and render it compound, contrary to the design of the experiment. It is also requisite, that there be a perfecter separation of the colours than, after the manner above described, can be made by the refraction of one single prism, and how to make such further separations, will scarcely be difficult to them that consider the discovered laws of refractions. But if trial shall be made with colours not thoroughly separated, there must be allowed changes proportionable to the mixture. Thus, if compound yellow light fall upon blue bise, the bise will not appear perfectly yellow but rather green, because there are in the yellow mixture many rays indued with green, and green being less remote from the usual blue colour of bise than yellow, is the more copiously reflected by it.

In like manner, if any one of the prismatic colours, suppose red, be intercepted, on design to try the asserted impossibility of reproducing that colour out of the others which are pretermitted; it is necessary, either that the colours be very well parted before the red be intercepted, or that together with the red the neighbouring colours, into which any red is secretly dispersed, (that is, the yellow, and perhaps green too) be intercepted, or else, that allowance be made for the emerging of so much red out of the yellow green, as may possibly have been diffused, and scatteringly blended in those colours. And if these things be observed, the new production of red, or any intercepted colour will be found impossible.

This I conceive is enough for an introduction to experiments of this kind; which if any of the Royal Society shall be so curious as to prosecute, I should be very glad to be informed with what success; that, if any thing seem to be defective, or to thwart this relation, I may have an opportunity of giving further direction about it, or of acknowledging my errors, if I have committed any.

An Account of some Books. N° 80, p. 3088.

I. Beschriving der Oost Indische Kusten, Malabar, Coromandel, Ceylon, &c. Door Philippus Baldæus. T. Amsterdam. 1672, in fol.

The author of this recent history, an active Dutch minister, having lived many years in the East Indies, especially in Ceylon, has taken great pains to give an account not only of the late civil transactions of his countrymen and others in those parts, but also of many of the observables in natural and moral things in these countries. His natural observations are chiefly these following:—

That in the haven of Surat, a north north-east and a south west moon makes the highest water. That the elephants made to fight with one another before the Great Mogul, manage the combat with a far greater agility and courage than one would imagine; obediently falling to and desisting according to the word given, and embracing one another most lovingly with their trunks, as soon as they are commanded to end the combat. That on the coast of Malabar the nights are very cold, and that there falls a great dew, especially in the months of January, February, and March, which is followed by intolerable heats in the day; as also, that the land breezes begin in the morning about nine and ten o'clock, and the sea breezes soon after sun-setting. The productions of that country are chiefly pepper, aloe, cardimom, ginger, some salt petre, and gum lack, also bezoar-stones, which are best at Cananor; likewise mirabolans, tamarinds.—That pepper grows best in shady places, has a weak stem, to be supported like vines; having on each branch commonly six clusters, each a foot long, in colour like unripe grapes; that they gather it, being green, in October and November, exposing it to the sun to dry, whereby it grows black in a few days. That at Cananor there are sometimes found bezoar-stones, of the size not only of a pigeon's egg, which may be had for 6 or 7 reals, but also of that of a hen's egg, for 12 reals a-piece. That most of the inhabitants of Porca in the country of Malabar have swollen legs, ascribed to the brackish and salt petre water, they must drink there. That about Tutecoryn, in Malabar, is one of the three chief pearl fishings in the East Indies, comprizing Manaar and Aripou, situate between Comorin and Ceylon; the other two places for taking that rich commodity being Ormus in Persia, and Ainam on the coast of China. That the pearl-bearing oysters are hard and tough, and not good to eat; that they must be dived for 7, 8, 9, 10, fathoms deep; that all years do not equally yield pearls, in regard that sometimes the pearl-banks are covered with sand, and sometimes the oysters are too small; that the pearls of Tutecoryn and Manaar are inferior in goodness to those of Ormus, those being neither so white nor bright as these. That the best stained cloths are made about Maliapour, to which a peculiar kind of water springing there is thought to be very conducive. That between Penna and Caleture, north of Palecatta, on the coast of Coromandel, there grows the best essaye; which is a small root used in staining Indian cloths with fixed colours. That at Petapouli, near Masulipatan, on the same coast, there grows another excellent essaye, called tambrevelle, yielding so high and intense a colour, that it must be mixed with the essaye of arrical or ortacour, to qualify its intenseness. This is only in the power of the governor of that place. That the great traffic in diamonds and rubies is at Masulipatan; that diamonds are digged in the countries of Golconda and Decan, behind Bengala, near the town Basilaga; the prince whereof keeps a continual guard there, reserving for himself all such stones as weigh above 25 mangelys or carats. The old rock is in the country of Deyam, yielding the best diamonds of all. Borneo likewise is famous for these stones, and especially the town Bangar Massing. That the plant which yields the indigo, bears a flower like that of thistles, and a seed like that of fænum Græcum: that being first sown, it holds out three years. That thea is by the people of China esteemed wholesomest when taken fasting, and without sugar; that the Dutch use it much in India for health and cheerfulness; that it is very diuretical, and opening the kidneys, and causing free respiration; that the best grows in the province of Kiangnan in China, about the town Hocicheu; that when good the leaves yield a very pleasing scent; that the thea of China far excels that of Japan. That in Patiarapalli, one of the provinces of Jafnapatnam, the elephants by the strength of their body bear down every year abundance of wild palm-trees, when their fruit is ripe. That in Paletiva, one of the small isles near Jafnapatnam, the people catch the wild horses there by chasing them into a water pool, and so mastering them with nooses. That the wild elephants are by the tame females of the same kind as it were duckoyed into a lodge with trap doors, where by hunger and long wakes, and the discipline exercised upon them by tame elephants, they are at length tamed themselves. That Ceylon abounds, besides elephants and wild horses, with buffaloes, oxen, cows, sheep, hogs, goats, deer, elks, wild boars, tygers, bears, jackals, apes, peacocks, nightingales, larks, snipes, partridges, pigeons, geese, crows, kites, owls, &c. The jackals are so greedy after man's flesh that the inhabitants are fain to keep their dead from them by covering their sepulchres with large stones. To which he adds, that their flesh is very medicinal for a consumption. That Ceylon affords divers sorts of precious stones, as rubies, sapphires, topazes, granates: and mines also of gold, silver, and iron, but that the kings of the island will not suffer the royal metals to be dug up. That the commodities for trade in Ceylon are, stained stuffs, silks, porcelain, spices, camphire, ambergris, radix chinæ, amphion, muscus, santal, salt-petre, sulphur, lead, copper, tin, &c.

II. Antonii le Grand Institutio Philosophiæ, secundum principia Renati Descartes; nova methodo adornata et explicata.

This author has with much industry and clearness laid together, in this small pocket volume, all the parts of the Cartesian philosophy, to facilitate the study of the same to such as desire to instruct themselves in it.

III. An Essay to the Advancement of Music. By Tho. Salmon, M.A. London 1672, in 8vo.

The design of this essay is, to advance music by casting away the perplexity of different cliffs, and uniting all sorts of music, lute, viol, violin, organ, harpsichord, voice, &c. in one universal character.

END OF VOLUME SIXTH OF THE ORIGINAL.

An Account of a New Catadioptrical Telescope, invented by Mr. Newton, F. R. S. and Professor of Mathematics in the University of Cambridge. * *N° 81, p. 4004. Vol. VII.*

This excellent mathematician having given us, in the Transactions of Feb. last, an account of the cause which induced him to think on reflecting telescopes instead of refracting ones, has thereupon presented the world with an essay of what may be performed by such telescopes; by which it is found, that telescopical tubes may be considerably shortened without prejudice to their magnifying effect.

This new instrument is composed of two metalline specula, the one concave, (instead of an object glass) the other plain; and also of a small plano-convex eye-glass. By fig. 1. of pl. 15, the structure of it may be easily imagined, viz. that the tube of this telescope is open at the end which respects the object; that the other end is close, where the said concave is laid, and that near the open end there is a flat oval speculum, made as small as may be, the less to obstruct the entrance of the rays of light, and inclined towards the upper part of the tube, where is a little hole furnished with the said eye-glass. So that the rays coming from the object, first fall on the concave placed at the bottom of the tube, and are thence reflected toward the other end of it, where they meet

* In this paper we have the description of the first reflecting telescope that was ever made, as far as we know. The idea had indeed been mentioned a few years before, viz. by Mersenne, in a letter to Descartes, who did not approve of it; and again by James Gregory, in his Optica Promota, who endeavoured in vain to carry the idea into execution. Those attempts however were suggested by a motive far inferior to that of Newton, being intended, besides shortening the telescope, to avoid the errors arising from the figure of the lenses; whereas that of our author was to obviate the error and inconvenience of the coloured images, and of the unequal refraction of the rays of light; a splendid discovery, which had but just before been made by himself.

with the flat speculum, obliquely posited, by the reflection of which they are directed to the little plano-convex glass, and so to the spectator's eye, who looking downwards sees the object which the telescope is turned to.

To understand this more distinctly and fully, the reader will observe that AB is the concave speculum, of which the radius or semi-diameter is $12\frac{2}{3}$ or 13 inches. CD another metalline speculum, whose surface is flat and the circumference oval. GD an iron wire, holding a ring of brass, in which the speculum CD is fixed.

F, a small eye glass, flat above, and convex below, of the 12th part of an inch radius, if not less; as the metal collects the sun's rays at $6\frac{1}{3}$ inches distance, and the eye-glass at less than $\frac{1}{6}$ of an inch distance from its vertex: besides that the author knew their dimensions by the tools to which they were ground, and particularly measuring the diameter of the hemispherical concave, in which the eye glass was wrought, found it the 6th part of an inch.

GGG, the fore part of the tube, fastened to a brass ring HI, to keep it immoveable. PQKL, the hind part of the tube, fastened to another brass ring PQ. O, an iron hook, fastened to the ring PQ, and furnished with a screw N, thereby to advance or draw back the hind part of the tube, and so by that means to put the specula at their due distance. MQGI, a crooked iron, sustaining the tube, and fastened by the nail R to the ball and socket S, whereby the tube may be turned every way. The centre of the flat speculum CD, must be placed in the same point of the tube's axis, where falls the perpendicular to this axis, drawn to the same from the centre of the little eye-glass, which point is here marked at T.

And that the reader may understand in what degree it represents things distinct, and free from colours, and know the aperture by which it admits light, he may compare the distances of the focus E from the vertices of the little eye-glass and concave speculum, that is, EF, $\frac{1}{6}$ of an inch, and ETV, $6\frac{1}{3}$ inches, and the ratio will be found as 1 to 38; whereby it appears that the objects will be magnified about 38 times. To which proportion is very consentaneous an observation of the crown on the weather-cock, about 300 feet distant. For the scheme X, figure 2, represents it larger by $2\frac{1}{2}$ times in diameter, when seen through this, than through an ordinary telescope (as at figure 3) of about two feet long. And so supposing this ordinary one to magnify 13 or 14 times, as by the description it should, this new one by the experiment must magnify near as much as has been assigned.

Thus far as to the structure of this telescope. Concerning the metalline matter fit for these reflecting speculums, the inventor has also considered the same, as may be seen by two of his letters written to the editor from Cambridge, Jan. 18 and 29, 1671-72 to this effect, viz.

1. That for a fit metalline substance he would give this caution; that while men seek for a white, hard and durable metalline composition, they resolve not upon such a one as is full of small pores, only discoverable by a microscope. For though such a one may, to appearance, take a good polish, yet the edges of those small pores will wear away faster in the polishing than the other parts of the metal; and so however polished the metal may appear, yet it shall not reflect with such an accurate regularity as it ought to do. Thus tin glass mixed with ordinary bell metal makes it more white, and apt to reflect a greater quantity of light; but its fumes, raised in the fusion like so many aërial bubbles, fill the metal full of those microscopical pores. But white arsenic both blanches the metal, and leaves it solid without any such pores, especially if the fusion has not been too violent. What the Stellate Regulus of Mars (which I have sometimes used) or other such like substance will do, deserves particular examination.

He adds this: that putty, or other such like powder with which it is polished, by the sharp angles of its particles frets the metal if it be not very fine, and fills it full of such small holes as he speaks of. Wherefore care must be taken of that, before judgment be given, whether the metal be throughout the body of it porous or not.

2. Not having tried many proportions of the arsenic and metal, he does not affirm, which is absolutely best, but thinks there may conveniently be used any quantity of arsenic, equalling in weight between a sixth and eighth part of the copper, a greater proportion making the metal brittle.

The way was this: he first melted the copper alone, then put in the arsenic, which being melted, he stirred them a little together, taking care in the mean time, not to draw in breath near the pernicious fumes. After this, he put in tin, and again as soon as that was melted, which was very suddenly, he stirred them well together, and immediately poured them off. He knows not, whether by letting them stand longer on the fire after the tin was melted, a higher degree of fusion would have made the metal porous, but he thought the way he proceeded to be the safest. He adds, that in that metal which he sent to London, there was no arsenic, but a small proportion of silver; as he remembers one shilling in three ounces of metal. But he thought withal, that the silver did as much harm in making the metal soft, and so less fit to be polished, as good in rendering it white and luminous. At another time he mixed arsenic one ounce, copper six ounces, and tin two ounces: and this an acquaintance of his has, as he intimates, polished better than he did the other.

As to the objection, that with this kind of perspectives, objects are found with difficulty, he answers in another letter, of Jan. 6, 16$\frac{71}{72}$, that this is the inconvenience of all tubes that magnify much; and that after a little use the inconvenience will grow less, seeing that himself could readily enough find any day-objects, by knowing which way they were posited from other objects that he accidentally saw in it; but in the night to find stars, he acknowledges it to be more troublesome, which yet may, in his opinion, be easily remedied by two sights affixed to the iron rod by which the tube is sustained; or by an ordinary perspective glass fastened to the same frame with the tube, and directed towards the same object, as Descartes in his dioptrics has described, for remedying the same inconvenience of his best telescopes.

So far the Inventor's letters touching this instrument; of which having communicated the description to M. Christian Huygens, we received from him an answer to this effect, in a letter of Feb. 13, 1672, N. S

I see by the description you have sent me of Mr. Newton's admirable telescope, that he has well considered the advantage which a concave speculum has above convex glasses in collecting the parallel rays, which certainly, according to the calculation I have made, is very great. Hence it is that he can give a far greater aperture to that speculum, than to an object-glass of the same distance of the focus, and consequently that he can magnify objects much more this way than by an ordinary telescope. Besides by it he avoids an inconvenience which is inseparable from convex object-glasses, which is the obliquity of both their surfaces, which vitiates the refraction of the rays that pass towards the sides of the glass, and does more hurt than men are aware of. Again, by the mere reflection of the metalline speculum there are not so many rays lost, as in glasses which reflect a considerable quantity by each of their surfaces, and besides intercept many of them by the obscurity of their matter.

Mean time the main business will be, to find a matter for this speculum that will bear so good and even a polish as glasses, and a way of giving this polish without vitiating the spherical figure. Hitherto I have found no specula that had near so good a polish as glass; and if M. Newton has not already found a way to make it better than ordinarily I apprehend, his telescopes will not so well distinguish objects as those with glasses. But it is worth while to search for a remedy to this inconvenience, and I despair not of finding one. I believe that M. Newton has not been without considering the advantage which a parabolical speculum would have above a spherical one in this construction; but that he despairs, as well as I do, of working other surfaces than spherical ones with due exactness; though else it be more easy to make a parabolical than eliptical or hyperbolical ones, by reason of a certain propriety of the parabolic conoid, which is, that all the sections parallel to the axis make the same parabola.

Thus far M. Huygens's judicious letter, to the latter part of which, concerning the grinding parabolical conoids, Mr. Newton says, in a letter of Feb. 20, 71, that though he, with him, despairs of performing that work by geometrical rules, yet he doubts not but that the thing may in some measure be accomplished by mechanical devices. To all which I cannot but subjoin—

An Extract of a Letter, received very lately (March 19), from the Inventor of this new Telescope, from Cambridge, viz. N° 81, p. 4009.

In my last letter I gave you occasion to suspect, that the instrument which I sent you is in some respect or other indisposed, or that the metals are tarnished. And, by your letter of March 16, I am fully confirmed in that opinion. For while I had it, it represented the moon in some parts of it as distinctly as other telescopes usually do which magnify as much as that. Yet I very well know, that that instrument has its imperfections, both in the composition of the metal, and in its being badly cast, as you may perceive by a scabrous place near the middle of the metal of it on the polished side, and also in the figure of that metal near that scabrous place. And in all those respects that instrument is capable of further improvement.

You seem to intimate, that the proportion of 38 to 1 holds only for its magnifying objects at small distances. But if for such distances, suppose 500 feet, it magnify at that rate, by the rules of optics it must, for the greatest distance imaginable, magnify more than $37\frac{3}{4}$ to 1; which is so inconsiderable a diminishing, that it may be even then as 38 to 1.

Here is made another instrument like the former, which does very well. Yesterday I compared it with a 6-foot telescope, and found it not only to magnify more, but also more distinctly. And to day I found, that I could read in one of the Philosophical Transactions, placed in the sun's light, at 100 feet distance, and that at 120 feet I could discern some of the words. When I made this trial, its aperture (defined next the eye) was equivalent to more than an inch and a third part of the object metal. This may be of some use to those that shall endeavour any thing in reflections; for hereby they will in some measure be enabled to judge of the goodness of their instruments, &c.

Epitome Binæ Methodi Tangentium Doctoris JOHANNIS WALLISII *Geom. Prof. Saviliani Oxoniæ, &c. N° 81, p. 4010.*

These two methods of Dr. Wallis, for drawing tangents to curves, may well be spared here; as they may be read in his Conic Sections and his other works, from whence they are extracted.

Extract of a Letter of M. Hevelius, from Dantzick, of March 9, 1672, N. S. giving some Account of a New Comet, lately seen in that Country. N° 81, p. 4017.

There has been seen here a new comet, from the 2d of March 1672; which I myself being some days absent from home and from my instruments, could not observe till March 6th, in the evening. It is seen both morning and evening. It is but small, having at the present a train not above a degree or a degree and a half long: which would doubtless appear larger, if it were not for the twilight, and the presence of the moon. It is now about the stars in the right arm of Andromeda on her shoulder blade. As far as I can collect from one or two observations, it tends towards the lucida of Andromeda's girdle, and that with a direct diurnal motion of about two degrees in its course. See fig. 4, pl. 15.

The 6th of March in the evening, 7h. 40m. it was in 7° of ♈, in the 35th deg. of north latitude; as I guessed by the hasty inspection of a globe.—March 7, in the morning, 3h. 30m. its longitude was about 8 deg. ♈, with a somewhat less latitude than before: in the evening of the same day its longitude was 10 deg. ♈, and latitude 34° nearly.—March 8, in the morning, 4h. the longitude was 12 deg. ♈, and the latitude 33°. Which yet I would not have taken precisely, because I cannot yet reduce my observations to a calculus. This evening, I hope I shall see it again; although this morning we could see nothing by reason of the dark weather.—I cannot omit to mention, that I have observed again, March 6, 1672, the new star under the head of the constellation of the Swan; but it can hardly be seen as yet with the naked eye.

We have received fresh letters from Paris, informing us that there, and at La Fleeche also, it has been seen from March 16, N. S. to March 26.

We have since been informed by Mr. Isaac Newton, that about the 16th of March $16\frac{71}{72}$, O. S. he saw at night a dull star, south-west of Perseus, which, he says, he now takes to have been that comet of which we gave him information: But he adds, that it was very small, and had not any visible tail, which made him regard it no further; so that he fears that it will now be difficult to find it.

An Account of what has been lately observed by Dr. Kerkringius, concerning Eggs to be found in all sorts of Females. N° 81, p. 4018.

Of this subject some notice has been already taken in N° 70, p. 585, of our Abridgement; but Mr. Oldenburg deemed it proper to give a more detailed account of this author's observations hereon from the French philosophical journals. As the assertion advanced by Kirkringius, that man hath his origin from an egg, has given rise to a diversity of opinion as well as to much ridicule, Mr. Oldenburg has inserted, along with that author's remarks, engravings showing the shape and structure of the female organs of generation.*

Plate 16, figure 1, represents a matrix with its chief dependances: where

B is the matrix.

C the urinary bladder, fastened to the neck of the matrix.

DD the two testiculi, or rather the repositories which contain the eggs spoken of.

EE the two tubes of the matrix.

FF the two vasa deferentia, esteemed by anatomists to convey semen testiculorum in uterum.

GG the two vasa præparantia, for preparing the matter, to be perfected in testiculis.

Fig. 2 represents eggs of different sizes, as Dr. Kerkringius affirms to have found them in the testicles of a woman.—Fig. 3 shows a larger egg, such a one as we have found at Paris in a woman of 40 years of age, and in those of a maid of 18 years.—Fig. 4 exhibits smaller eggs, of which we have found a good number in the testicles of a cow.—Fig. 5 represents an egg, which D. Kerk-

* Nothing can be more unnatural than the figure given in the plate belonging to these observations, of the uterus and its appendages. By testiculi are meant the ovaria, and what are termed vasa deferentia, we imagine to be ligaments. The shape, situation and direction of the Fallopian tubes are strangely misrepresented. But we could not do otherwise than copy the original plate; the main purport of which is, to show the oviform vesicles (of which de Graaf was the discoverer,) contained in the ovaria. In the act of sexual intercourse, it is supposed that one of these vesicles bursts, and that the fringed extremity of one of the Fallopian tubes, being applied closely and with orgasm to that point of the ovarium, the contents (amounting to no more than a single drop) of the said ruptured vesicle, pass into that end of the Fallopian tube, and are conveyed along that channel into the cavity of the uterus. It is also supposed that the semen virile in uterum projectum, traverses along the above mentioned tube, and is brought in contact with the oviform vesicle in the ovarium (for the purpose of fecundation,) at the moment when the fringed extremity of the tube is applied to the ovarium. It is obvious that these explanations of physiologists are for the most part conjectural. It should be added, that after conception a yellow substance (termed corpus luteum) is found in those places of the ovaria, where the oviform vesicles were ruptured, and that on the external surface of the ovaria, cicatrices are perceived at those places.

ringius affirms to have opened three or four days after it was fallen into the matrix of a woman, and in which he saw that little embryo marked B, whereof he found the head began to be distinguished from the body yet without a distinct perception of the organs.*—Fig. 6 shows a larger egg, which D. Kerkringius opened a fortnight after conception, finding in it these particulars;

A, a little secundine.

BBBB the membrane chorion, divided in four places.

CCCC the membrane amnios, divided in the same manner.

D the navel-string, by which the child is fastened to the secundine.

E a child of 14 days after conception, in which the face begins to appear, together with the principal parts of the body.

Fig. 7 represents the skeleton of an infant, found by the same in one of these eggs three weeks after conception.—Fig. 8 exhibits the skeleton of another child, found also by him in an egg, a month after conception.—Fig. 9 represents the skeleton of an embryo, found by him in an egg six weeks after conception.

Though this opinion (says Kerkringius) about the first formation of man in an egg, as that of all fowl, is not common, yet it is true; and if any find it difficult to believe, he may cast his eyes upon fig. 2, where he will see those eggs represented after the life, as I have found them myself in the body of many women opened by me.

These eggs are to be met with, not only in the testicles of women married, but also in those of maids, even as young hens will lay eggs without any commerce with a cock.

These eggs are of the size of a pea, and contain a glutinous liquor, which will be hardened by the fire, just as the white and yolk in other eggs. The taste of them is flat and unpleasant enough; they are invested with one or two fine skins, which stretch themselves a little while after the eggs are fallen into the womb, and change into two membranes, called amnios and chorion. And as these two membranes are always found afterwards, enwrapping the child; so it is very probable, that the eggs of women are also covered with two skins from their beginning; though by reason of their fineness I could not distinctly see them.

It seems that Fallopius has seen these eggs before me; as appears in his anatomical observations. And their use in generation seems easy to be determined, by reflecting on what the expert anatomist Thomas Wharton teaches in his treatise of glands, ch. 33, concerning the manner of conception. For, ac-

* This is irreconcilable with the observations of Haller and other accurate physiologists.

cording to him, semen viri penetrat in testes fæminæ per uteri tubas. Now there it is joined with the egg, in such a manner, much resembling other oviparous animals.

The egg being thus made fecund, descends into the womb through the vasa deferentia, and in two or three days grows to the size of a black cherry. When they fall down, they are a little larger than we have represented them; but being soft, they are easily flatted, and never remain round. If in falling they are handled and slightly pressed, there will stick a little skin to the finger, which shows that it is not seed, nor any thing like it, but of such eggs as we speak of. Fæminæ dejiciunt hæc ova imprimis tempore menstruorum, vel in iræ vehementia.

I have had (says Kerkringius) an occasion favourable enough for examining that germ of three or at most four days, represented in fig. 5. A married woman died 3 or 4 days post fluxum menstruum. I assisted at the opening of her body, and having found in the matrix a little round mass of the size of a large black cherry, I took the husband aside, and asked him, Num à tempore fluxus menstruorum uxorem cognovisset? And having received for answer, that he had, I prayed him to let me carry home with me this little ball, which I had found in her womb. I was no sooner come home but I opened it, and found that nature had wrought with so much activity in so small a time, that one might already see the first lineaments of a child, since we observed in it the head as distinct from the body, and in the head we took notice of some traces of its principal organs. As for the rest of the body, it was nothing as yet but a mass grossly wrought, as you may see in this figure.*

But further, the embryo represented in fig. 6, was only of 15 days, when in its head there were noted the eyes, nose, mouth, and ears; and the body began to have legs and arms, as well distinguishable as appears in this figure; which represents it just as it was given me.

In fig. 7 is delineated a child, which is already furnished with all its cartilages, though it had been conceived but three weeks.

Fig. 8 represents a fœtus of a month, having now the whole human shape, and the bones thereof firm enough in many places to support the parts. Behold the figure well, which represents this little engine in its natural size. It already in a manner sustains itself. The two jawbones appear; the clavicles are formed; and all the ribs are very distinct, except the first and last; which are not wont to have, even in the second month, the consistence of bones. One

* It has been already remarked at p. 413 of our Abridgement, that these observations respecting the existence of a spherical ovum in the uterus of women 3 or 4 days post coitum are not to be relied on.

may see in the arms the joints of the shoulder bones and of the elbows; as also the thighs and both the legs, together with their bones, called focils; which I had not observed, when I wrote my treatise of the Generation of the Bones. All that you see of white in this eighth figure, has at this time the quality of bones.

Fig. 9 represents a child of six weeks after conception; where it is to be noted, that comparing together the bones of divers fœtuses it will be found, perhaps to admiration, that that which has been conceived but a little time after another, has yet the bones in proportion twice as large. That which is here exhibited by fig. 9, appears much smaller than another of two months, as appears in my book of the generation of bones; but the bones are not the less remarkable; for whatever has the hardness and consistence of bones in that, has already the nature of cartilages in this. The inferior jaw-bone is most observable in this child of six weeks, marked A, it being at this age composed of six little bones, which when it is born are all joined together and make but one.

If it be asked how I come to know that these degrees of growth come to pass exactly within those times recited, especially since in abortions we often see embryos of four months and more that are not so big as those spoken of? I might answer by repeating all I said before, when I compared the proportions of those different germs. To which I shall only add, that embryos which miscarry have often remained a long while in the body before they came forth, or have lived there so sickly, as not to draw perhaps half the nourishment necessary for them, and therefore much less than they otherwise would be.*

So far Kerkringius; on whose discourse are made these reflections by M. Denys.

1. That those eggs are generated in fæminarum testiculis, and thence made to descend per tubam into the matrix, in coitu, per vim spirituosam seminis masculi, per uteri tubam penetrantis.

2. That those eggs are of different sizes, since those of the third fig. represent one according to the life, as it was found with nine or ten smaller ones in a woman of forty years of age. Such as were found by him in the testicles of a cow, are duly exhibited in fig. 4. If any wonder that in so large an animal they should be so much smaller than in a woman, he will have more cause to admire that women have them so little in comparison of those of ducks, hens, &c. the first beginnings of things not bearing always a proportion to their state of increase, as beans and peas (*e. g.*) whence grow plants but of a very middling size, are much larger seeds than the kernels of apples and pears, which produce con-

* For more accurate representations of fœtuses at different periods of gestation, see Ruysch's Thesaur. Anat. and Bidloo's works.

siderable large trees. Besides it may be that cows, when in their heat, may afford larger eggs. Meantime the reason why the eggs of fowls are always proportionably greater than those of women and of quadrupeds is, that they, when laid, must contain the matter not only for forming, but also for feeding the young animal.

3. That this opinion is not so new as some imagine; since Fallopius in Observ. Anatom. Bartol. Anat. Reform. l. 1. c. 26. Riolan. Ench. Anat. l. 2. c. 37. Laurent. Anat. l. 7. c. 10, mention them.

But here we shall observe the true state of the question, from the Journal of M. Gaulois, saying that the vesicles or eggs in all sorts of females, are to be considered in three conditions: 1. When they are fastened to the place where nature has lodged them as in a repository. 2. When they are loosened from thence. 3. When they enclose the embryo. The first of these, viz. that there are vesicles in all sorts of females fastened to their bodies, is certain and not new, as appears by the authors just now quoted. It is also certain, that after conception, that which encloses the fœtus is almost like an egg: but this is not new neither, seeing that Hippocrates has observed it lib. de Natura Pueri; and Aristotle has said it more than once, viz. l. 7. Hist. Anim. c. 7. and l. 3. de Gener. Anim. c. 9. To which also the moderns agree, and amongst others the famous Harvey Exper. 68. de Gener. Anim. The question therefore is only, whether these vesicles, fastened to the body of females are loosened from it, and whether that kind of eggs, wherein the embryo is formed, is one of the vesicles loosened? Kirkringius asserts the affirmative. Those who are of a contrary opinion say, that it is certain that that bladder, like an egg in which the fœtus is formed, comes not from elsewhere; since it is known that it is produced in the place of conception, and even how it is there produced; as appears out of Harvey, ibid. et Tract. de Concept. Besides say they, the vesicles found in the body of women are so fastened there, that naturally they cannot be separated from thence; and suppose if they were loosened, there is in the same place where they are, no passage large enough to get through. They add, that if you will give the name of eggs to all the vesicles to be found in the parts of generation, there would also be eggs in the body of men, it being known, that at the side of the vasa deferentia there are found divers vesicles, which anatomists compare to a cluster of grapes by reason of their figure.

The reader, says this Journalist, is left to decide this question. He only intimates, that in the many animals dissected in the Royal Philosophical Academy at Paris, there were never found any vesicles actually loose: but that as to a passage for them, there had been, three years since, dissected a woman, and found in each of the tubæ uteri a manifest cavity going into the bottom of the matrix: adding, that though these conduits appear not open ordinarily, they may yet dilate themselves at the time of conception: as the conduit, through which the eggs of the fowl do pass out of the ovarium into the matrix is usually very close, but yet opens sometimes.

4. To return to M. Denys, he observes, that all other animate creatures (not to speak now of plants) are produced by means of eggs, as birds, insects of all sorts, fishes, (of which last sort, though whales, sea calves and dolphins bring forth live creatures of their kind, yet they first breed them within their bodies in eggs :) and why not quadrupeds also, and the females of mankind?

5. As for eggs, said by Kerkringius to have been found in virginibus, the same M. Denys esteems that probable enough. For, says he, though we had not the instance of hens laying eggs without any congress of a cock; the place where they are bred shows enough, that man contributes nothing to their production; all that he can do being nothing but an attraction of the eggs out of their conservatory, and the making them descend into the uterus, ut ibi irrorentur à semine, et fœcunditatem acquirant; even as the juices of the earth vivify all the plants by insinuating themselves into the grains, and penetrating their skins. And it may be, it is the alteration that befalls these eggs when they are retained too long, which causes the abundance of vapors and disorders which other parts are accused of. On which occasion he alleges a notable example of a young maid of quality, that lately died in the 18th year of her age; who was subject to very frequent hysterical fits of vapors, of which she was one day assaulted with so great violence that it cost her her life. Her body being opened, Testiculus dexter erat flaccidus, et figuræ solitæ; at sinister adeò tumidus et inflatus, ut ovi anatis æquaret magnitudinem: eoque aperto, ovum fuit intus repertum, olivam figura et magnitudine referens, et separatu nequaquam difficile. This he says is still kept by M. Charas.

An Account of some Books. N° 81, *p.* 4027.

I. Plantarum Umbelliferarum Distributio nova, &c. A. Rob. Morrison, Med. et Prof. Botan. Regio, &c. Oxoniæ è Theatro Sheldoniano, 1672.

This work of Morrison's is incorporated into his *Historia Plantarum*; it is therefore unnecessary to particularise its contents.

II. *Λοιμολογία*, sive Pestis nuperæ Londini grassantis Narratio Historica. Auth. Nathan. Hodges, M. D. &c.

Dr. Hodges's treatise on the plague which raged in London during the year 1665, and by which 68,000 persons and upwards perished in the course of 12 months, is so well known to medical men, that it would be superfluous to retain the analysis given by Mr. Oldenburg of its contents.

III. A Philosophical Essay, declaring the probable causes of stones in the greater world, in order to find out the causes and cure of the stone in the kidneys and bladder of men; by D. Thomas Sherley, physician in ordinary to his majesty, Londini, in 8vo.

Contains nothing worth notice.

IV. Caroli Claromontii, M. D. &c. de Aëre, Solo, et Aquis Angliæ, deque Morbis Anglorum vernaculis Dissertatio: Nec non Observationes Medicæ Cambro Britannicæ. Londini, 1672, in 12mo.

In the first of these tracts, the author gives an account of the situation, air, soil, and waters of England: as also of the temper, diet, exercises, and chief sicknesses of the inhabitants thereof; in the other, he delivers several histories of diseases, managed by himself in Wales, describing the nature of each disorder, with the treatment and termination thereof.

Mr. Newton's Letter to the Editor, of March 26, 1672, containing some more Suggestions about his new Telescope, and a Table of Apertures and Charges for the several Lengths of that Instrument. N° 82, p. 4032.

Sir,—Since my last letter I have further compared the two telescopes, and find that of metal to represent, as well the moon as nearer objects, something more distinct than the other; but I am not well assured of the goodness of that other, which I borrowed to make the comparison; and therefore desire that the other experiment should be rather confided in, of reading at the distance of between 100 and 120 feet, at which I and others could read with it in the Transactions, as I found by measure; at which time the aperture was $1\frac{1}{3}$ of an inch, which I knew by trying, that an obstacle of that breadth was requisite to intercept all the light which came from one point of the object.

I should tell you also, that the little plain piece of metal, next the eye-glass, is not truly figured, whereby it happens that objects are not so distinct at the middle as at the edges. And I hope that by correcting its figure, (in which I find more difficulty than one would expect) they will appear all over distinct, and more distinct in the middle than at the edges. And I doubt not but that the performances will then be greater.

But yet I find that there is more light lost by reflection of the metal which I have hitherto used than by transmission through glasses, for which reason a shallower charge would probably do better for obscure objects, suppose such a one as would make it magnify 34 or 32 times. But for bright objects at any distance, it seems capable of magnifying 38 or 40 times with sufficient distinctness. And for all objects, the same charge I believe may with advantage be allowed, if the steely matter, employed at London, be more strongly reflective than this which I have used.

Lengths.	Apertures.	Charges.
$\frac{1}{2}$	100	100
1	168	119
2	283	141
3	383	157
4	476	168
5	562	178
6	645	186
8	800	200
10	946	211
12	1084	221
16	1345	238
20	1591	254
24	1824	263

The performances of one of these instruments, of any length, being known, it will appear by this lateral table, what may be expected from those of other lengths by this way, if art can accomplish what is promised by the theory. In the first column is expressed the length of the telescope in feet, which doubled gives the semidiameter of the sphere, on which the concave metal is to be ground. In the second column are the proportions of the apertures for those several lengths. And in the third column are the proportions of the charges, or diameter of the spheres, on which the convex superficies of the eye-glasses are to be ground.

The use of this table will best appear by example: suppose therefore a half foot telescope may distinctly magnify 30 times with an inch aperture, and it being required to know what ought to be the analogous constitution and performance of a four-foot telescope: by the second column, as 100 to 476, so are the apertures, as also the number of times which they magnify. And consequently, since the half foot tube has an inch aperture and magnifies 30 times; a four-foot tube proportionably should have $4\frac{76}{100}$ inches aperture, and magnify 143 times. And by the third column, as 100 to 168, so are their charges; and therefore if the diameter of the convexity of the eye-glass for a half foot telescope be $\frac{1}{5}$ of an inch, that for a four-foot should be $\frac{168}{500}$, that is, about $\frac{1}{3}$ of an inch.

In like manner, if a half foot telescope may distinctly magnify 36 times with $1\frac{1}{4}$ of an inch aperture, a four-foot telescope should, with equal distinctness, magnify 171 times with 6 inches aperture; and one of six-foot should magnify 232 times, with $8\frac{2}{3}$ inches aperture; and so of other lengths. But what the event will really be, we must wait to see determined by experience. Only this I thought fit to insinuate, that those who intend to make trials in other lengths may more readily know how to design their instruments. Thus for a four-foot tube, since the aperture should be 5 or 6 inches, there will be required a piece of metal 7 or 8 inches broad at least, because the figure will scarcely be true to the edges. And the thickness of the metal must be proportional to the breadth, lest it bend in the grinding. The metals being polished, there may be trials made with several eye-glasses, to find what charge may with best advantage be made use of.

An Extract of another Letter of the same to the Editor, dated March 30, 1672, by way of Answer to some Objections made by an ingenious French Philosopher to the New Reflecting Telescope. N° 82, p. 4034.

Sir,—I doubt not but M.A. will allow the advantage of reflection in the theory to be very great, when he shall have informed himself of the different refrangibility of the several rays of light. And for the practical part, it is in some measure manifest by the instruments already made, to what degree of vivacity and brightness a metalline substance may be polished. Nor is it improbable but that there may be new ways of polishing found out for metal, which will far excel those that are yet in use. And when a metal is once well polished, it will be a long while preserved from tarnishing, if diligence be used to keep it dry and close shut up from air; for the principal cause of tarnishing seems to be the condensing of moisture on its polished surface, which by an acid spirit, wherewith the atmosphere is impregnated, corrodes and rusts it; or at least, at its exhaling, leaves it covered over with a thin skin, consisting partly of an earthy sediment of that moisture, and partly of the dust, which, flying to and fro in the air, had settled and adhered to it.

When there is not occasion to make frequent use of the instrument, there may be other ways to preserve the metal for a long time: as perhaps by immerging it in spirit of wine or some other convenient liquor. And if it chance to tarnish, yet its polish may be recovered by rubbing it with a soft piece of leather, or other tender substance, without the assistance of any fretting powders, unless it happen to be rusty, for then it must be new polished.

I am very sensible that metal reflects less light than glass transmits, and for that inconvenience I gave you a remedy in my last letter, by assigning a shallower charge in proportion to the aperture, than is used in other telescopes. But, as I have found some metalline substances to be more strongly reflective, and to polish better, and be more free from tarnishing than others, so I hope there may in time be found out some substance much freer from these inconveniences than any yet known.

Observationes Jovis ad duas Fixas transeuntis, Derbiæ Anglorum habitæ mensibus Febr. et Martii A. 1671-72, *st. veteris, à* JOH. FLAMSTEED. *N°* 82, *p.* 4036.

The calculations in this paper, from the old tables of Mr. Street, are of no use now, in the present very accurate state of astronomy.

Account of the Return of a large permanent Spot in the Planet Jupiter, observed by Signor CASSINI. *N°* 82, *p.* 4039.

It is now above six years since Cassini published the theory of two sorts of spots, at certain times to be seen in the disk of Jupiter.*

One sort are nothing but the shadows of the four satellites, which he had often very well observed, when these satellites moving through the lower part of their small circles that environ Jupiter, passed between him and the sun which illuminates him, making a kind of solar eclipse, like that which the moon makes, when she is between the sun and the earth. These spots, as he observed from that time, have this peculiar, which distinguishes them from all others, that they are precisely found in that place of Jupiter, where some satellite is seen by the sun; that they go from the oriental limb to the occidental of the disk of Jupiter, with a motion always equal to that of the satellite; that in respect to us they precede the satellite before the opposition of Jupiter to the sun, and follow him after the opposition; that the further Jupiter is distant from the opposition, the greater is the apparent distance of the same satellite; that at divers times of the year this distance changes in proportion to the annual parallax of the satellite, according as he is differently seen by the sun and by the earth; and that at one and the same time of the year, when divers satellites happen to be between Jupiter and the sun, the spots correspondent to them, are distant from them in proportion to the semidiameters of the circles of the same satellites.

The other sort of spots have no dependance at all on the satellites; but it seems that they have some resemblance to those spots that sometimes appear in the sun, or to those that are always seen in the moon; and they are perhaps of the same nature with those that are called belts. These spots also move from the eastern to the western limb of Jupiter's disk; but their apparent motion is unequal, and swifter near the centre than the circumference, and they never

* What was discovered of the permanent spot in this planet here in England by M. Hook, An. 1664, in May, may be seen No. 1, p. 3, compared with No. 4, p. 26, No. 8, p, 52, No. 12, p. 68, No. 14. p. 84.

are so well seen as when they approach to the centre, as they are very narrow, and almost imperceptible when they approach to the circumference: which makes us believe that they are flat and superficial to Jupiter.

Among these spots of the second sort, there is none so sensible as one that is situated between the two belts, which in the disk of Jupiter are usually seen extended from east to west; the largest of which is between the centre and the northern limb, and the narrowest is beyond the centre towards the southern limb. This spot is always adhering to the southern belt, its diameter is about the tenth part of that of Jupiter; and at the time that its centre is nearest to that of Jupiter, it is distant from it about the third part of the semidiameter of that planet.

Signor Cassini, after he had made many observations of this spot during the summer of the year 1665, found that the period of its apparent revolution is nine hours and 56 minutes. Jan. 19, of this present year 1672, (N. S.) when he observed Jupiter at 4¾ o'clock in the morning, he perceived, in the same place of his disk, the figure of the same spot adhering to the same southern belt. It was already gone beyond the moiety of this belt, and he saw it advance little by little towards the western limb, to which it seemed to be very near at 6¼ o'clock: but then it appeared so small and little, and so little sensible, that he was obliged to cease from observing it.

By the calculation he made in six years, of which one is a bissextile, it is found to have made, in respect of the earth, at least 5294 revolutions, each of nine hours 55 minutes, 58 seconds, compensating one revolution by another, and at most 5295 revolutions of nine hours, 55 minutes, 51 seconds; forasmuch as he was assured of the preciseness of one mean revolution to one 8th of a minute, which will be verified by future observations.

Until then he had never yet seen an immediate return of this spot after 9 hours and 56 minutes, because it had not happened that Jupiter, after the appearance of the spot, had stood, in one and the same night, long enough above the horizon, at least at a sufficient height to observe him with due distinctness. He had only concluded the time of this revolution by returns observed after about 20, 30, and 50 hours; and he had more precisely limited it by observations more distant. But the night after the 1st of March, at 7½ o'clock in the evening, he saw this spot in the midst of the belt; and the same night, at 5 o'clock, and 26 minutes in the morning, he saw it again returned precisely to the same place. Next day he made a report of these observations to the Royal Academy of the Sciences, and predicted, that the spot would arrive again at the midst of the belt on the 3d of March, at 8 minues after 9 o'clock at night, whereupon that assembly deputed M. Buot and M. Mariotte to be present at the observation; who, being come to the Royal Observatory, began to see at 4 minutes after 8 o'clock the spot already somewhat removed from the oriental limb, but yet obscure and small. At 47 minutes after 8 o'clock, they saw it very distinctly advancing towards the middle of the belt. From 5 minutes and 40 seconds after 9 o'clock until 8 minutes after 9 o'clock, they saw it in the midst of the belt. At 15 minutes after 9 o'clock it had passed the middle, and was come nearer to the occidental limb. And a little after, the Heavens being overcast, he could then observe it no further.

This observation being taken for the epocha, it is easy to find hereafter the times when this spot shall return to the midst of the belt. For you are only to add always 9 hours and 56 minutes; and, for greater exactness, not to omit the ordinary equation of days, that depends on the inequality of the motion of the sun in respect of the equinoctial, nor the particular equation that depends on the inequality of the motion of Jupiter, according to the diversity of the distance of the sun and his apogee.

This revolution being the swiftest and the most regular that is hitherto known in the Heavens, a traveller alone, even without having any correspondence with other observers, may make use of it to find the longitudes of the most remote places of the earth. We shall hereafter examine to what precision we may arrive by this way.

Observations of a New Comet, made at Paris in the Royal Observatory, by SIGNOR CASSINI. *N° 82, p.* 4042.

The mathematicians of la Fleche perceived the comet from March 16, N. S. and gave notice of it at Paris. Those of the college of Clermont saw it March the 25th.

March 26, at half an hour after seven, in the evening, S. Cassini saw it between the head of Medusa and the Pleiades. Without a telescope he appeared as a star of the third magnitude. Its head, seen with a telescope of 17 feet, appeared almost round, but well defined, and distinguished from the mistiness which formed a kind of chevelure, or bush of hair, with which it was encompassed; and even the middle was a little confused, and seemed to have inequalities as are seen in clouds. The tail, which is principally that which distinguishes comets from stars, was almost imperceptible; yet by the telescope it was seen turned opposite to the sun, and it appeared of the length of two diameters of the head. The whole comet, head, tail, and chevelure, taken together, took up no more than three or four minutes of a degree.

At 48 minutes after seven it was in a straight line with the lucida in the head of Medusa, and with the most western star of the Pleiades, and above the two clearest stars of the southern foot of Perseus; so that a straight line drawn through these two stars, almost touched the southern extremity of his chevelure. This place of the comet, transferred upon the map of the fixed stars, fell on 23° 25′ of Taurus, in 14° N. lat.

March the 28th, at 42 minutes after seven, in the evening, the comet was 24′ distant from the less bright star of the southern foot of Perseus, and had nearly the same latitude with it. So that it was precisely enough at 26° 8′ of Taurus, and in the latitude of 12° 8′. At 14 minutes after 8, the distance of the comet from Aldebaran was 19° 38′, and at 29 minutes after 8, the distance from Capella was found to be 22° 32′.

March 30, at 35 minutes after 9 at night, the comet seen without a telescope, appeared as a star of the 4th magnitude; through the telescope he exceeded even those of the first, but it was very obscure, and without any perceptible tail. It had passed one degree and a half beneath the lucida of the southern foot of Perseus; so that this star was exactly in the midst between the comet and the little star in the leg of Perseus, marked η by Bayer; its place there being 28° 45′ of Taurus, and 9° 56′ N. lat.

March 31, at eight in the evening, the comet was in a direct line with the lucida in the foot of Perseus, and with the most northern in the head of Taurus; but he was more than twice as far from the first as from the other, which being transferred on the map of the fixed stars, gave 15′ of Gemini, in latitude 8° 49′.

April the 1st, it had passed 45′ beyond the most northern star of the head of Taurus, and that he must have touched it by its southern limb; as also that it was distant 1° 43′ from the star that was nearest to that toward the south. This place being transferred on the map of the fixed stars, it gave 30′ of Gemini, in lat. 7° 44′.

April the 2d, at eight o'clock in the evening, it was 2° 30′ distant from the most northern star of Taurus; and one degree from the star of the ear, marked φ by Bayer, and by Tycho, called sequentis lateris borea. Two lines drawn from the most northern star of Taurus, one to the comet the other to the star that is wanting in Bayer, made a right angle; and the distance of the comet from this angle, was double of that which is between these two stars. This place transferred on the map of the fixed stars, fell on 2° 48′ of Gemini, in lat. 6° 40′ N.

At 50m. after 6, the line drawn through the horns of the moon passed through the star, that is at the point of the northern horn of Taurus, and the distance of this star to the northern horn of the ☽ was, by a minute, greater than the semidiameter of the moon.

April the 3d, at nine o'clock, the comet was at 4° from the sign of Gemini, in N. lat. 5° 38′.

April the 5th, at eight o'clock in the evening, the comet had passed the northern ear of Taurus, and was equally distant from the upper star of the northern ear, and from that which was on the front of Taurus. He was also as distant from the inferior star of the ear of Taurus, as this star is from the next westward, by Tycho called inferior præcedentis lateris quadrilateri, and a straight line, drawn through the comet and the upper star of the ear, made an almost right angle with another line, drawn from the comet to the inferior of the two small stars that are above the eye of Taurus. This place being transferred to the map of the fixed stars, the comet was found at 6° 18′ of Gemini, in lat. 3° 41′.

April the 6th, at eight o'clock in the evening, the comet was found at 7° 25′ of Gemini, in lat. 2° 45′.

April the 7th, at nine o'clock in the evening, the comet was in 8° 30′ of Gemini, in lat. 1° 56′.

Signor Cassini's reflections on the foregoing Observations.

All the places of the comet fall into a line little differing from an arch of a great circle, which cuts the ecliptic in 10° 45′ of Gemini, and which consequently has its greatest latitude in 10° 45′ of Pisces; which latitude is between 39 and 40 deg. N. The same circle cuts the equator at 101° of the vernal section eastward, and its greatest declination from the equator northward, is $38\frac{1}{2}$ degrees.

Having chosen two of our first observations, and having taken a mean between the first observations of the mathematicians of la Fleche, we have found by this method, that the comet had been in his perigee, March 12, at 8 o'clock in the morning. That in that time, which was that of its greatest apparent celerity, it made about 2° 32′ a day in the great circle of its apparent motion, and $\frac{444}{10000}$ of its perigee distance in the line of its equal motion. That it was in its greatest declination the 11th and 12th of March; and that at that time it passed through the inferior meridian at about two o'clock after midnight.

It is a thing worth observing, that this comet keeps its course almost like that of the 2d comet of 1665, and of another of 1577, observed by Tycho. For they have passed through almost the same constellations, though this be more inclined northward, and cut the ecliptic 5 or 6 degrees more forward than that of 1665. So that it seems that in this place of the Heavens there is, as it were, a zodiac for comets.

An Account of some Books. N° 82, p, 4050.

I. De Resistentia Solidorum Alexan. Marchetti, in Pisana Academia Phil. Prof. Excusum Florentiæ 1665, in thin 4to.

This book was promised some years ago under the title of Galilæus Ampliatus. But the author now follows not the steps of Galilæus, but demonstrates all his propositions another way; building all upon this ground: Momenta Gravium proportionem habent compositam ex proportionibus ponderum et longitudinum: which is his first proportion.

II. Tabula numerorum Quadratorum decies millium, unà cum ipsorum Lateribus ab Unitate incipientibus, et ordine naturalibusque ad 10000, progredientibus. Londini, 1672.

A table of ten thousand square numbers, namely of all the square numbers between 0 and 100 millions; and of their sides or roots: which are all the whole numbers between 0 and ten thousand: 8 sheets in fol.

III. Regneri de Graaf. de Mulierum Organis Generationi Inservientibus Tractatus novus. Lug. Bat. 1672, in 8vo.

The subject of the organs of generation in women having been discussed more than once in the preceding part of this volume, we deem it unnecessary to insert the analysis of this treatise of de Graaf's, whose works are in the hands of almost every anatomist.

IV. Discours de la Connoissance des Bestes; par le P. Ignace Gaston Pardies, S. J. A Paris, 1672, in 12mo.

The author's opinion is, that beasts have only a simple apprehension of objects without reflection.

Mr. Isaac Newton's Considerations on part of a Letter of M. de Berce, concerning the Catadioptrical Telescope, pretended to be improved and refined by M. Cassegrain. N° 83, p. 4056.

That the reader may be enabled the better to judge of the whole, by comparing together the contrivances both of Mr. Newton and Mr. Cassegrain; it will be necessary to borrow from the French memoir what is there said concerning them; which is as follows:—

I send you (says M. de Bercé to the editor of the memoir,) the copy of the letter which M. Cassegrain has written to me concerning the proportions of Sir Samuel Moreland's trumpet. And as for the telescope of Mr. Newton, it has as much surprised me as the former. For it is now about three months since M. C. communicated to me the figure of a telescope, which was nearly like Newton's, and which he had invented; but which I look upon as more ingenious. I shall here give you the description of it in short.

ABCD is a strong tube, in the bottom of which there is a great concave speculum CD, pierced in the middle E. F is a convex speculum so disposed, as to its convexity, that it reflects the species which it receives from the great speculum, towards the hole E, where is an eye-glass, which one looks through.

The advantage which I find in this instrument above that of Mr. Newton, is first, that the mouth or aperture AB of the tube may be of what size you please; and consequently you may have many more rays on the concave speculum, than upon that, of which you have given us the description.—2. The reflection of the rays will be very natural, since it will be made upon the axis itself, and therefore more vivid.—3. The vision of it will be so much the more pleasing, as you will not be incommoded by the great light, by reason of the bottom CD, which hides the whole face. Besides that you have less difficulty in discovering the objects than in Mr. Newton's.

The following are the Considerations of Mr. Newton, as we received them from him in a Letter, written from Cambridge, May 4, 1672.

SIR—I should be very glad to meet with any improvement of the catadioptrical telescope; but that design of it, which (as you inform me) Mr. Cassegrain has communicated 3 months since, and is now printed in one of the French memoirs, I fear will not answer expectation. For when I first applied myself to try the effects of reflections, Mr. Gregory's Optica Promota (printed in the year 1663) having fallen into my hands, where there is an instrument (described in page 94) like that of M. Cassegrain's, with a hole in the midst of the object metal, to transmit the light to an eye-glass placed behind it; I had thence an occasion of considering that sort of constructions, and found their disadvantages so great, that I saw it necessary, before I attempted any thing in the practice, to alter the design of them, and place the eye-glass at the side of the tube, rather than at the middle.

The disadvantages of it you will understand by these particulars.—1. There will be more light lost in the metal by reflection from the little convex speculum, than from the oval plane. For it is an obvious observation, that light is most copiously reflected from any substance when incident most obliquely.—2. The convex speculum will not reflect the rays so truly as the oval plane, unless it be of an hyperbolic figure; which is incomparably more difficult to form than a plane; and if truly formed, yet would only reflect those rays truly which respect the axis.—3. The errors of the said convex will be much augmented by the too great distance, through which the rays reflected from it must pass before their arrival at the eye-glass. For which reason I find it convenient to make the tube no wider than is necessary, that the eye-glass be placed as near to the oval plane as is possible, without obstructing any useful light in its passage to the object metal.—4. The errors of the object metal will be more augmented by reflection from the convex than from the plane, because of the inclination or deflection of the convex on all sides from the points, on which every ray ought to be incident.—5. For these reasons there is requisite an extraordinary exactness in the figure of the little convex, whereas I find by experience, that it is much more difficult to communicate an exact figure to such small pieces of metal, than to those that are greater.—6. Because the errors at the perimeter of the concave object metal, caused by the sphericalness of its figure, are much augmented by the convex, it will not with distinctness bear so large an aperture, as in the other construction.—7. By reason that the little convex conduces very much to the magnifying virtue of the instrument, which the oval plane does not, it will magnify much more in proportion to the sphere, on which the great concave is ground, than in the other design; and so magnifying objects much more than it ought to do in proportion to its aperture, it must represent them very obscure and dark; and not only so, but also confused by reason of its being overcharged. Nor is there any convenient remedy for this. For if the little convex be made of a larger sphere, that will cause a greater inconvenience by intercepting too many of the best rays; or, if the charge of the eye-glass be made so much shallower as is necessary, the angle of vision will thereby become so little, that it will be very difficult and troublesome to find an object, and of that object when found, there will be but a very small part seen at once.

By this you may perceive, that the three advantages which M. Cassegrain proposes to himself, are rather disadvantages. For, according to his design, the aperture of the instrument will be but small, the object dark and confused, and also difficult to be found. Nor do I see why the reflection is more upon the same axis, and so more natural, in one case than in the other: since the axis itself is reflected towards the eye by the oval plain; and the eye may be defended from external light as well at the side, as at the bottom of the tube.

You see therefore, that the advantages of this design are none, but the disadvantages so great and unavoidable, that I fear it will never be put in practice with good effect. And when I consider, that by reason of its resemblance with other telescopes, it is something more obvious than the other construction; I am apt to believe, that those, who have attempted any thing in catoptrics, have ever tried it in the first place, and that their bad success in that attempt has been the cause why nothing has been done in reflections. For Mr. Gregory, speaking of these instruments in the aforesaid book, page 95, says, De mechanica horum speculorum et lentium, ab aliis frustrà tentatâ, ego in mechanicis minus versatus nihil dico. So that there have been trials made of these telescopes, but yet in vain. And I am informed, that about 7 or 8 years since, Mr.

Gregory himself, at London, caused one of 6 feet to be made by Mr. Reive, which I take to have been according to the aforesaid design described in his book; because, though made by a skilful artist, yet it was without success.

I could wish therefore, M. Cassegrain had tried his design, before he divulged it: but if, for further satisfaction, he please hereafter to try it, I believe the success will inform him, that such projects are of little moment till they be put in practice.

Some Experiments proposed in relation to Mr. NEWTON's Theory of light, printed in N° 80; with the Observations made thereupon by the Author of that Theory, communicated in a Letter of his from Cambridge, April 13, 1672. N° 83, p. 4059.

1. To contract the beams of the sun without the hole of the window, and to place the prism between the focus of the lens and the hole, spoken of in M. Newton's theory of light.

2. To cover over both ends of the prism with paper at several distances from the middle; or with moveable rings, to see how that will vary or divide the length of the figure, insisted upon in the said theory.

3. To move the prism so, as the end may turn about, the middle being steady.

4. To move the prism by shoving it, till first the one side, then the middle, then the other side pass over the hole, observing the same parallelism.

The Observations made upon these Proposals.

I suppose the design of the proposer of these experiments is, to have their events expressed, with such observations as may occur concerning them.—1. Touching the first, I have observed, that the solar image falling on a paper placed at the focus of the lens, was by the interposed prism drawn out in length proportional to the prism's reflection or distance from that focus. And the chief observable here, which I remember was, that the straight edges of the oblong image were more distinct than they would have been without the lens.

Considering that the rays coming from the planet Venus are much less inclined one to another, than those which come from the opposite parts of the sun's disk; I once tried an experiment or two with her light. And to make it sufficiently strong, I found it necessary to collect it first by a broad lens, and then interposing a prism between the lens and its focus, at such distance that all the light might pass through the prism, I found the focus, which before appeared like a lucid point, to be drawn out into a long splendid line by the prism's reflection. I have sometimes designed to try, how a fixed star, seen through a long telescope, would appear by interposing a prism between the telescope and my eye. But by the appearance of Venus, viewed with my naked eye through a prism, I presage the event.

2. Concerning the second experiment, I have occasionally observed, that by covering both ends of the prism with paper, at several distances from the middle, the breadth of the solar image will be increased or diminished as much as is the aperture of the prism, without any variation of the length: or, if the aperture be augmented on all sides, the image on all sides will be so much augmented, and no more.

3. Of the third experiment I have occasion to speak in my answer to another person; where you will find the effects of two prisms in all cross positions of one to another described. But if one prism alone be turned about, the coloured image will only be translated from place to place, describing a circle, or some other conic section, on the wall on which it is projected, without suffering any alteration in its shape, unless such as may arise from the obliquity of the wall, or casual change of the prism's obliquity to the sun's rays.

4. The effect of the fourth experiment I have already insinuated, telling you (in page 679) that light, passing through parts of the prism of divers thicknesses, did still exhibit the same phænomena.

Note, that the long axes of the two prisms, in the experiment described in the said page 679, were parallel one to another. And for the rest of their position, you will best apprehend it by this scheme: where, let EG design the window (fig. 6, pl. 15); F the hole in it, through which the light arrives at the prisms; ABC the first prism, which refracts the light towards PT, painting there the colour in an oblong form; and $\alpha\beta\gamma$ the second prism, which refracts back again the rays to Q, where the long image PT is contracted into a round one.

The plane $\alpha\gamma$ to BC, and $\beta\gamma$ to AC, I suppose parallel, that the rays may be equally refracted contrary ways in both prisms. And the prisms must be placed very near to one another; for if their distance be so great, that colours begin to appear in the light before its incidence on the second prism, those colours will not be destroyed by the contrary refractions of that prism.

These things being observed, the round image Q will appear of the same size which it does when both the prisms are taken away, that the light may pass directly towards Q from the hole without any refraction at all. And its diameter will equal the breadth of the long image PT, if those images be equally distant from the prisms.

If an accurate consideration of these refractions be designed, it is convenient that a lens be placed in the hole F, or immediately after the prisms, so that its focus be at the image Q or PT. For thereby the perimeter of the image Q, and the straight sides of the image PT, will become much better defined than otherwise.

Account of a Stone cut out from under the Tongue of a Man. By Mr. Lister. *N° 83, p.* 4062.

The man's opinion is, that it was caused by a winter sea voyage, which lasted much longer than he expected, and wherein he suffered an exceeding cold; and that, not long after his landing, he perceived a nodus or hard lump in the place whence this stone was cut. About eight years passed between its breeding and being taken away.

As to its growth and the inconveniences thence ensuing; he further says, that upon all fresh cold-taking, he suffered much pain in that part; and yet that cold once being over, the part was no more painful than the rest of his mouth. He adds, that towards the seventh and eigthth year it often caused sudden swellings in all the glands about the mouth and throat, upon the first draught of beer at meals, which yet would in a short time fall again.

Lastly as to the particulars remarkable at the time of its being taken away, he relates; that it began its work with a sudden vertigo, which vertiginous disposition continued more or less, from spring till August, in which month, without any previous cause, save riding, the place where it was lodged suddenly swelled, and emitted purulent matter at the aperture of the Whartonian duct. That it suddenly stopt its running, (which he cannot attribute to any thing but cold) and swelled with a great inflammation, and very great danger of choaking, causing extreme pain to the party when endeavouring to swallow any thing liquid.

This extremity lasted five days, in all which time the party had so extreme a salivation that he could not sleep, without wetting all the bed about him; so as that it was supposed by his visitors that he had made use of some mercurial medicine. The first day, the saliva ran thin and transparent almost like water without bubbles. The second day it ran frothy, it tasted salt, (which yet he is apt to think hot rather than really salt, because that day the inflammation was at the height). The third day it roped exceedingly, and a small pin hole broke directly over the place of the stone, and ran with purulent matter as formerly. The fourth day the saliva ran insipid, sensibly cold in the mouth, and very little frothy; the coldness confirms me in the opinion, that the former sharp taste was the effect of heat, and not the immediate quality of a salt humour. The fifth day (which was the day of the incision) it ran as on the fourth, but left an extreme clamminess on the teeth, insomuch that they often clave together, as though they had been joined together with glue.

Upon the incision, which proved not wide enough, the membranes or bags, wherein the stone lay, came away first. As to the stone itself it was so hard as to endure the forceps in drawing it forth. It was covered over with grass-green matter, which soon dried, and left the stone of a whitish colour. It is but light in proportion to its bulk, weighing about 7 grains, and is much of the shape of our ordinary horse beans. There are visible impressions upon it of some capillary and small vessels where it was bred amongst. It is scabrous or rough, sand-like, although the substance is tophaceous.

The accidents accompanying the working away of this stone, (for the incision was merely obstetrical,) and the place of its birth, gave occasion to call the distemper a ranula. Yet in truth this was nothing else but one of those tumours called atheroma, and therefore we will name it lapis atheromatis.*

Extract of a Letter from the same Mr. Lister, *written from York, April* 12, 1672, *concerning animated Horse-hairs; rectifying a vulgar Error.* *N°* 83, *p.* 4064.

It has been credibly reported, that horse hairs thrown into water will be animated; and yet I shall show you by an unquestionable observation, that such things as are vulgarly thought animated hairs, are very insects, nourished within the bodies of other insects, even as ichneumones are within the bodies of caterpillars.

I will premise the particulars concerning this animal as I find them collected by the industry of Aldrovandus, and save you the trouble of that voluminous author. This insect, says he, seems to have been unknown to the ancients, as it is called by the moderns seta aquatica or vermis setarius, either from the very slender form of the body, or because it is thought to be generated of a horse-hair putrifying in water. The Germans call them by a name rendered vituli aquatici.

It is bred in corrupt waters, perhaps of horse-hair,† for (says Albertus, upon his own frequent trial, as I find him quoted by Aldrovandus,) these hairs, put into standing water move and are animated, or, as he words it, vitam et spiritum accipiunt, et moventur. Others have thought them to have their birth

* Instances of sublingual calculi have been frequently recorded by writers on surgery. The term atheroma seems to be little adapted to this case.

† Certainly not. See note at the end of this paper.

from weeds hanging down from the banks into ponds and rivers. Others from locusts and grashoppers (ex bruchis;) which last, though it be near the matter, yet it is rejected by Aldrovandus himself, as the most unlikely. They have been found in a cold and good spring, and elsewhere (which is a wonder, says Aldrovandus) upon a leaf in the garden. And this, which was there found, was five or six fingers-breadth long; the thickness of a bristly horse-hair, with a duskish back and a white belly, and the tail on every side white. I saw, says Aldrovandus, a black one, thicker than the whitish one. Other authors describe them otherwise, as Bertrutius, Albertus, &c. Some affirming them to have been a cubit long, others two cubits, others 9 inches long at the least; that they are white of colour, and so hard as scarcely to be crushed with one's foot: to be every where of the same thickness; that they move not as worms move but snake like, and knit themselves up into knots; that their skin is one continued thing without incisures, and therefore some would exclude them from the insect kind; that they have no head but swim both ways, and therefore may be called amphisbæna aquatica; that they are poison drunk down into the stomach, but not venom to the touch. And thus much out of Aldrovandus.

Our observation is this. April 2, there was thrown up out of the ground of my garden, in digging amongst other things of this nature, a coal black beetle, of a middle size, and flat shape, and which I have observed elsewhere common enough. These beetles I dissected on account of some curiosity, wherein I had a mind to satisfy myself. But I was surprised to find in their swollen bellies several of these hair worms, in some three, in others one only. The following particulars we carefully noted. 1. That upon the incision they crawled forth of themselves. 2. That putting them into water they lived many days, and seemed endeavouring to escape by lifting up their heads out of the water, and fastening them to the side of the vessels, very plainly drawing the rest of their body forward. 3. That they cannot be said to be amphisbæna, but move forward by the head only, which is fairly distinguishable from the tail by a notable blackness. 4. That the three I took out of the body of one beetle, were all of a dark hair colour, with whitish bellies, somewhat thicker than hogs bristles; but I took out of the body of another beetle, one that was much thicker than the rest, much lighter coloured, and by measure just five inches and a half long, whereas all the rest did not exceed three inches and three quarters.*

* The worm here described is the *Gordius Aquaticus* of Linnæus, and is by no means uncommon in stagnant waters during the summer months: it is also occasionally found in damp ground in gardens, &c. particularly after great rain accompanying thunder storms. The kind described by Lister as found in the bodies of beetles, is supposed to constitute a distinct species, and in the Gmelinian edition of the Systema Naturæ, is even referred to a different genus under the name of *Filaria*, it is the *Filaria*

Extract of a Letter, written March 5, 1672, by Dr. THOMAS CORNELIO, *a Neapolitan Philosopher and Physician, to* JOHN DODDINGTON, *Esq. his Majesty's Resident at Venice; concerning some Observations made of persons pretending to be stung by Tarantulas. Translated by the Editor.* N° 83, *p.* 4066.

In the country of Otranto, where those insects are in great numbers, a man, who thought himself stung by a Tarantula, showed in his neck a small speck, about which, in a very short time, there arose some pimples full of a serous humour, and in a few hours after, he was sorely afflicted with very violent symptoms, as syncopes, great agitations, giddiness of the head, and vomiting; but without any inclination at all to dance, and without any desire of having musical instruments, he miserably died within two days.

The person who gave me the above particulars affirmed, that those who think themselves bitten by Tarantulas, are for the most part young wanton girls, whom the Italian writer calls Dolci di sale, who by some particular indisposition falling into this melancholy madness, persuade themselves, according to the vulgar prejudice, that they have been stung by a Tarantula. And I remember to have observed in Calabria, some women, who, seized on by some such accidents, were accounted to be possessed with the devil, it being the common belief in that province, that the greatest part of the evils which afflict mankind proceeds from evil spirits.

This brings to my mind a terrible evil, which is often observed in Calabria, and is called in their language Coccio maligno. It arises on the surface of the body, in the form of a small speck of the size of a lupine. It causes some pain, and if it grow not soon red thereupon, it in a very short time certainly kills. It is the common opinion of those people, that such a distemper befalls those only that have eaten flesh of animals dead of themselves: which opinion I can from experience affirm to be false. So it frequently falls out, that of many strange effects we daily meet with, the true cause not being known, such a one is assigned as is grounded upon some vulgar prejudice. And of this kind I esteem to be the vulgar belief of the cause of that distemper, which appears in those that think themselves stung by Tarantulas.

But why should we not rather think that that distemper is caused by an inward disposition, like that which in some places in Germany is wont to produce that evil, which they call Chorea St. Viti, (St. Vitus's dance). But of this I hope I

Scarabæi of that author. Another species is sometimes found in the bodies of caterpillars, and is the *Filaria Lepidopterorum* of Gmelin: it is described by Goetze, Roesel, Degeer, &c. &c.

shall soon be able to write my thoughts more fully, which will, I think, be sufficient to refute the fable of the Tarantula.*

An account of the Aponensian Baths near Padua, communicated by the same Mr. DODDINGTON, *in a Letter dated Venice, March* 18, 1672. *N°* 83, *p.* 4067.

Five miles from Padua are the waters called Aponensia, from the town Aponum, famous in antiquity, and among others frequently mentioned by Livy.

1st, The waters are very hot. 2dly, They are stinking. 3dly, They yield a great deal of very fine salt, of which the natives serve themselves in their ordinary occasions. This salt is the thing I think most considerable there. It is gathered in the following manner: the natives, after sun set, stir pieces of wood in the water, and presently the salt sticks to them, and comes off in small flakes exceedingly white. This never loses its savour. The people there with the same water wash their walls to render them whiter than ordinary, which has an effect superior to lime. Such walls retain their saltness a few days only, and then become insipid, even though they exude a white excrescence, in thin and light flakes like nitre, many years after. But that salt which is collected from the stones, gravel and earth, by which the rivulets, descending from those baths do run, is without any taste of salt; though there be no difference in the form or colour from that which is gathered with the wooden instrument.

Reflections made by P. FRANCISCO LANA, *S. J. on an Observation of Signor M.* ANTONIO CASTAGNA, *concerning the formation of Crystals: translated from the XI. Venetian Giornale de Litterati. N°* 83, *p.* 4068.

In the month of September last, being arrived in the Val Sabbia at a place called le Mezzane, where I knew those crystals to be generated, I observed in a spacious meadow, on a hillock, some narrow places bare of all herbs, in which alone those crystals are produced, being all hexangular, both points of them terminating in a pyramidal figure hexangular likewise.

I was told that they were produced from the dews, because being gathered

* The common opinion relative to the bite of the Tarantula, and its cure by music, is now sufficiently exploded.

The small, sudden, and painful swellings, mentioned in the latter part of this paper, seem rather to resemble the supposed effects of that highly singular animal the *Furia Infernalis* of Linnæus, the history of which is still but obscurely understood.

over night, the next morning there would be found others, at such a time only when it was a serene and dewy sky; and that on the herbs of the meadow, and without the bounds of those bare and sterile places, never any crystals were to be found; besides, that the ground having been in some places bared of all greens, and reduced to the condition of those other naked places, yet no crystals were ever seen to have been formed there. But when I had examined that in the neighbourhood of that hill there was no mark at all of any mines, I concluded that it might be a plenty of nitrous steams which might hinder vegetation in those places, and coagulate the dew falling thereon. And that those exhalations were rather nitrous than of another kind, I was induced to believe, because nitre is not only the natural coagulum of water, as is manifest in artificial glaciations; but also it ever retains the above hexangular figure, altogether like that of those crystals. Which may also be the very cause of the hexangular figure in snow; this being nothing else but water concreted by its natural coagulum, which is a nitrous exhalation. And to make it yet more manifest, that these are indeed expirations of nitre, I dug up some of the earth, and drew a salt from it, which had both the taste and figure of nitre, though some grains of it were of a square, others of a pyramidal figure.

Account of an Inland Sea (or Lake) near Dantzick, yielding at a certain season of the year a green substance, which causes certain death; with an Observation on White Amber; communicated by Mr. Kirkby, in a Letter to the Editor, from Dantzick, Dec. 19, 1671. *N°* 83, *p.* 4069.

Near a small village called Tuckum, two German miles and a half distant from this city westward, there is an inland sea (made by the meeting of three rivulets, some springs from the adjoining hillocks, and the descending rain and snow water,) of about half a German mile long, and an eighth part of such a mile broad. It stretches N. N. W. and S. S. W. About the middle of the bow on the east side it discharges itself with a pretty stream, as it also does in another place more southerly. The soil of the groundround about seems to be sand mixed with clay. Its shore generally sandy, as is its bottom also. Its depth, where deepest, 4 fathoms, but generally but 1 or 1½. It is stored with wholesome and delicate fish, as perch, roach, eels, &c. and famed for a small fish, much esteemed here, and not much unlike a perch, only not so party-coloured, and having a larger head in proportion to its body, called the cole-perch. The water sweet and wholesome; but only in the three summer months, June, July, and August, it becomes every year, during the dry weather, green in the middle, with a hairy efflorescence, which green substance, being by some violent wind forced ashore, and with the water drunk by any cattle, dog, or poultry, causes certain and sudden death; whereas at the same time, that a knowing and ingenuous person saw three dogs killed with it, the horses that were rode into the water beyond the place where this green substance floated, drunk without any hurt, and that also during the same season, the water in the streams that flow from it are wholesome.

One thing more I must add, that the chief fisher here informed me, that two or three years ago fishing in this sea, his net brought up a considerable large piece of white amber, which as a rarity he presented to one of the chief Fathers of the Olive's abbey, to which this sea belongs. Now since this sea is not to be suspected to come from the ocean, it lying so high, and about three German miles distant from the ocean; and since also the neighbouring woods, that bear only very resinous trees, cannot be reasonably said to furnish such amber, that conjecture which imports, that amber is a bituminous fluid substance, hardened by the operations of the aqu-aërial particles upon it, may receive some confirmation from this account.

An Account of some Books. N° 83, p. 4071.

I. De Anima Brutorum Exercitationes duæ, &c. Auth. Thoma Willis, M. D. Philos. Natur. Prof. Sidlei. Oxon. nec non Med. Coll. Lond. et Soc. Reg. Socii. Oxon. An. 1672, in 4to.

We shall not detain our readers with an account of the speculative opinions, (however ingenious) and obsolete pathological doctrine delivered in this book.

II. Suite des nouvelles Experiences sur la Vipere, avec une Dissertation sur son Venin; per Moyse Charas. A Paris, 1671, in 8vo.

This author obstinately maintains what he so erroneously asserted in his former observations on this subject. (See pp. 411-412 of this vol.) "That the venom of vipers is only in their enraged spirits, and not in the yellow liquor contained in the vesicles attached to their gums:" as stated and proved by Redi. See pp. 58 and 654 of this vol.

III. The Chirurgical and Anatomical Works of Paul Barbette, M. D. Practitioner at Amsterdam; together with a Treatise of the Plague. Translated from Low Dutch. London 1672, in 8vo.

An analysis of this Treatise would afford little satisfaction to our medical and chirurgical readers, who well know that both medicine and surgery, (but particularly the last) have received great improvements since the time when Barbette wrote.

IV. The American Physician; or a Treatise of Roots, Plants, Trees, Shrubs, Fruit, Herbs, &c. growing in the English Plantations in America: whereunto is annexed a Discourse of the Cacao-nut tree, and the use of its fruit. By W. Hughes. London 1672, in 12mo.

In this tract we have not only an account of the vegetables growing in the English plantations of America, but also of several things not belonging to the vegetable kingdom; such as the white-coral-rocks, found upon the coast of Jamaica, as well as in other parts of America; the Sea-star fish; the Alligator; an easy way of making good salt in Jamaica, &c.

Some Additions to the Narrative that was published in Number 58, on the Conjunction of the Ocean and Mediterranean by a Canal in France. By Mons. de Froidour. N^o 84, *p.* 4080.

In the description of this Canal, inserted in Number 58, mention was made of the great magazine of water, for a continual supply in case of want. This storehouse is in a valley a little above the town of Revel, at St. Feriol: and is to be filled with the waters of the rivulet Audot, and those from the rain and snow on the Black Mountain. This valley is very narrow at the beginning, large in the middle, and straitened at the foot by the approach of two rocky hills bounding it on both sides; which, to make a lake and to keep in the water, are conjoined by a causey, of such a height and thickness, that it may be called a third hill. This causey is 61 fathoms broad, and is to be 25 high, and 500 long, to gain the hills on both sides. The basis of this great work is a solid body of masonry, laid and every way enclosed within the rock. It has only a small opening below in the form of a vault, which is even with the ground, to serve as a passage to the water of this magazine. This passage is 9 feet wide, 12 high, and 94 fathoms long. Upon this body of masonry, which by some fathoms exceeds the height of the said vault or aqueduct, there is raised a thick wall from the top of this dam, down in a straight line to the foot of it. This wall encloses within its thickness another vault in the manner of a gallery; the entry whereof, being at the foot of the causey, is of the same height and breadth with the former. This gallery answers directly from the top of the causey to the orifice of the aqueduct, 5 fathoms above the surface of the ground, and runs down along the side, and on the left hand of its mouth.

On this work three thick walls are built across it from one end of the causey to the other, founded upon the body of the masonry, and running into the work of the gallery which they traverse crosswise. They are ancred and enchased on the right and the left, in the rocks of the two skirts of the valley. The first wall, which is at the beginning of the causey, is 12 feet thick at the end, being much broader below by reason of the talus or slope: it is to be but 12 fathoms high. The second, to be more raised, is 3 fathoms thick, and 25 fathoms high: it stands very near the midst of the causey, 33 fathoms distant from the first. The third, which makes the foot of the causey, and is 31 fathoms off from the second, is 8 feet thick, and 15 fathoms high. The empty space between the first and second wall, is to be filled with stones and earth well rammed together and made even, so that it may be covered with a bed of loam 6 feet thick, sloping, and insensibly descending from the second to the first wall, so that the water, which will be made to swell to the height of 20 to 25 fathoms, spreading itself upon this glacis or slope, and to lean every where on its centre, may not spoil the causey. In the like manner is to be filled up the empty space between the second and third wall, descending also slopewise from the second to the third, to serve for a buttress to the second, that is to bear all the weight and force of the water.

All these walls, and even those of the gallery, are to be counter-walled by a wall of 2 feet thickness. Besides, the gallery is to be counter-vaulted by another vault; and the intermediate empty spaces are to be done up with a clayey earth well rammed; that so, in case that by any extraordinary accident the water should come to make any gap in the loam-bed, that is between the first and second wall, the rest may by this means be preserved entire.

In the first wall there are stones in toothings from the top to the bottom, on the right and left of the aqueduct's mouth, and of the galleries window: and this is to make a cavity chamber-wise, 4 feet square. The wall of this little structure is to be 6 feet thick, counter-walled with another wall 2 feet thick, with a loam-bed between, to keep it safe. It shall end on the top above the gallery slopewise and like a vault, which is likewise to be counter-walled and counter-vaulted, with clay between. This walled square cavity is to be pierced by six or seven metal pipes, as large as those of cannons, and have their orifices from without; thereby to receive the water of the magazine to the height of the gallery. The cocks are to be within, shut up in the little space that the said cavity is to contain, to cast the water down so as it may have a fall of 4 fathoms. These cocks shall be opened through a window that is at the end of the gallery. And there is to be yet another little aperture beneath, at which one may descend into this chamber, in case the passage of the water shall be incumbered, or that any other inconvenience is to be remedied. For which purpose there shall be fastened eight bars of iron in the walls like a kind of stairs, for the conveniency of those that shall go down.

This gallery is only to serve for passing to open the cocks, according as there shall be need of water: and the water falling down will find issue through the aqueduct, following the bed of the rivulet Audot, falling into the deriving channel below the village Vaudreuil.

It was necessary to place these pipes so, that they might not take the water but at about four fathoms high from the ground, because it is not possible to make such a great collection of waters in this conservatory, especially of such waters as come in part from snows and rains, without store of dirt, sand, stones, pieces of wood from the neighbouring forests, and therefore there was an absolute necessity to leave some space for all this stuff, to prevent obstructing or spoiling the pipes. On which occasion it is to be added, that forasmuch as in length of time this dirt may be heaped up to such a height as to stop up the pipes; to avoid this inconvenience, there has been made in the said square cavity, an opening below, answering to the aqueduct; which opening is to be commonly shut by a large iron-gate, that can be drawn up or opened at pleasure from the gallery, that so the water finding this vent, and forcibly issuing, may carry with it all the dirt, or other stuff that shall be gathered at the bottom of this store-house.

As to the grand basin, to which the waters of the store-house are by the channel of St. Feriol to be conveyed, it may be added, that it is to be two fathoms deep all over; that it receives the waters of the said channel at one of its corners, and distributes them by two others, through two channels, into the ocean and Mediterranean: that there are to be two other channels, one to discharge the basin when there is too much water, the other is to come out of the deriving channel, to make the dirty waters run out, that so the pond receiving no other but clear and clean waters, may not be filled up with mud.

This basin is in a manner nearly finished. It is to have not only an elegant key, but a regular town built round about it after the model of the Place Royal of Paris; all the houses alike and equal, with great arches beneath, to go under cover to the key. There is also to be an arsenal, for boats to lie under, and for containing all necessaries to build and furnish them.

This royal channel is every where 5 fathoms broad at the bottom, and for the most part 9 fathoms and 2 feet broad above. It is ordinarily between 6 and 9 feet deep, and sometimes more. And as there is a considerable fall for about ten Languedoc leagues, from the point of distribution, where it is highest, unto its discharge into the Garonne, where it is very low, you descend and ascend by the means of 18 sluices, which at certain distances cut this channel, and bear up the waters that are above.

But as to the communication of this channel on the other side of the great basin towards the Mediterranean, that will require more time: yet the work is there begun, and the same order and rules will be observed on that side as on the other, as well for the opening of the channels as the structure of the gates and sluices.

Cape Cette, is by a cut of 800 fathoms, joined with the deep lake of Thau, which cut is almost finished. And when altogether perfected, it will prove a considerable harbour and place of safety for those that navigate in this part of the Mediterranean, called the Gulf of Lyons.

But the main advantages aimed at by this communication of the two seas are three: first, that by this means Languedoc will be enabled to vend their commodities of oils, wines, grain, wool, in which that large province abounds. Secondly, that whereas hitherto they have been obliged to carry all the merchandises of the Levant, with great charge and danger, all along the coast of Spain round about, through the Straits of Gibraltar; the same may henceforth be brought to Bourdeaux, and other ports of France lying on the ocean, by a much shorter, surer, and even, as it is thought, a cheaper way. Thirdly, that henceforth a man may travel round about France by water, save four days journey by land. For by taking boat at Guyse on the river of Oyse, lately made navigable above Fere, one may descend to its mouth, where it enters into the Seine; and then, passing St. Germain, St. Denis, Paris, Corbeil, Melun, go up as far as Montreau, and there take the river Yonne, and go as far as Auxerre, where you quit the river and go by land to Chalons, seated on the Soane; descending thence to Lyons, and so falling down to Tarascon; and having left the Rhone, take the Robine of Aigues-mortes, which is an ancient channel communicating from that river with the lakes of Languedoc; and so afterwards go up through the channels of Narbonne, Carcassone, and Castlenaudary to Tolouse; thence pass on the Garonne to Bourdeaux, and there embark, and coast it about that part of France which lies on the ocean, and so re-enter the Seine, and see Roan and the other towns lying on that river, until you come again to the mouth of the Oyse, whence you first parted.

Some Animadversions on the Theory of Light of Mr. Isaac Newton, Prof. of Mathematics in the University of Cambridge, printed in No. 80. *In a Letter of April* 9, 1672, *N.S. from Ignatius Gaston Pardies,* P. Prof. of Mathematics in the Parisian College of Clermont. Translated from the Latin. N°* 84, *p.* 4087.

I have read Mr. Newton's very ingenious hypothesis of light and colours.

* Ignatius Gaston Pardies, a French Jesuit, and professor of mathematics in the Parisian college of Clermont, was born in 1636. He entered the Jesuits order at 16, and after some time he devoted himself entirely to mathematics and natural philosophy. In this latter branch he followed the opinions of Descartes, though he feebly affected the contrary. He died at Paris in 1673, aged only 37, of a contagious disorder caught at the Bicêtre, where he officiated as a preacher and confessor. He

And as I have given some attention to that subject, and also made experiments, I shall here inform you of what has occurred to me on that new doctrine.

It seems very extraordinary that the learned author should make light to consist of an almost infinite number of rays, endued with a natural disposition of retaining and exhibiting their own proper colours, and that are disposed in a certain peculiar way to be refracted, some in a greater, and others in a less degree: that these rays which, while promiscuously blended together in open daylight, are undiscernible, and exhibit only the colour of whiteness, should notwithstanding in refraction have rays of one colour separated from those of all others, and, thus separated, appear in their proper and native colours: and that bodies should appear of a certain colour, red for instance, which are adapted to reflect or transmit rays of that colour only.

This extraordinary hypothesis, which, as he observes, overturns the very basis of dioptrics, and renders useless the practice hitherto known, is founded entirely on the experiment of the prism, in which rays entering into a dark room through a hole in the window-shutter, and then falling on the wall, or received on a paper, did not form a round figure, as he expected according to the received rules of refraction, but appeared extended into an oblong form: whence he concluded, that this oblong figure was owing to the different refrangibility of the rays of light.

But it appears to me that, according to the common and received laws of dioptrics, the figure ought to be, not round but oblong. For since the rays proceeding from the opposite parts of the sun's disk are variously inclined in their passage to the prism, they ought also to be variously refracted; that since the inclination of some rays is at least 30′ more than that of others, their refraction must also be greater. Therefore the opposite rays, emerging from the other surface of the prism, become more diverging, than if they had proceeded without any refraction, or at least with an equal one. Now that refraction of the rays is made only towards those parts, which may be supposed to be in the planes perpendicular to the axis of the prism; for there is no inequality of refraction towards those parts which are conceived to be in planes parallel to the axis, as may easily be demonstrated: for the two surfaces of the prism may be

was author of several ingenious works, which are written in a manner remarkably neat and clear; by which he acquired considerable credit, and by his talent as a teacher; but, unfortunately for him, lost himself by the above imprudent attack on Sir I. Newton's theory of light and colours. His works were chiefly, 1. Elements of Geometry, translated into English by Dr. John Harris, secretary of the Royal Society. 2. Discourse on the Knowledge of Beasts. 3. Statics, or the Science of Moving Forces. 4 Two machines for drawing dials. 5. Discourse on Local Motion. 6. Horologium Thaumanticum Duplex. 7. Dissertation on the Nature and Motion of Comets.

considered as parallel, with respect to the inclination to the axis, since they are both parallel to it. But the refraction through two parallel plane surfaces is accounted none, because by how much a ray is refracted one way by the first surface, by just so much is it refracted the contrary way by the other surface. Therefore since the solar rays, transmitted by a hole through a prism, are not refracted sideways, they proceed in that respect as if no prism at all stood in their way, that is with regard to the lateral divarication; but when the same rays on the superior and inferior parts are refracted, some more, some less, as being unequally inclined, they must needs diverge more, and consequently be extended in an oblong figure.

But when a calculation is rightly made, as the lateral rays were found by Mr. Newton, of a breadth that subtended an arc of 31′, which answers to the sun's diameter; so there is no doubt but the length of the image, which subtended 2° 49′, would correspond with the same diameter after the unequal refractions. Thus, supposing the prism at ABC, (fig. 7. pl. 15,) having the angle A of 60°; and a ray DE making with the perpendicular EH an angle of 30°; after emerging in the line FG, I find it makes with the perpendicular FI an angle of 76° 22′. But taking another ray dE, which makes with the perpendicular EH an angle of 29° 30′, I find that, when it emerges by fg, it makes with the perpendicular fi, an angle of 78° 45′. Hence those two rays DE, dE, which are supposed to proceed from opposite parts of the solar disk, and forming between them an angle of 30′, where they emerge by the lines FG, fg, they diverge so as to form between them an angle of 2° 23′. And if two other rays were assumed approaching nearer the perpendicular EH, as suppose one of them forming with it an angle of 29° 30′, and the other 29°; these rays, after emerging, would diverge still more, and form a greater angle, even sometimes more than 3°. And besides, this distance between the refracted rays is further increased, on this account, that the two rays DE, dE, meeting in E, begin immediately to diverge, and then fall on two distant points of the second surface, viz. in F and f. Therefore, in order to render the calculation just, it is not sufficient barely to subduct the diameter of the hole from the length of the image; for supposing the hole E to be invisible, or almost nothing, yet there would be formed a great hole as it were in Ff, in the second surface of the prism.

What the author calls the Experimentum Crucis, seems also to agree with the commonly received laws of refraction. For, as was just now shown, the sun's rays, which approaching and converging from an angle of 30′, coming from an invisible hole, do afterwards diverge in an angle of two or three degrees. It is not then to be wondered at, if these rays falling severally on a second prism, and having a very small hole in it, be unequally refracted, since their inclination is unequal. Nor does it alter the case, that those rays are raised or depressed by the rotation of the first prism, the second remaining immoveable, (which however cannot be done in all cases) or contrarywise the second being turned while the first is fixed, that it may successively receive the coloured rays of the whole image, and transmit them through its proper hole; for in either case it is necessary that the extreme rays, viz. the red and the violet, should fall on the second prism under unequal angles, and consequently that their refraction be unequal, that of the violet being the greater.

Since then here is an evident cause of that oblong figure of the rays, and that cause such as arises from the very nature of refraction; it seems needless to have recourse to another hypothesis, or to admit of that divers refrangibility of the rays.

The author's notion of colours indeed follows very well from the preceding hypothesis; yet it is not without its difficulties. For when he says, that all the rays being promiscuously blended together, yield no colour, but rather a whiteness, this does not seem conformable to all the phænomena. Doubtless the same variations that are seen in the mixture of divers bodies of different colours, are also observed in the mixture of different rays of various colours: and the author himself has well observed, that as a green colour arises from a yellow and a blue body, so likewise a green colour is produced from a yellow and a blue ray. Therefore, if all the rays of the several colours be blended together, it is necessary in that hypothesis, that that colour should appear, which in reality arises on mixing together the several sorts of painters colours. That is, as the red, yellow, blue, purple, and all the others, when mixed together, produce, not a white, but an obscure sated colour. So also ordinary light should appear of the same colour, being a like aggregate of all the colours.

Indeed nothing can be more ingenious and proper, than what he says about Mr. Hook's experiment, in which are two different liquors, the one red, the other blue, and each apart transparent, yet when mixed together they become opaque: this the ingenious author thus explains: that the one liquor is disposed to transmit only the red rays, the other only the yellow; hence, both being mixed together, they transmit none at all. But it should seem that the like opacity should take place on the mixture of liquors of any other different colours; which however is far enough from the truth.

Mr. NEWTON's Letter of April 13, 1672, O. S. written to the Editor, being an Answer to the foregoing Letter of F. PARDIES. Translated from the Latin. N° 34, p. 4091.

I received, Sir, the observations of the Rev. Father Ignatius Pardies, on my letter concerning the refractions and colours of light: for which I acknowledge myself much obliged to him; and shall here clear up the difficulties he complains of. In the first place, he says that the length of the solar image produced by the refraction of the prism, requires no other cause to account for it, than the different incidence of the rays from opposite parts of the sun's disk; and that therefore it does not prove a different refrangibility in the different rays. And, to prove the truth of his assertion, he states a case, in which from a difference of 30′ in the incidence, the difference of the refraction may be 2° 23′, or rather more, as my experiment requires. But the Rev. Father is under a mistake. For he has made the refractions by the different parts of the prism to be as unequal as possible, whereas in the experiments, and in the calculation from them, I employed equal refractions. Thus, let ABC (fig. 8, pl. 15,) be a section of the prism perpendicular to its axis; FL and KG two rays crossing each other in x, the middle of the hole, and incident on the prism at G and L; which let be first refracted into GH and Lm, and then into HI and mn. And since I supposed the refractions at the side AC are nearly equal to those at the side BC; if AC and BC be equal, the inclination of the rays GH and Lm, to the base AB of the prism, will be similar; and therefore the angle CLm = the angle CHG, and the angle CmL = the angle CGH. Therefore the refractions in G and m will be also equal, as well as those at L and H; consequently the angle KGA = the angle nmB, and the angle FLA = the angle BHI; and hence the inclination of the refracted rays HI and mn will be the same with that of the incident rays FL and KG. Therefore let the angle FxK of 30′ be equal to the sun's diameter, then the angle made by HI and mn will be also of 30′, provided the rays FL and KG be equally refrangible. But my experiment gave that angle about 2° 49′, which is constituted by the ray HI of the extreme violet colour, and by the ray mn which gives the blue; and therefore those rays were differently refrangible, or the refractions were necessarily produced according to the unequal ratio of the sines of incidence and refraction.

The Rev. Father further adds, that to make a just calculation, it is sufficient to subtract the magnitude of the window hole from the length of the image on the paper; since, even supposing the hole indivisible, yet there would be formed as it were a broad hole in the posterior surface of the prism. But yet it seems to me, that the refractions of rays crossing each other, both in the anterior and posterior surface of the prism, may be justly calculated from my principles. But if the case were otherwise, the breadth of the hole in the posterior surface, if such there be, would hardly produce an error of two seconds; and in practice such niceties may well be neglected.

What the Rev. Father contends is not inconsistent with what I called the Experimentum Crucis, viz. that the unequal refractions of rays endued with different colours, were produced by unequal incidences: for transmitting rays through two very small immoveable holes, and at a distance from each other, the incidences, as I made the experiment, were always equal, and yet the refractions were manifestly unequal. If he has any doubt of our experiment, I request that he may measure the refractions of the said rays of divers colours from equal incidences, and he will then see that they are unequal. But if he dislikes the manner in which I have performed this matter (than which however nothing can be clearer) it is easy to devise other ways; as indeed I myself have tried several other methods with advantage.

Against the theory of colours it is objected, that powders of divers colours mixed together do not yield a white, but an obscure and dusky colour. But to me, white, black, and all the intermediate dusky colours, which can be compounded of mixtures of white and black, do not differ as to their species, but only as to their quantity of light. And since in the mixture of painters colours, each corpuscle reflects only its own proper colour, and therefore the greatest part of the incident light is suppressed and retained; the reflected light will become obscure, and as if mixed with darkness, so that it exhibits not an intense whiteness, but an obscure dusky colour.

Again it is objected that an opacity ought equally to arise from a mixture of any liquors of different colours in the same vessel, as from the same liquors contained in different vessels; which however he says is not true. But I see no consequence in this. For many liquors act mutually on each other, and acquire a new texture of parts; hence they may become opaque, or diaphanous, or of various colours, in no manner owing to the colours of the compound. And on that account I have always esteemed experiments of this kind not so proper to draw conclusions from. It must also be noted that this experiment requires liquors of full and intense colours, which transmit very few rays besides those of their own colours; such as rarely occur, as will be seen by illuminating liquors with different prismatic colours in a dark room. For few will be found diaphanous enough in their own proper colours, and opaque in the others. Besides, it is proper that the colours employed be opposites, such as I account red and blue to be, or yellow and violet, or green and that purple which approaches to scarlet. And perhaps some of these liquors mixed together, whose tinging parts do not coalesce, will become more opaque. But I am not solicitous about the event, both as the experiment is clearer in liquors apart, and as the experiment (like the phænomena of the iris, and the tincture of lignum nephriticum, and of other natural bodies) I proposed not to prove but only to illustrate the doctrine.

I do not take it amiss that the Rev. Father calls my theory an hypothesis, inasmuch as he was not acquainted with it. But my design was quite different, for it seems to contain only certain properties of light, which now discovered I think easy to be proved, and which if I had not considered them as true, I would rather have them rejected as vain and empty speculation, than acknowledged even as an hypothesis.

Two Observations about Stones found, the one in the Bladder of a Dog, the other fastened to the Backbone of a Horse: both mentioned in two Roman Journals de Letterati. Nº 84, *p.* 4094.

The dog was a pretty spaniel, two palms and a half high, white, and an excellent setter for quails. Being kept tied, as such dogs are wont to be, he would rather have burst, than urine or dung in the place where he was kept. By reason of his aptness to bite, he was cut when he was five years old, and two years after that, he began to urine with much difficulty. And as often as he was loose, he ran immediately into the garden, and eat pellitory of the wall, and fig-leaves. This disease continued with him for five years together, sometimes with that violence that his master had him syringed, and anointed with oil of scorpions, and used other remedies to help the poor creature.

At length he died at twelve years of age, and being opened by a skilful anatomist, there was found in his bladder a stone weighing an ounce, of an irregular figure, white, yet here and there with some reddish specks; and in the bottom of the bladder was found much small white gravel, and in the mouth of the urinal passage a stone as large as a great pine-kernel, white and tender. The rest of the body was all swelled.

The other stone, that was fastened to the backbone of a Spanish gelding, that died at the age of between thirteen and fourteen years, weighed four ounces and a half; it was round and a little flatted; of an olive colour, marked with red specks, like coagulated blood; and so polished and shining, that it reflected images. It was wrapped up in a membrane full of fat, and fastened on both ends to the backbone, over against the kidneys.

An Account of some Books. N° 84, p. 4095.

I. An Essay about the Origin and Virtues of Gems; by R. Boyle, Esq. F.R.S. 1672, in 8vo.

The design of this essay is to prove, 1st. That most gems and medical stones were either once fluid bodies, or in part made up of such substances as were once fluid; and 2dly, That the real virtues of such stones are probably derived from the mixture of metallic or other mineral substances with which they are usually impregnated.

II. Johannis Swammerdami, M.D. Uteri Muliebris Fabrica, &c. Lugduni Batav. 1672, in 4to.

Besides a description of the organs of generation in women, and an inquiry into the mode of impregnation and conception, there are in this tract some observations on the force of the imagination in breeding women, and an account of a method of injecting the blood vessels with wax variously coloured.

III. Three letters of Jo. Dom. Cassini, concerning his Hypothesis of the Sun's motion, and his doctrine of Refractions; printed at Bononia, in 4to.

The first letter is to G. Montanari, professor of mathematics in Bononia. His former hypothesis was grounded on observations of the sun, from whose altitudes, when they were great, he made no abatement; because, according to the common opinion, the refraction is nothing, or at least inconsiderable. A specimen of it was published about 16 years ago. But afterwards he changed that hypothesis, that it might agree with his observations as diligently made, and more corrected. For having determined the height of the pole (and thereby of the equinoctial,) at Bononia; he observed also the sun's meridian height in both solstices. And subtracting that winter height from that equinoctial's height; and the said equinoctial's height from the summer height, he always found that former difference less, by above 4 minutes and a half, than the latter difference. Wherefore he attempted to order the parallaxes and refractions so, as that those summer and winter observations, being corrected according to that doctrine, might yield the sun's southern greatest declination, equal to the sun's greatest declination northward. In this letter he sets down what course he took to find the refractions; what experiments he made in glass and in water; how he applied them to celestial refractions; his proceeding to determine the proportion of the height of the air to the semidiameter of the earth; and at last to make tables to single degrees of apparent distance from the vertex.

The second letter is to Carlo Rinaldini, professor of mathematics in the university of Padua, dated August 7, 1666.—In it he shows some defect in the ways of making experiments of refraction, prescribed by Vitello, Descartes, Riccioli and Manzini. And then describes an instrument of his own invention for that purpose, &c.

The third letter is concerning a book of Dr. Mengoli; wherein is a table of refractions for every degree of altitude. But Cassini shows that table of Mengoli to be false; as being easily refuted by experience, and grounded upon a wrong foundation.

IV. Dr. Richard Sharrock's History of the Propagation and Improvement of Vegetables, &c. the 2d ed. Oxford, 1672, in 8vo.

This treatise shows the several ways for the propagation of plants usually cultivated in England, as they are increased by seed, offsets, suckers, truncheons, cuttings, slips, laying, circumposition: the several ways of graftings and inoculations; as likewise the methods for the improvement and culture of field, orchard and garden plants, &c.

A Series of Quæries proposed by Mr. Isaac Newton, to be determined by Experiments, positively and directly concluding his new Theory of Light and Colours, imparted to the Editor in a Letter of the said Mr. Newton's, of July 8, 1672. *N°* 85, *p.* 5004.

Give me leave, Sir, to insinuate that I cannot think it effectual for determining truth, to examine the several ways by which phænomena may be explained, unless where there can be a perfect enumeration of all those ways. You know that the proper method for inquiring after the properties of things, is to deduce them from experiments. And I told you that the theory which I proposed was evinced to me, not by inferring it is thus because not otherwise, that is, not by deducing it only from a confutation of contrary suppositions, but by deriving it from experiments concluding positively and directly. The way therefore to examine it is by considering, whether the experiments which I proposed do prove those parts of the theory to which they are applied; or by prosecuting other experiments which the theory may suggest for its examination. And this I would have done in a due method; the laws of refraction being thoroughly inquired into and determined before the nature of colours be taken into consideration. It may not be amiss to proceed according to the series of these quæries; which I could wish were determined by the event of proper experiments; declared by those that may have the curiosity to examine them.

1. Whether rays, that are alike incident on the same medium, have unequal refractions; and how great are the inequalities of their refractions at any incidence?

2. What is the law according to which each ray is more or less refracted; whether it be that the same ray is ever refracted according to the same ratio of the sines of incidence and refraction; and divers rays, according to divers ratios; or that the refraction of each ray is greater or less without any certain rule? That is, whether each ray have a certain degree of refrangibility according to which its refraction is performed; or is refracted without that regularity?

3. Whether rays which are endued with particular degrees of refrangibility, when they are by any means separated, have particular colours constantly belonging to them, viz. the least refrangible, scarlet; the most refrangible, deep violet; the middle, sea-green; and others, other colours? And on the contrary?

4. Whether the colour of any sort of rays apart may be changed by refraction?

5. Whether colours by coalescing do really change one another to produce a new colour, or produce it by mixing only?

6. Whether a due mixture of rays, indued with all variety of colours, produces light perfectly like that of the sun, and which has all the same properties, and exhibits the same phænomena?

7. Whether the component colours of each mixture be really changed; or be only separated, when from that mixture various colours are produced again by refraction?

8. Whether there be any other colours, produced by refraction, than such as ought to result from the colours belonging to the diversely refrangible rays, by their being separated or mixed by that refraction?

To determine by experiments these, and such like quæries, which involve the propounded theory, seems the most proper and direct way to conclusion. And therefore I could wish all objections were suspended, taken from hypothesis or any other heads than these two; of showing the insufficiency of experiments to determine these quæries, or prove any other parts of my theory, by assigning the flaws and defects in my conclusions drawn from them; or of producing other experiments which directly contradict me, if any such may seem to occur. For if the experiments which I urge be defective, it cannot be difficult to show the defects; but if valid, then by proving the theory they must render all objections invalid.

Some Annotations by Dr. Walter Needham, on a Discovery pretended to have been made by M. Pecquet, of a Communication between the Ductus Thoracicus and the Inferior Vena Cava. N° 85, p. 5007.

The relation itself of that pretended discovery, as it is to be found in the Journal des Sçavans of Feb. 8, 1672.

Great part of this paper consists of arguments to show the necessity of the chyle being conveyed into the blood by other channels than that by which the thoracic duct pours its contents into the subclavian vein. Among other reasons it is urged, that the orifice of that duct at its insertion into the aforesaid vein is not sufficiently large for allowing all the chyle to pass, which should be conveyed into the blood. To this Dr. Needham replies, that what is sufficient or not sufficient must be judged of by nature and not by us. Yet, adds he, if we consider the time that is spent in carrying the chyle up into the blood, it is easy to believe that a much greater quantity of liquor may be discharged by that duct than is usually pretended to. But passing over the imaginary difficulties raised by M. Pecquet, and the answers thereto, we shall proceed to the experiments by which the pretended new communication is supposed to be proved.

The observations made in the beginning of this year in his majesty's library, by searching carefully the passage of the ductus thoracicus in the body of a woman, have shown (says Mons. P.) that these difficulties are well grounded. For it has been found by divers experiments, made about this matter, that there ascends at least as much chyle through the trunk which is beneath the heart, as there descends through that which is above it.*

These experiments have appeared considerable, as they confirm those which were also made by the same Royal Academy of Sciences about five years since, and which were inserted in the 7th Journal des Sçavans 1667.† But this last experiment has been more clear and ample than the first, in that the communication which the first time appeared to be only with the left emulgent vein, has been found this second time not only with this vein but also with the two lumbar veins, which are inserted in the trunk of the inferior vena cava.

DR. NEEDHAM'S ANNOTATIONS.

(In the original these annotations are printed in columns by the side of the above relation.)

* What those experiments are we should be glad to know. But the experiment of 1667 (if I rightly remember it) was only a lusus naturæ, found by M. Pecquet; which I therefore call so, because neither he nor any one else has found it since; whereas the vasa lactea, and the ways of ordering them are so well known, that, if any such thing were, it could not long be hid.

† See No. 25, p. 163, of this vol. of the Abridgement.

As to the manner of proceeding, in the presence of the whole company, for finding this communication, it was this: after there had been showed the commerce of the ductus thoracicus with the right ventricle of the heart by an injection of milk, which having been syringed into the beginning of this channel, issued in great quantity through this ventricle; we tied the trunk of the vena cava above the heart, so that nothing might pass that way; and the trunk of the emulgent and that of the vena cava having been opened above longwise, some milk, ready to boil, was* injected into the emulgent through the left lumbar vein (which we have ever observed to come from the emulgent) and at the same time we saw it come away through the other lumbar.

† This way was, to syringe into the trunk of the ductus thoracicus a composition, that might run into it being hot, and which by being refrigerated might become solid enough to afford a greater facility to follow and trace the channels, in the cavity of which it should be thus hardened. And this design succeeded in part. For the composition filled the whole ductus thoracicus, and ascended as far as into the subclavian; but there passed nothing into the channel that makes the communication sought for, though care was had to warm the ambient parts by several injections of warm milk, to the end that the composition might not harden before it had penetrated into all the conduits. We also tried to inject the same composition through the lumbar that issues out of the trunk, if its valves would permit it; but they stopped all that we endeavoured to make pass that way, and neither the milk nor the wind would ever enter there.

‡ The advantage we had from the injection of this composition into the said

DR. NEEDHAM'S ANNOTATIONS.

* An injection into the lumbar vein with its effects mentioned, can prove nothing but the inosculation of the two lumbar veins with each other, which is acknowledged to be such in all the capillary vessels of the same kind, viz. veins with veins, and arteries with arteries. But the thing required here is, the passage from the receptaculum to the lumbar vein, or to any other vein besides the subclavian.

† The way of syringing a liquor, which is apt to coagulation, into the ductus thoracicus, &c. I think to be needless and unprofitable as to this inquiry, when there is a more easy experiment to be made, which is more demonstrative; viz. open a dog at a convenient distance of time from his feeding, and then tie a ligature on the ductus thoracicus nigh the subclavian; your receptaculum chyli will continue full, 48 hours or longer if you please; so that if there be any such ductus, it must remain likewise full with its own natural liquor, and be all that while visible. But, if there were any such ductus, it would in a quarter of the time empty the whole receptacle; whereas upon a ligature you will find the clean contrary, viz. all the lacteal vessels (that are acknowledged to be such) fully distended; which is a full demonstration that they have no way of evacuation by any other duct than the thoracic.

‡ The other use of the coagulating injection I applaud, though the same may be done by the ligature abovesaid. However the event of the experiment, made by the learned Pecquet, makes against the opinion of a new ductus, and not for it, as appears by the narrative. [It is a fact well established

ductus was, that we very distinctly saw the figure and the whole structure of it when the composition wherewith it had been filled was refrigerated and hardened. For we found, that that ductus did ascend unto the right side of the heart, keeping one and the same size, which was no more than $\frac{1}{12}$ of an inch; that afterwards it was enlarged to $\frac{1}{6}$ of an inch in diameter; that in this enlargement its tunicle on the right side of the vertebræ was, as it were, pierced by four small holes, distant $\frac{1}{12}$ of an inch from one another, and all disposed in a row; into which holes the said composition had not been able to penetrate; that the same ductus, after having retaken its first size, had two appendixes fashioned like sacks; that there was yet a third appendix beneath the dilatation; that the first and highest appendix was of the form and bigness of a small phaseolus; that the third, which was beneath the dilatation, was like to the second; that they had all a straight orifice; and that the last was full of chyle conspissate, so that the composition could not enter there, as it had done into the other.

A second Letter of P. PARDIES, written to the Editor from Paris, May 21, 1672, to Mr. NEWTON's Answer made to his first Letter, printed in N° 84. N° 85, p. 5012. Translated from the Latin.

I have received your letter, with the observations of the very ingenious Mr. Newton, in which he answers my difficulties, which I have read with great pleasure. And first, with respect to that experiment of the greater breadth of the colours than what is required by the common theory of refractions; I confess that I supposed the refractions at the opposite sides of the prism unequal, till informed by the letter in the Transactions, that the greater breadth was observed by Newton in that case in which the refractions are supposed reciprocally equal, in the manner mentioned in those observations. But since I now see that it was in that case that the greater breadth of the colours was observed, on that head I find no further difficulty. I say on that head; for the greater length of the image may be otherwise accounted for, than by the different refrangibility of the rays. For according to that hypothesis, which is explained at large by Grimaldi, and in which it is supposed that light is a certain substance very rapidly moved, there may take place some diffusion of the rays of light after their passage and decussation in the hole. Also on that other hypothesis, in which light is made to proceed by certain undulations of a subtile matter, as explained by Mr. Hook, colours may be explained by a certain diffusion and expansion of the undulations, made on the sides of the rays beyond the hole by

that there is no other channel by which the chyle is conveyed into the blood than that of the thoracic duct, which generally opens into the left subclavian vein at the angle formed by it and the internal jugular vein. Sometimes however it is inserted directly into the internal jugular.]

the influence and continuation of the subtile matter. Indeed I admit such an hypothesis in "the Dissertation on the Motion of Undulation," which is the sixth part of my mechanics, as I suppose that those apparent colours are the sole effect of that communication of motion which is diffused laterally by the direct undulations. As if the rays entering by the hole *a*, (fig. 9, pl. 15) should proceed towards *b*, the undulations ought indeed to terminate directly, with regard to their direct and natural motion, at the right line *ab*; yet nevertheless, because of the continuity of the matter, there is some communication of the motion towards the sides *cc*, where it becomes tremulous and undulatory. And if colours be supposed to consist in the lateral undulation, all their phænomena may be explained in this manner, as I have shown in the dissertation before mentioned; by which also the reason will appear, why the breadth of the colours must be expanded beyond the divergency of the rays themselves.

As to what he says of the error, which might arise in the calculation, from what I mentioned like a hole made in the posterior face of the prism, that that error could not cause any sensible variation; his remark is very proper; neither have I judged that hence the breadth of the colours would be much increased, but I wished only to indicate an accurate mode of calculation: and therefore I also think that this caution may be neglected in practice.

As to the Experimentum Crucis, I make no doubt that the incident rays had an equal inclination, since the author expressly affirms it. But that is what I could not gather from what I read in the Transactions; where it is stated, that there are two small and very distant holes, and one prism near the first hole in the window; through which prism the coloured rays escaping, fall on the other distant hole. And it is added, that the first prism was turned round its axis, to cause all the rays to fall successively on the second hole. Now in this case the inclination of the rays which fall on the second hole, must necessarily be changed: and I hinted in my letter, that it would be the same thing, whether the second hole were raised or depressed, for all the rays pointing to the sun's image, to fall successively on it, while the first prism was invariable; or whether, the second hole being immoveable, the first prism were turned round, so that the same image might change its situation, and all its parts successively fall on the second hole. But no doubt the sagacious Newton used other precautions.

As to what I objected about colours, I am well satisfied with the solutions. And as to my calling the author's theory an hypothesis, that was done without any design, having only used that word as first occurring to me; and therefore request it may not be thought as done out of any disrespect. I have always esteemed ingenious discoveries, and the excellent Newton I very highly admire and honour.

Mr. NEWTON'S *Answer to the foregoing Letter.* N° 85, *p.* 5014.
Translated from the Latin.

In the observations of the Rev. F. Pardies, one can hardly determine whether there is more of humanity and candour, in allowing my arguments their due weight, or of penetration and genius in starting objections. And doubtless these are very proper qualifications in researches after truth. But to proceed, F. Pardies says, that the length of the coloured image can be explained, without having recourse to the divers refrangibility of the rays of light; as suppose by the hypothesis of F. Grimaldi, viz. by a diffusion of light, which is supposed to be a certain substance put into very rapid motion; or by Mr. Hook's hypothesis, by a diffusion and expansion of undulations; which, being formed in the æther by lucid bodies, is propagated every way. To which may be added the hypothesis of Descartes, in which a similar diffusion of *conatus*, or pression of the globules, may be conceived, like as is supposed in accounting for the tails of comets. And the same diffusion or expansion may be devised according to any other hypothesis, in which light is supposed to be a power, action, quality, or certain substance emitted every way from luminous bodies.

In answer to this, it is to be observed that the doctrine which I explained concerning refraction and colours, consists only in certain properties of light, without regarding any hypothesis, by which those properties might be explained. For the best and safest method of philosophizing seems to be, first to inquire diligently into the properties of things, and establishing those properties by experiments; and then to proceed more slowly to hypotheses for the explanation of them. For hypotheses should be subservient only in explaining the properties of things, but not assumed in determining them; unless so far as they may furnish experiments. For if the possibility of hypotheses is to be the test of the truth and reality of things, I see not how certainty can be obtained in any science; since numerous hypotheses may be devised, which shall seem to overcome new difficulties. Hence it has been here thought necessary to lay aside all hypotheses, as foreign to the purpose, that the force of the objection should be abstractedly considered, and receive a more full and general answer.

By light therefore I understand, any being or power of a being, (whether a substance, or any power, action, or quality of it,) which, proceeding directly from a lucid body, is apt to excite vision. And by the rays of light I understand its least or indefinitely small parts, which are independent of each other; such as are all those rays which lucid bodies emit in right lines, either successively or all together. For the collateral as well as the successive parts of light are independent; since some of the parts may be intercepted without the others, and be separately reflected or refracted towards different sides. This being premised, the whole force of the objection will lie in this, that colours may be lengthened out by some certain diffusion of light beyond the hole, which does not arise from the unequal refraction of the different rays, or of the independent parts of light. And that the image is no otherwise lengthened, was shown in my letter in Numb. 80, of the Transactions; and to confirm the whole in the strictest manner, I added that experiment now known by the name Experimentum Crucis; of the conditions of which since the Rev. Father has some doubt, I have thought fit to represent it by a scheme. Let BC (fig. 10, pl. 15) then be the anterior board, to which the prism A is immediately prefixed, and let DE be the other board, at the distance of about 12 feet from the former, to which the other prism F is affixed. And let the boards be perforated at x and y in such a manner, that a little of the light refracted by the former prism may pass through both the holes to the second prism, and be there refracted again. Now let the former prism be turned about its axis with a reciprocal motion; then the colours falling on the latter board DE will be raised and depressed by turns: and thus the several colours may at pleasure be made to pass successively through the hole y to the latter prism, while all the other colours fall on the board. Then you will see that the said rays of different colours will be differently refracted at the latter prism, as they will be seen on different places of the opposite wall, or of any obstacle GH, at the distance of some feet from it; as suppose the violet rays at H, the red at G, and the intermediate rays at the intermediate places: and yet, because of the determinate position of the holes, the incidence of the rays of each colour through both must be similar. And thus it appears, by measuring, that the rays of different colours have different laws of refractions.

But I suspect what it was that caused the Rev. Father to doubt; viz. it seems he placed his first prism A behind the board B C, and thus by turning it about its axis, it is probable that the inclination of the rays intercepted between the two holes may have suffered some change by the intermediate refraction. But by the description before given in the Transactions, the first board ought to be placed after the prism, that the rays may pass in a straight direction between the holes, agreeably to my words; "I took two boards, and placed one of them close behind the prism at the window." And the design of the experiment requires the same thing.

It may be further observed, that in this experiment, because of the refraction of the second prism, the coloured light is much less diffused and less divergent, than when it is quite white, so that the image at G or H is nearly circular; especially if the prisms be placed parallel, and their angles in a contrary position, as in the present figure. And besides, if the diameter of the hole *y* be equal to the breadth of the colours, the coloured light will not be diffused lengthwise; but the image, which is formed by any colour at G or H, will be manifestly circular; supposing the holes to be circular, and the refraction of the latter prism not to be greater than that of the former, and the rays to be nearly perpendicular to the obstacle. This shows that the diffusion, above-mentioned, does not arise from the influence or continuity of the undulating matter, or matter put into a rapid motion, or any such like causes, but from a certain law of refractions for every species of rays. But why the image is in one case circular, and in others a little oblong, and how the diffusion of light lengthwise may in any case be diminished at pleasure, I leave to be determined by geometricians, and compared with experiments.

After the properties of light shall, by these and such like experiments, have been sufficiently explored, by considering its rays either as collateral or successive parts of it, of which we have found by their independence that they are distinct from one another; hypotheses are thence to be judged of, and those to be rejected which cannot be reconciled with the phænomena. But it is an easy matter to accommodate hypotheses to this doctrine. For if any one wish to defend the Cartesian hypothesis, he need only say that the globules are unequal, or that the pressures of some of the globules are stronger than others, and that hence they become differently refrangible, and proper to excite the sensation of different colours. And thus also according to Hook's hypothesis, it may be said, that some undulations of the æther are larger or denser than others. And so of the rest. For this seems to be the most necessary law and condition of hypotheses, in which natural bodies are supposed to consist of a multitude of corpuscles cohering together, and that from the different particles of lucid bodies, or from the different parts of the same corpuscle, (as they may happen to differ in motion, figure, bulk, or other qualities) unequal pressions, motions, or moved corpuscles, may be propagated every way through the æther, of the confused mixture of which light may be supposed to be constituted. And there can be nothing more difficult in these hypotheses than the contrary supposition.

As to that aperture or dilatation of the light in the posterior face of the prism, which the Rev. Father supposes to resemble a hole, it is sufficient that no sensible error can arise from it, if any at all. For if a calculation be made precisely according to the observations, the error will be found nothing. For by subtracting the diameter of the hole from the length of the image, there will remain that length which the image would have, if the hole before the prism were an indivisible point, and that notwithstanding the aforesaid dilatation of the light in the posterior face of the prism; as is easily shown. Then from that given length of the image, and its distance from the indivisible hole, as also from the position and form of the prism, and besides from the inclination of the incident rays, and from the angle which the refracted rays bending to the middle of the image make with those that are incident from the sun's centre, all other things may be determined. And the same data that determined the refractions and positions of the rays, are sufficient for an accurate calculation of these refractions. But this matter seems not to be of importance enough to be much regarded.

As to the Rev. Father's calling our doctrine an hypothesis, I believe it only proceeded from his using the word which first occurred to him, as a practice has arisen of calling by the name hypothesis whatever is explained in philosophy; and the reason of my making exception to the word, was to prevent the prevalence of a term, which might be prejudicial to true philosophy.

The above answer being sent to the Rev. Father Ig. Pardies, he returned his acknowledgment in a note as below.

I am quite satisfied with Mr. Newton's new answer to me. The last scruple which I had, about the Experimentum Crucis, is fully removed. And I now clearly perceive by his figure what I did not before understand. When the experiment was performed after his manner, every thing succeeded, and I have nothing further to desire.

An Account of some Books. *N° 85, p.* 5019.

I. Scarborough Spa Spagyrically Anatomized, An. 1670; And a New Year's Gift for Dr. Witty; London, 1671; both in 12mo. by George Tonstal, M.D.

To particularise the contents of these tracts, would be an abuse of the reader's time and attention.

II. New England's Rarities discovered; together with the remedies used by the Natives to cure their diseases, wounds, and sores, &c. by John Josselin, Gent. London, 1672, in 12mo.

Later and more accurate accounts of the natural productions of North America, and of the diseases of that climate, render it unnecessary to notice the contents of this book.

III. A Rational way of preparing Animals, Vegetables, and Minerals, for a Physical use: by Edw. Bolnest, Med. Reg. Ord. London, 1672, in 12mo.

A book of no use to the modern student, or practitioner of Physic.

IV. Miscellanea Curiosa Physico-Medica Academiæ Naturæ Curiosorum: annus secundus. Jenæ 1671, in 4to.

A continuation of the German ephemerides noticed in Numb. 68. To show the nature of the work, we there gave a catalogue of the contents of that 1st volume, but deem it unnecessary to enter into similar details concerning this and the subsequent volumes.

END OF VOLUME FIRST.

ERRATA.

Page 267, l. 20, for *Polyscopes* read *Polemoscopes.*
270, l. 10, for *Speculators* read *Spectators.*
297, l. 19, for *Bladder* read *Ureter.*
338, l. 21, for *Brounker* read *Brunker.*
From p. 226 to p. 304, *Vol. II.* is printed instead of *Vol. III.* in the running title.

Fig. 1.

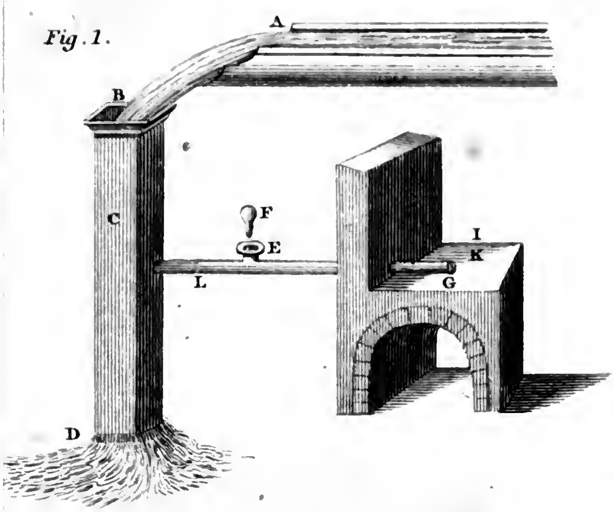


Fig. 2.

Fig. 4.

Fig. 5.

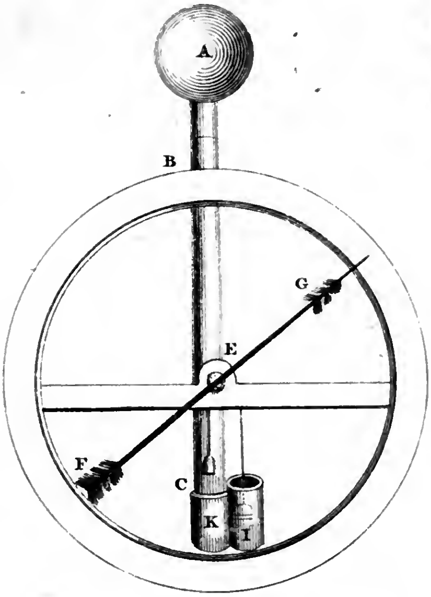


Fig. 3.

Fig. 6.

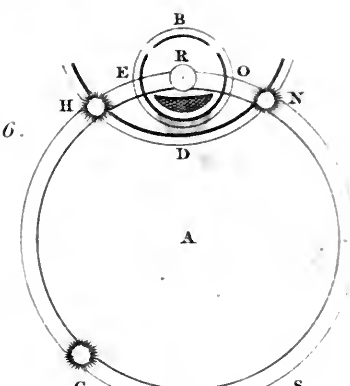


Fig. 8.

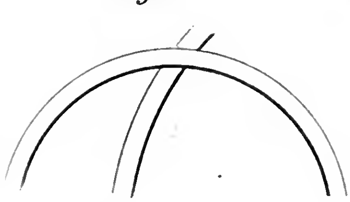
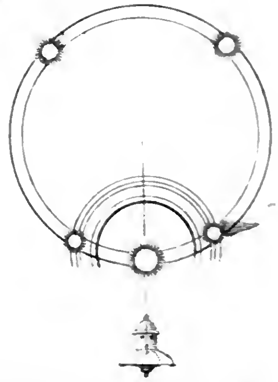


Fig. 7.



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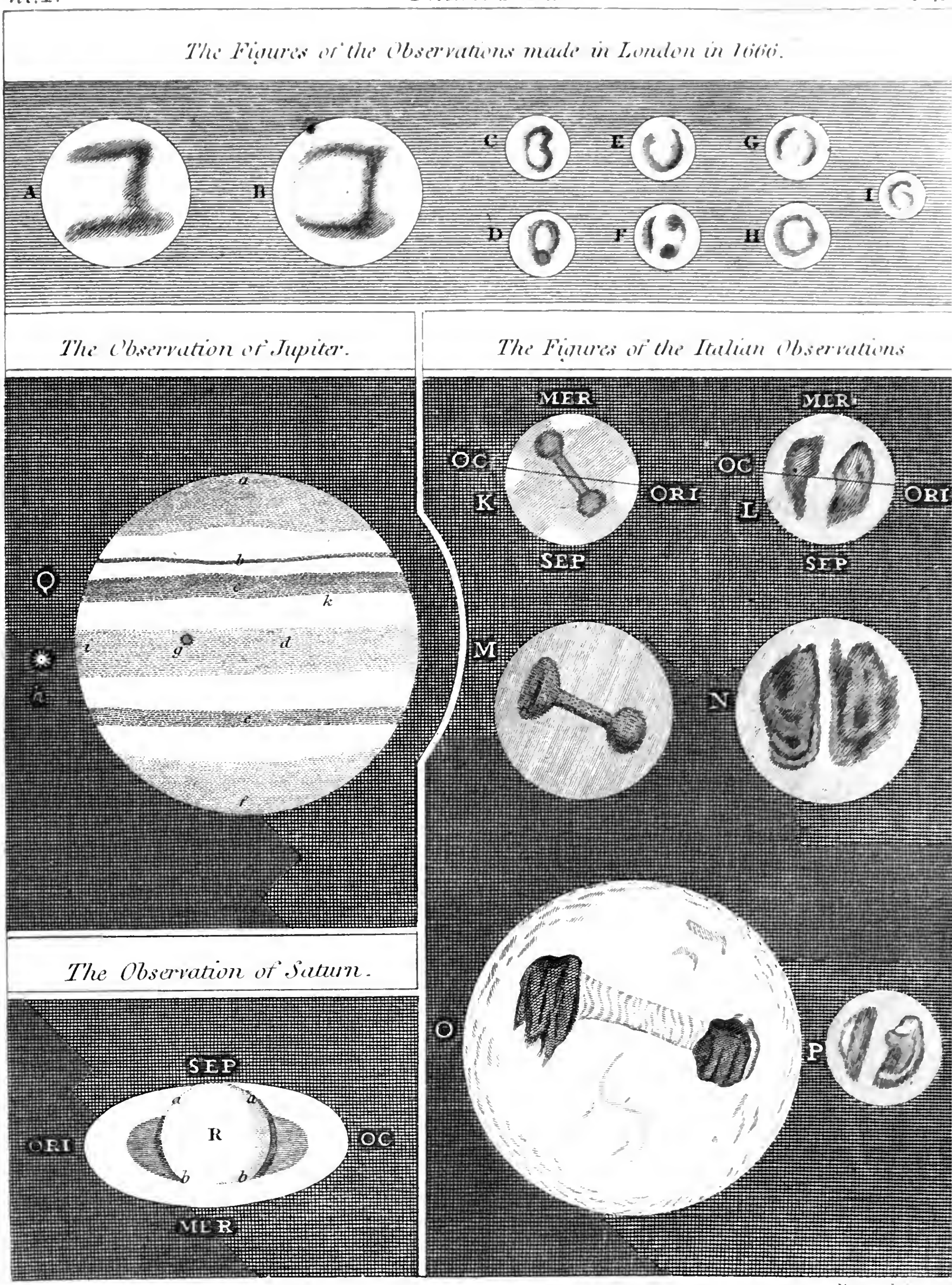

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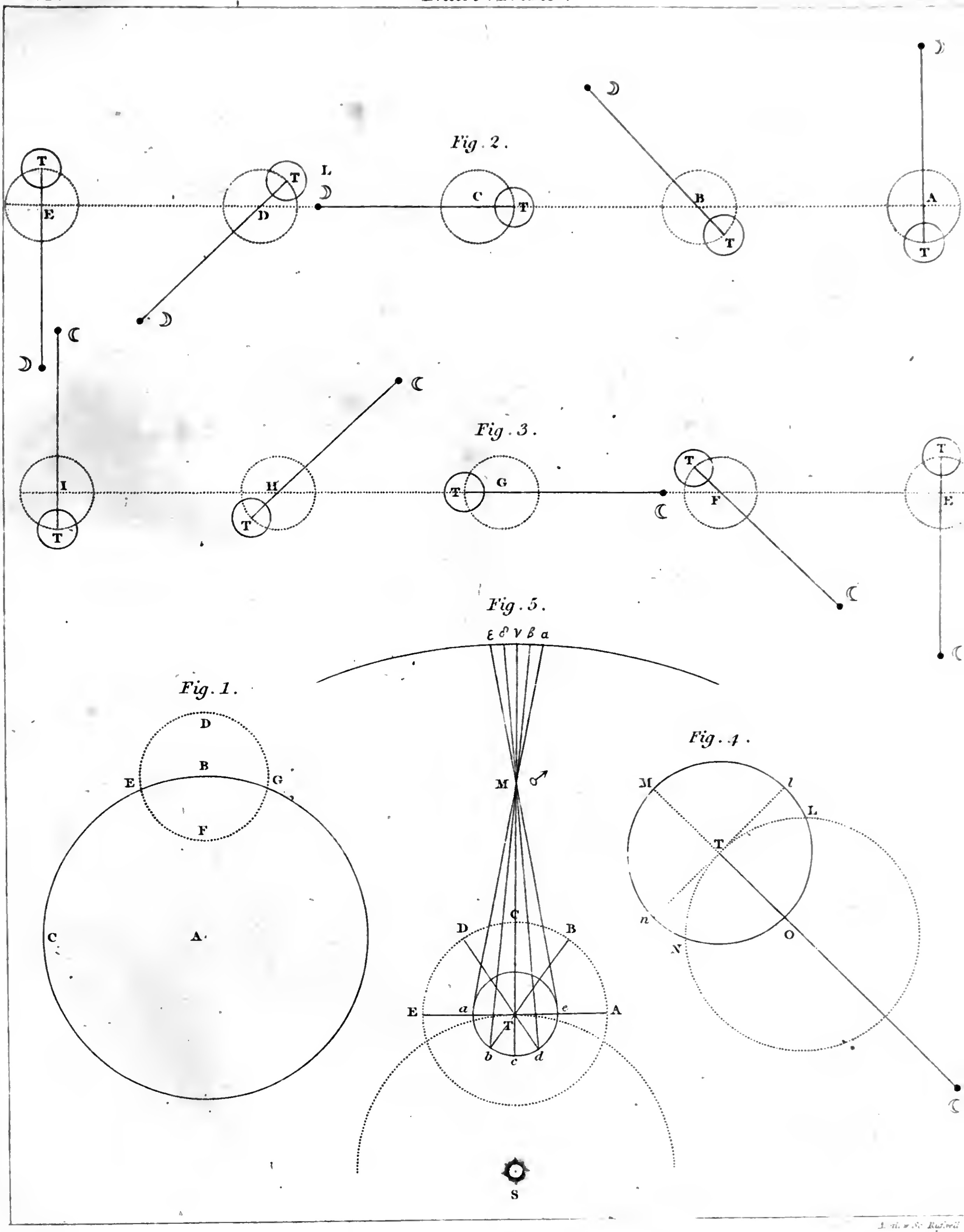

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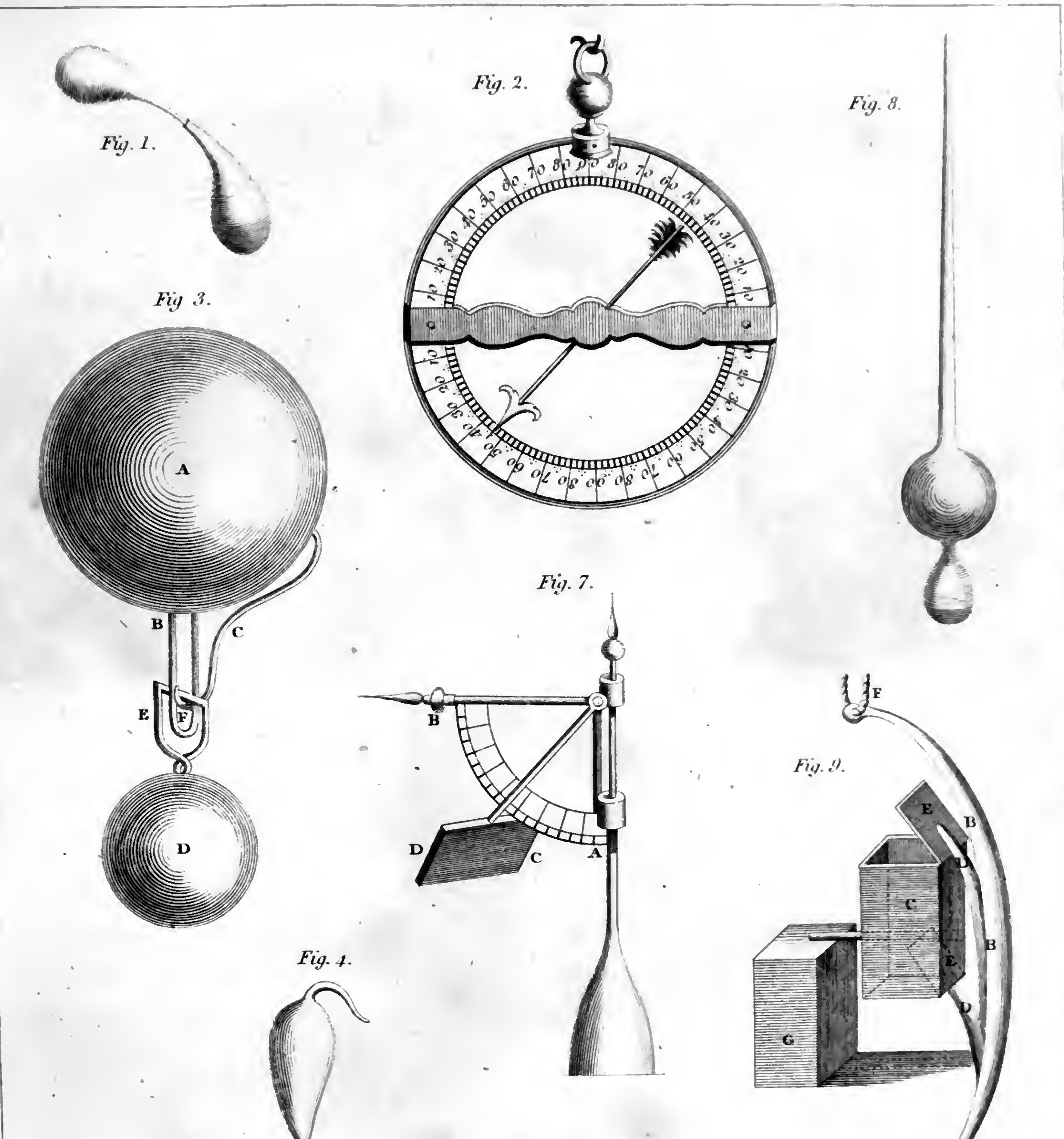

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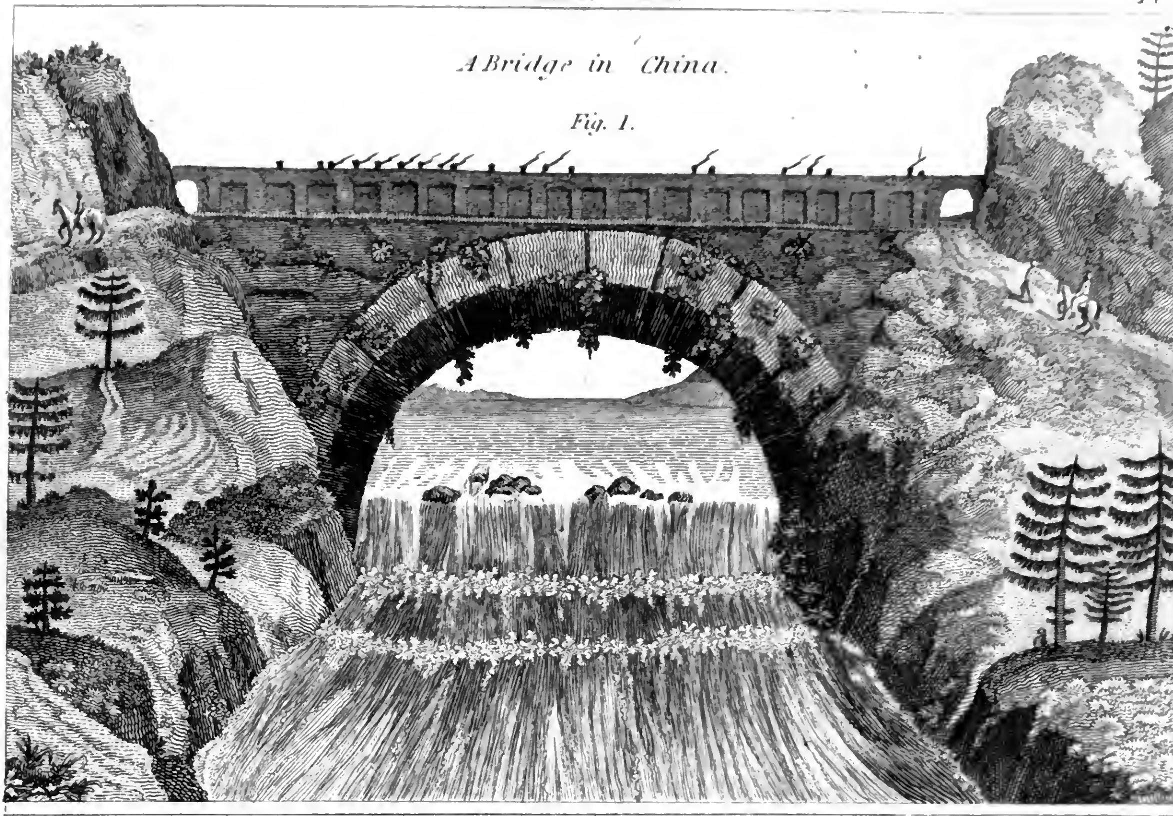

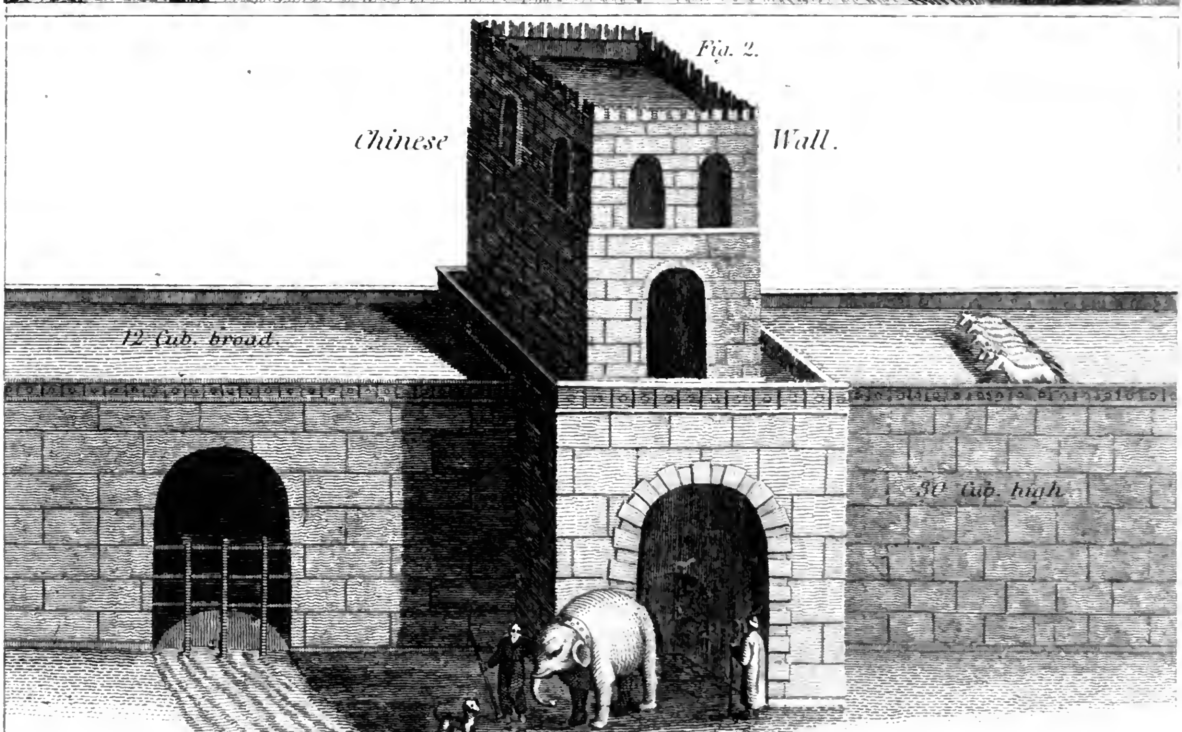

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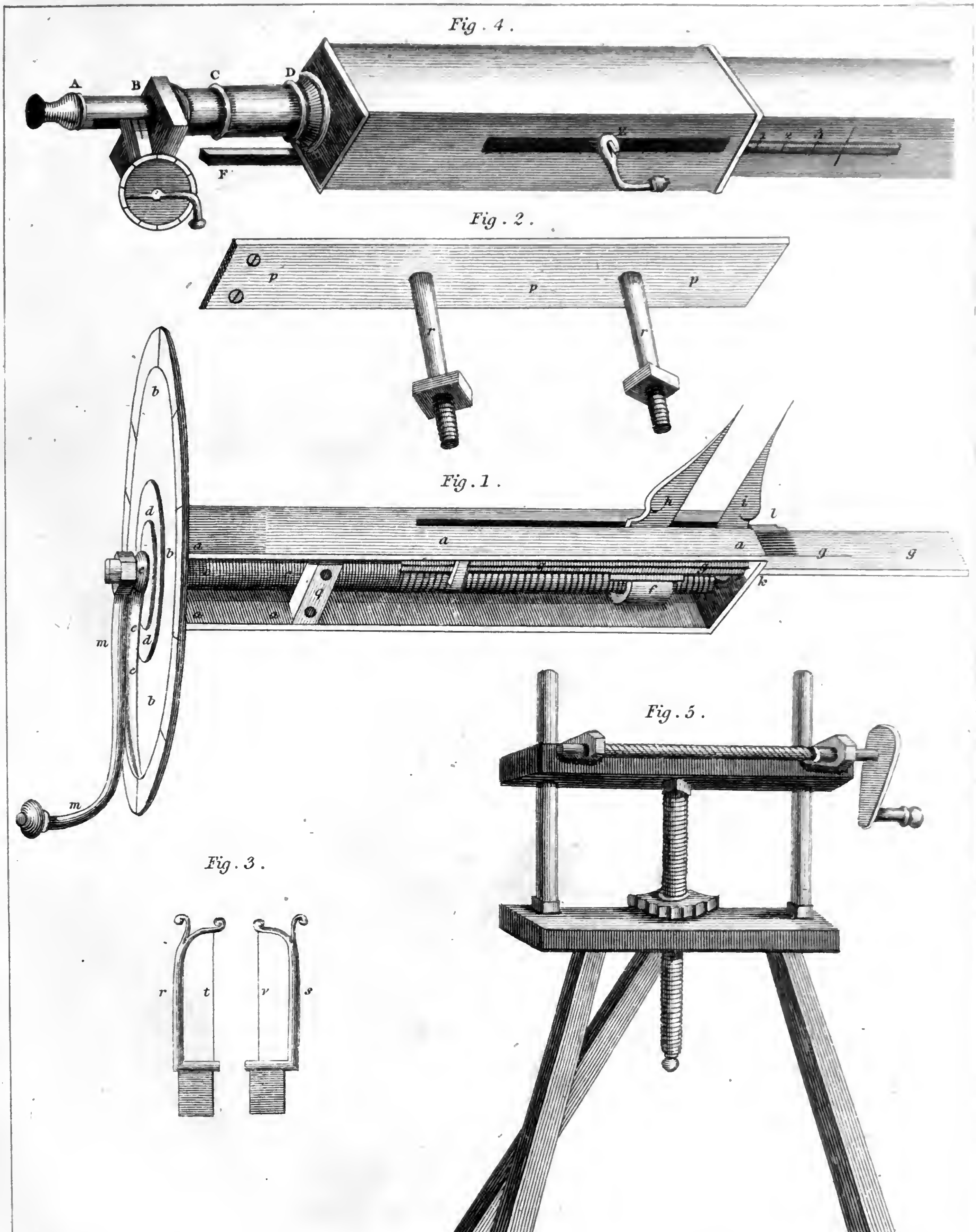

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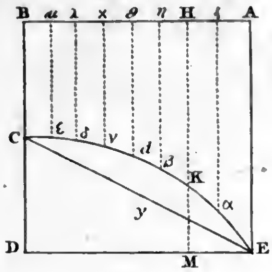
Fig. 1.



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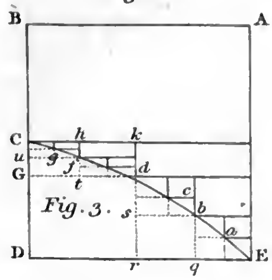

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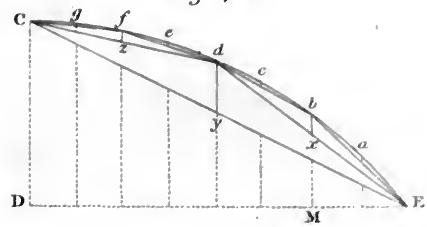

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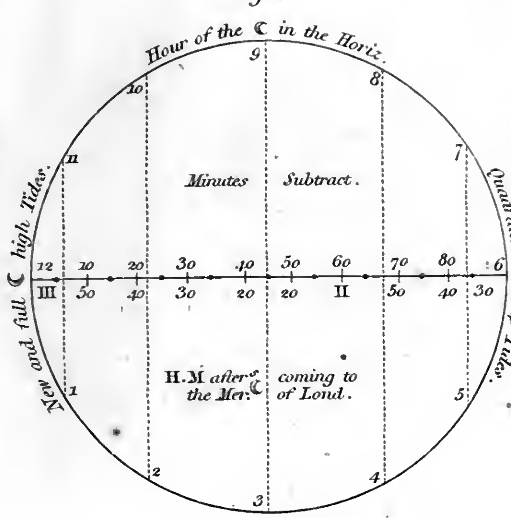

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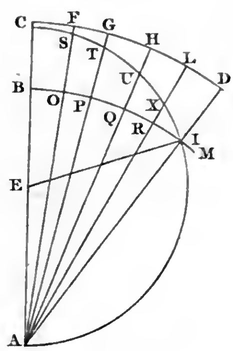

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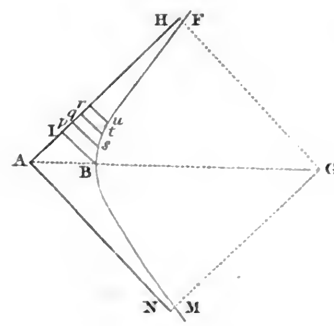

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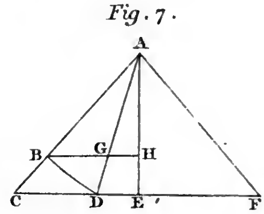


Fig. 12.

Equal Bodies.

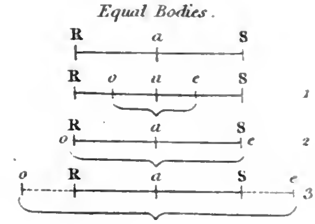


Fig. 9.

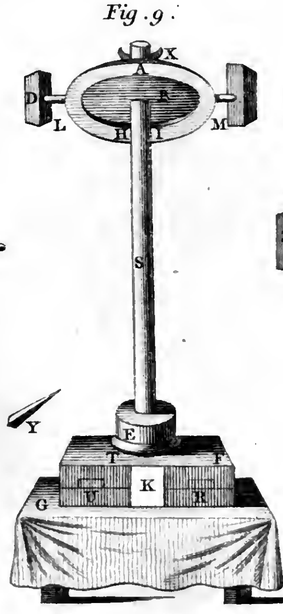


Fig. 10.

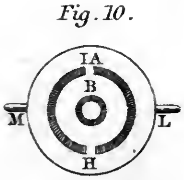


Fig. 11.

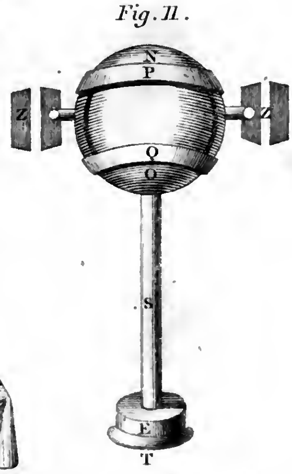
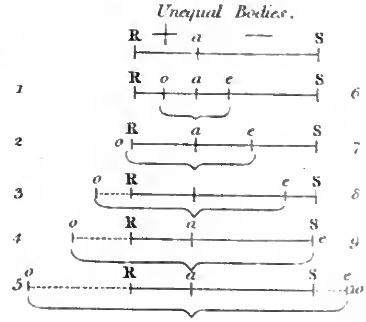


Fig. 13.

Unequal Bodies.



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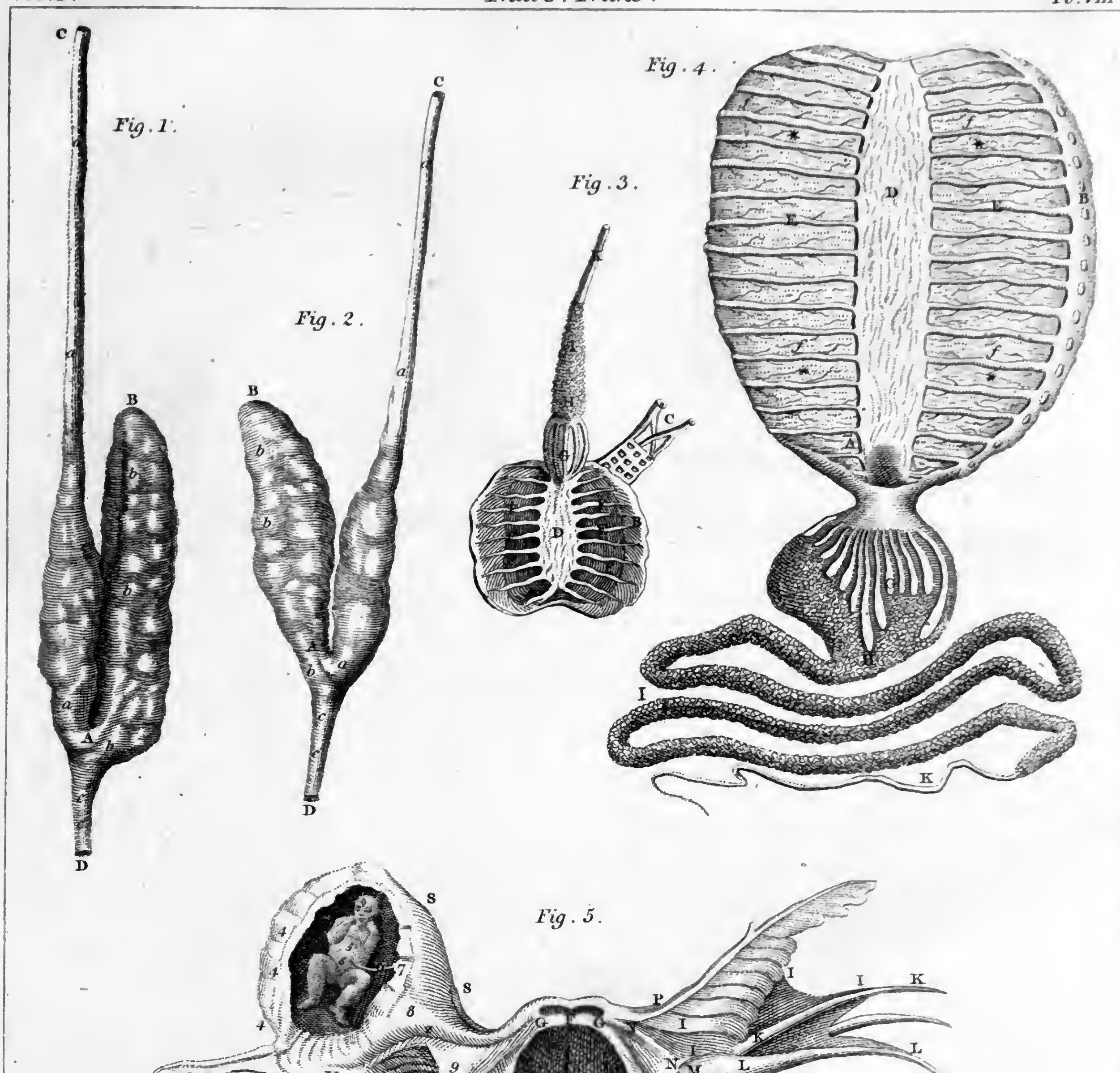

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Fig. 1.

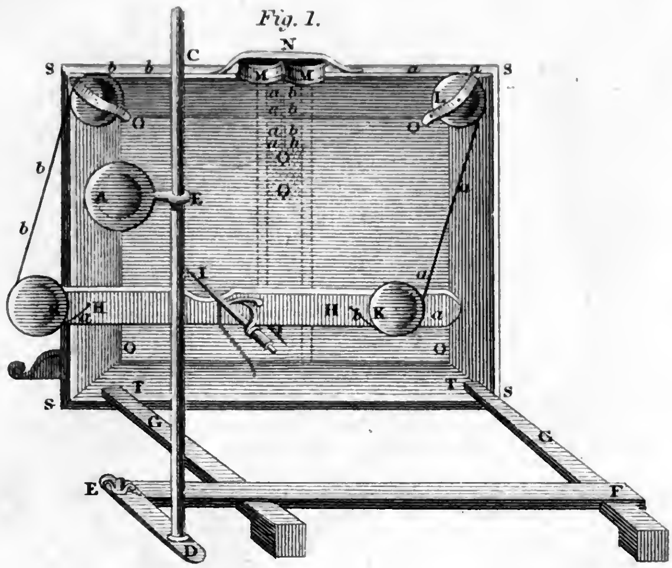


Fig. 2.

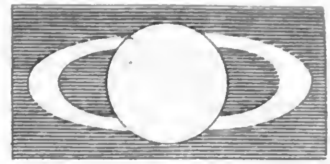
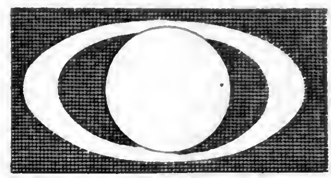


Fig. 3.



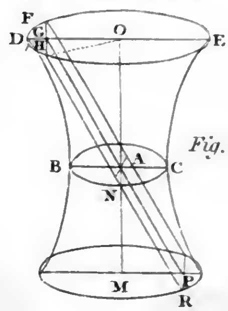
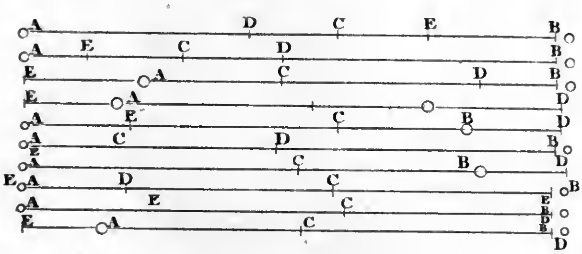


Fig. 5.

Fig. 4.



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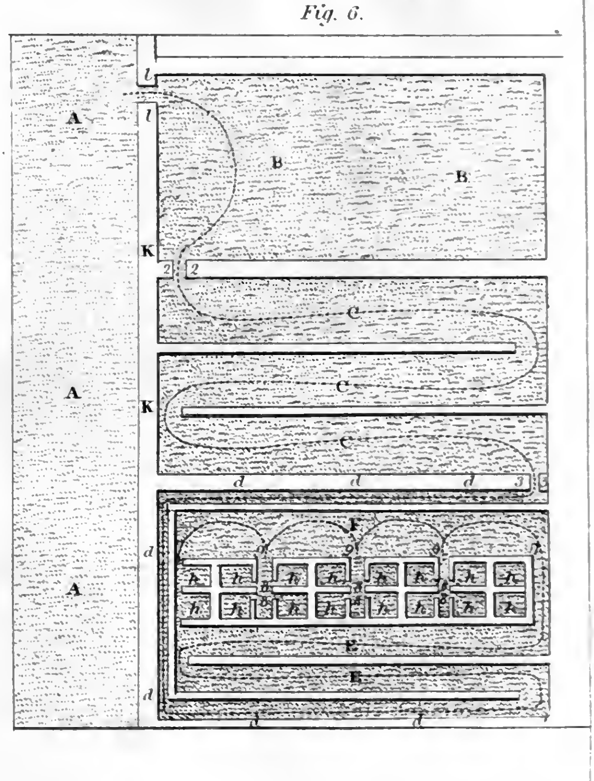

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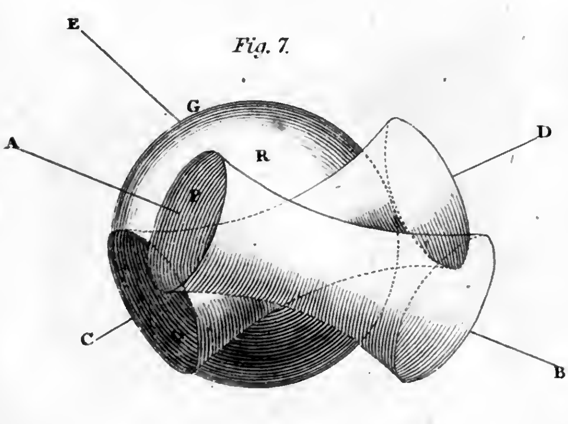

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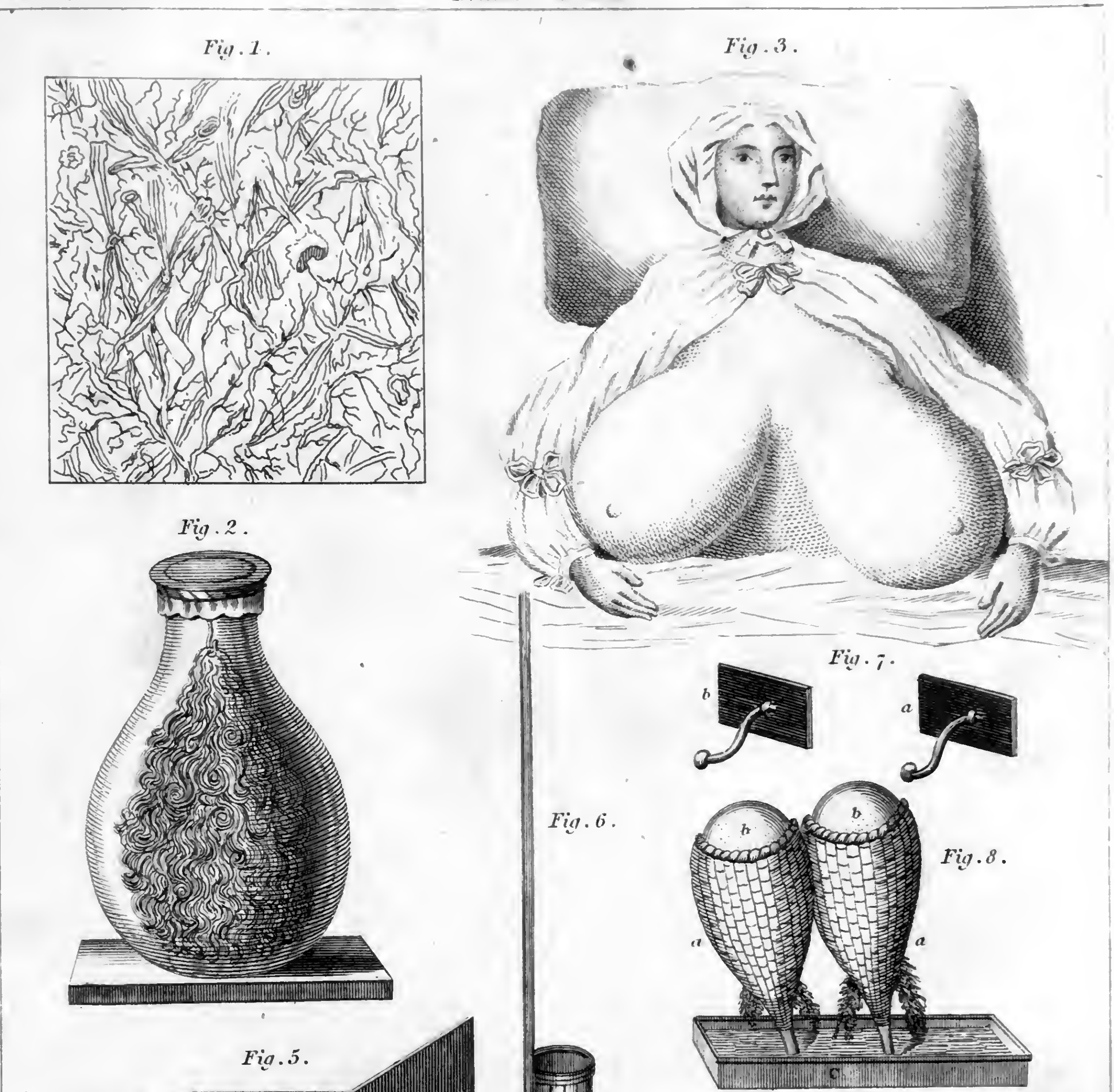

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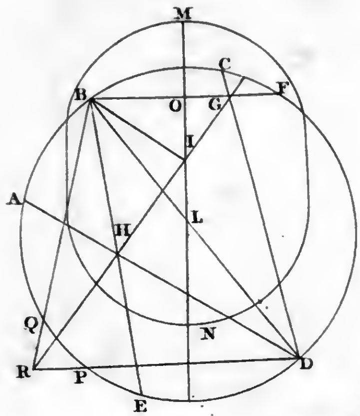
Fig. 2.



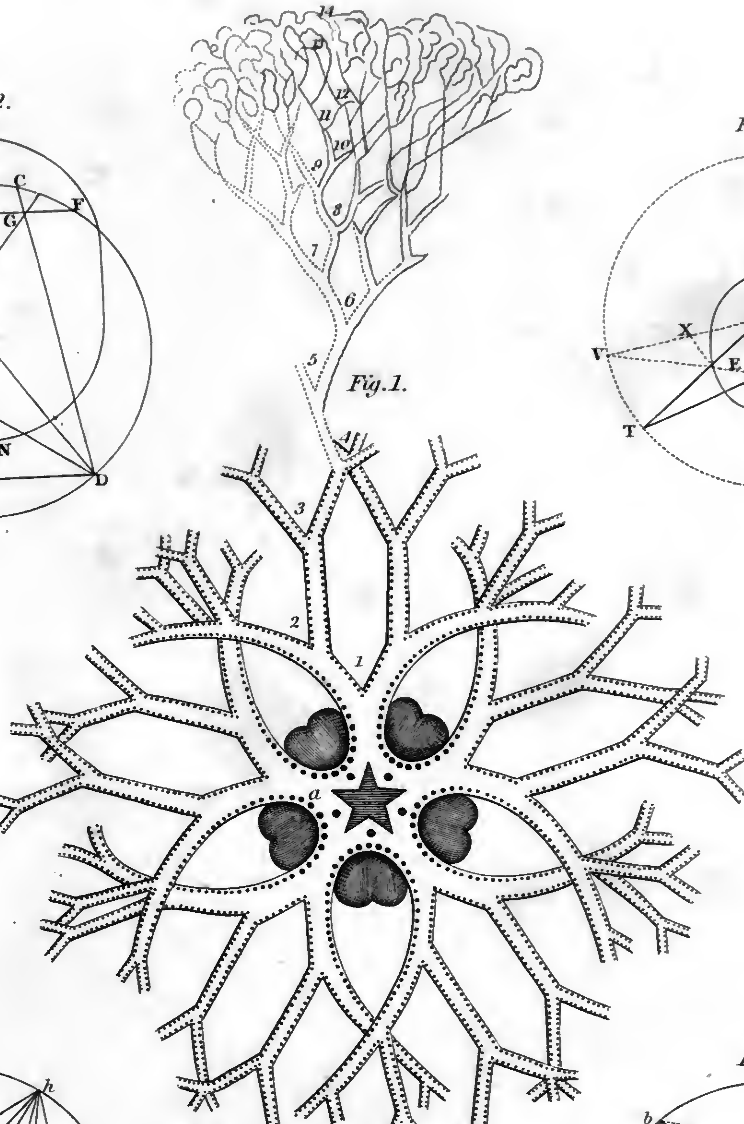
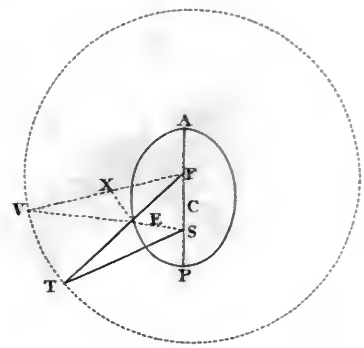


Fig. 1.

Fig. 3.



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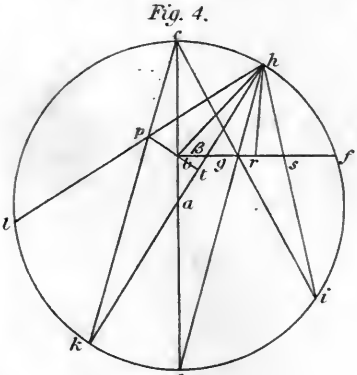

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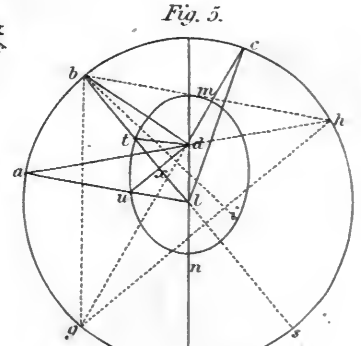

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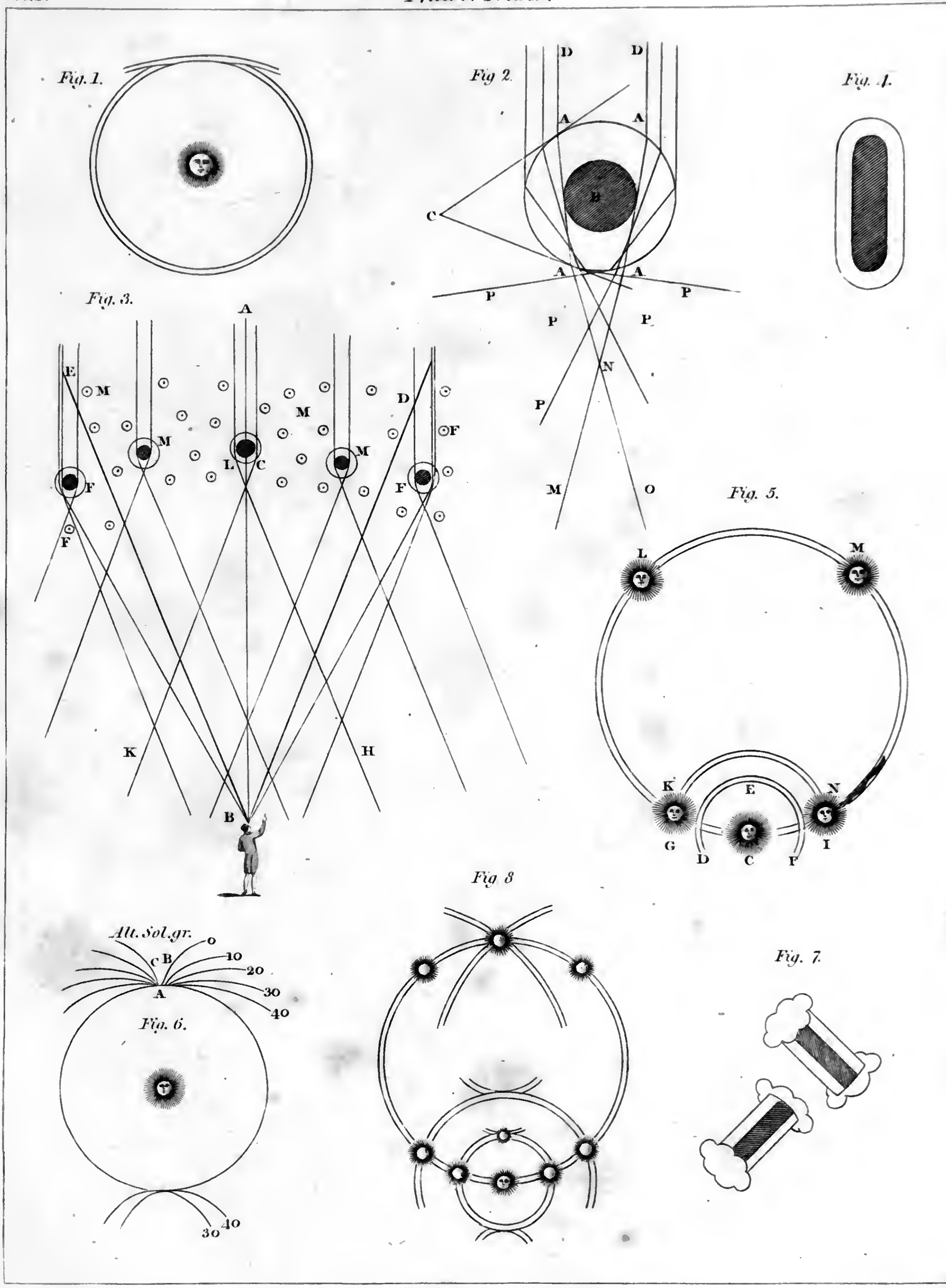

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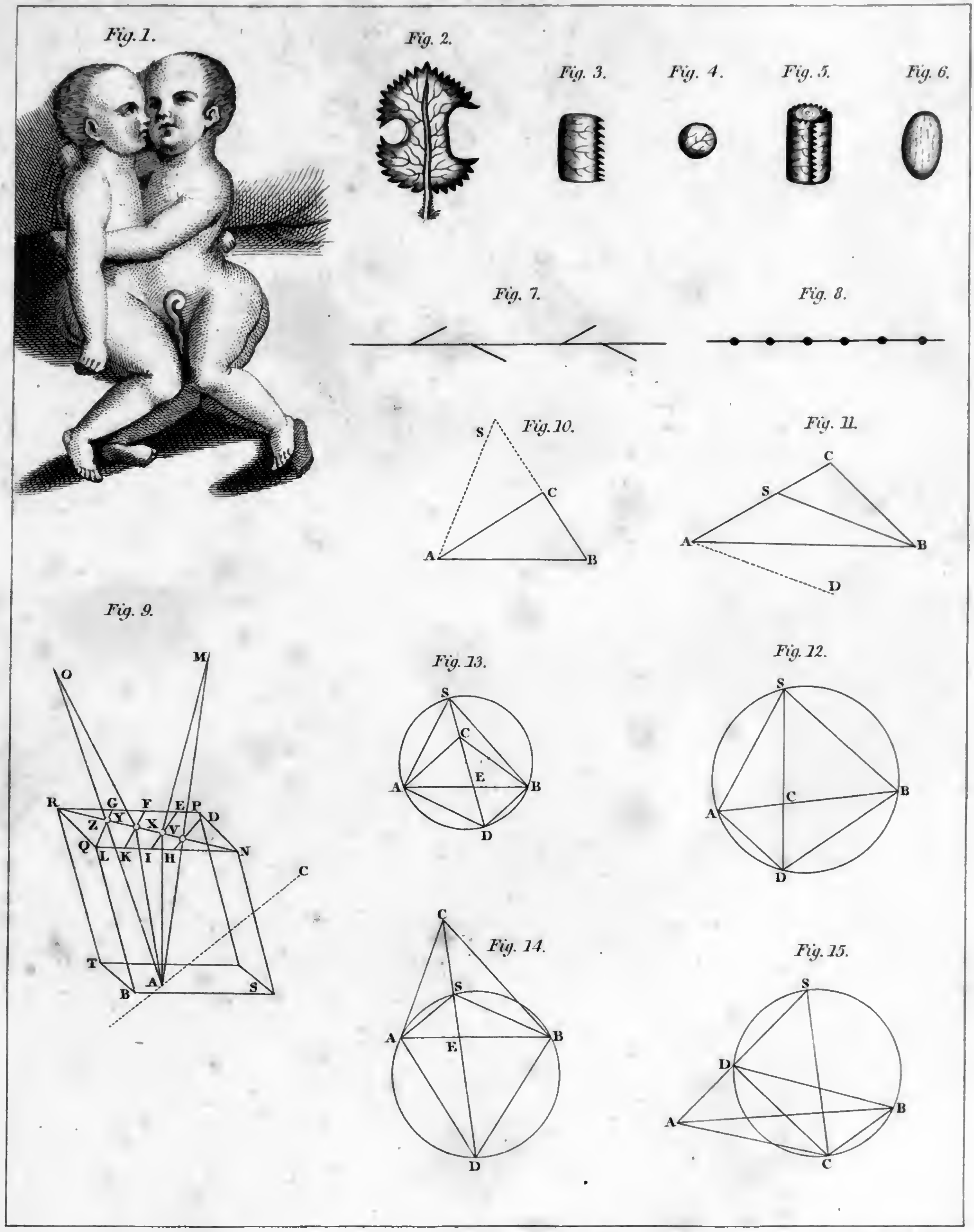

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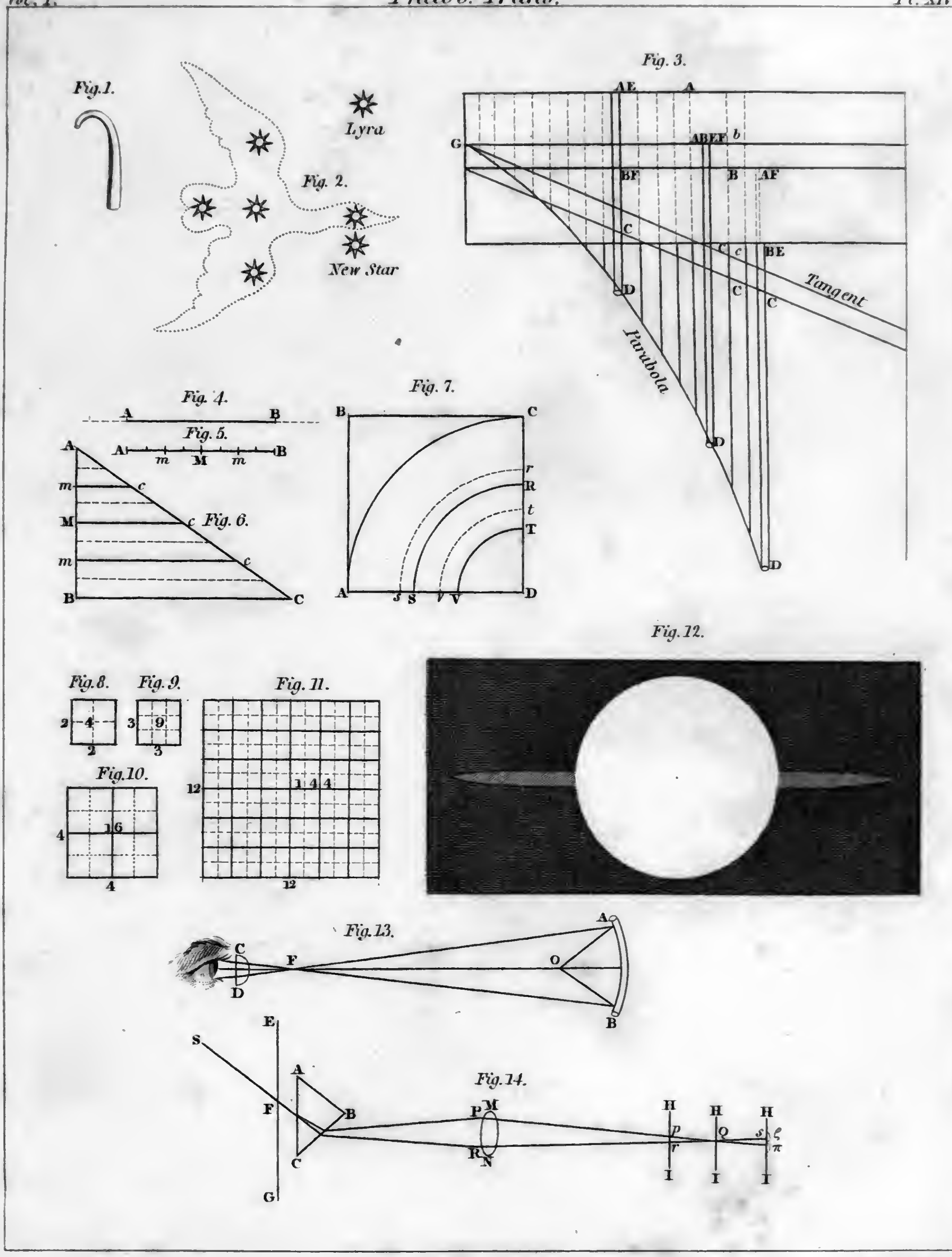

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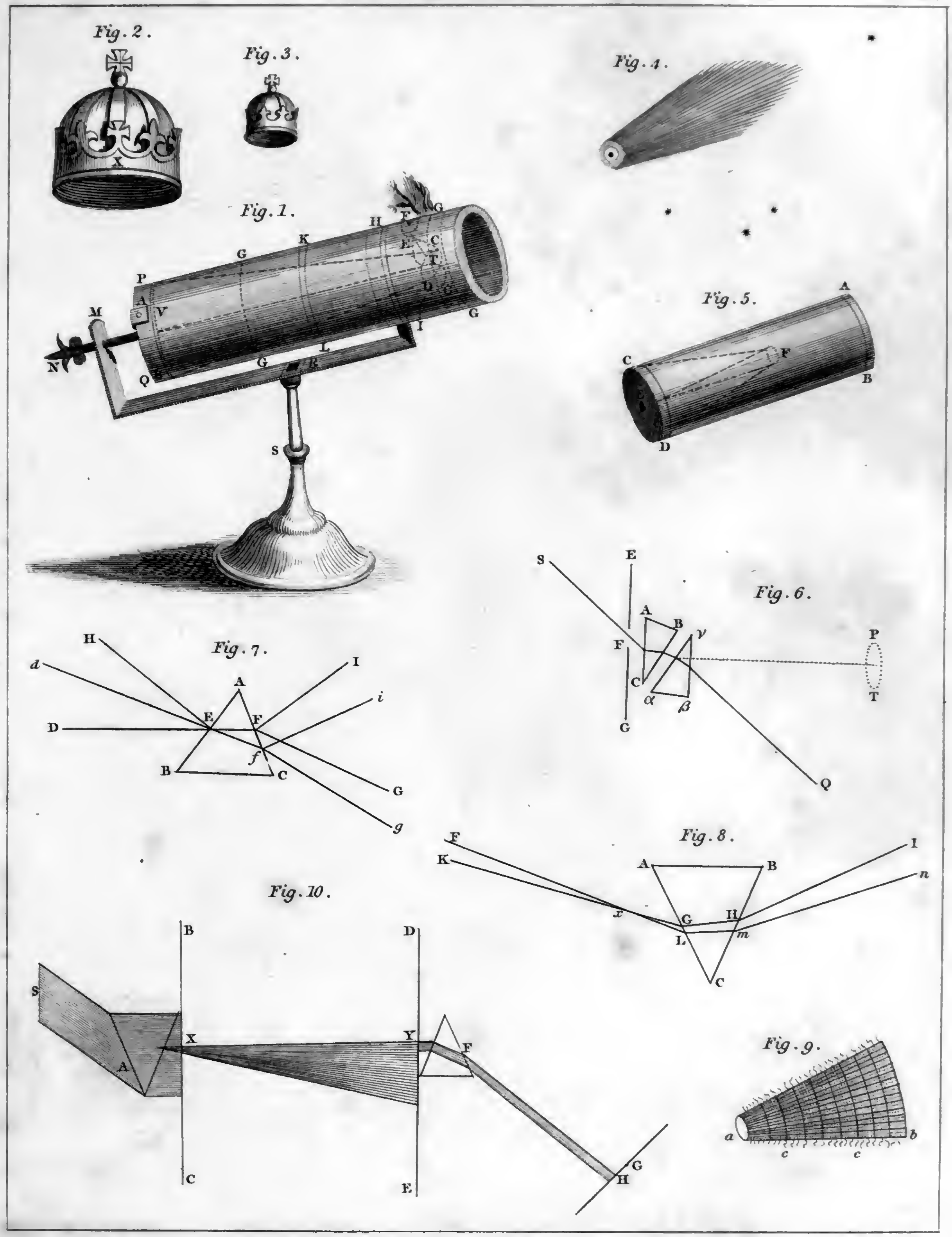

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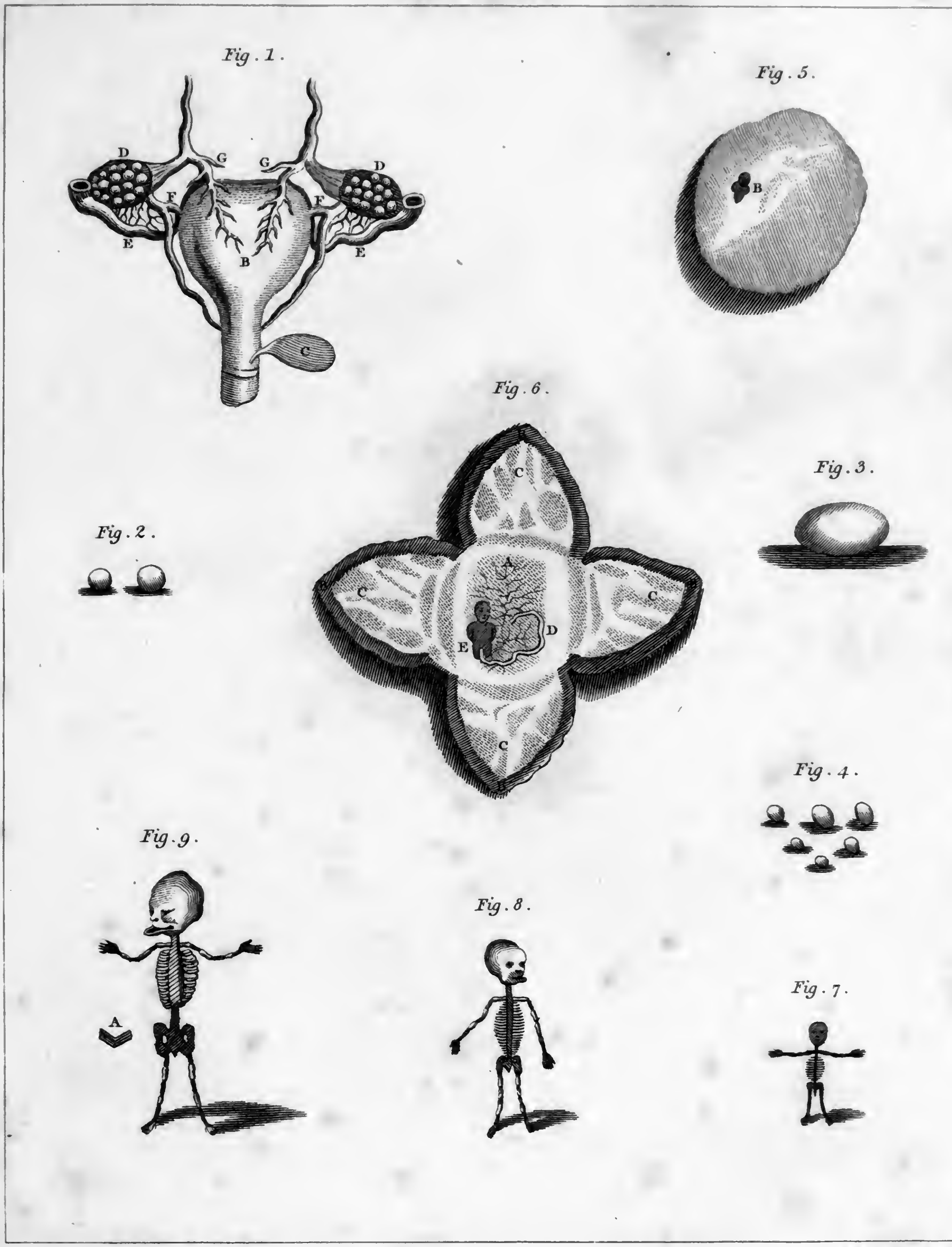

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